

THE AMERICAN ECONOMIC REVIEW

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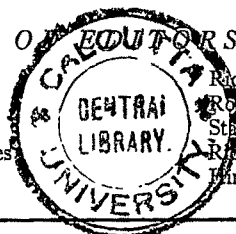
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For the people

The American Economic Review

Volume LVII

MARCH 1967

Number 1

THEORIES OF THE FIRM: MARGINALIST, BEHAVIORAL, MANAGERIAL*

By FRITZ MACHLUP

Last year, when it was my task to plan the program for the annual meeting of our association, a friend suggested that, with twenty years having passed since the outbreak of the "marginalism controversy," it was appropriate to review what has since happened to the embattled theory of the firm. The topic did not fit the general theme I had chosen for the 1965 meeting, but I reasoned that 1966 would give me a good opportunity to undertake the review myself.

The Battlefield Revisited

So let us recall that literary feud and the warriors, and let us revisit the battlefield. The major battlefield was the *American Economic Review*, with six articles and communications between March 1946 and March 1947 [16] [43] [21] [17] [22] [44]. There had been earlier gunfire elsewhere, chiefly in the *Oxford Economic Papers* in 1939 [14]. But, since the shooting then was not returned and it takes at least two opponents to join battle, it must be agreed that the real hostilities were the exchanges in the *AER*.

The fight was spirited, even fierce. Thousands of students of economics, voluntary or involuntary readers, have been either shocked or entertained by the violence of some of the blows exchanged and may have thought that the opponents must have become mortal enemies forever. These readers would have been wrong. Even before we came out for the last round of the fight, we exchanged friendly letters (December 1946) assuring each other that we would bear no grudges.

We have remained the best of friends; for several years now Richard Lester and I have been colleagues in the same department; and, as a token of our friendship, he has generously accepted my invitation to share this platform with me today as chairman of the session. Thus the veterans of both sides of the War of 1946 are now joined in revisiting

* Presidential address delivered, in a shorter version, at the Seventy-ninth Annual Meeting of the American Economic Association, San Francisco, December 28, 1966.

the battlefield. This, incidentally, does not mean that either of us has succeeded in converting the other to the "true faith."

What was the outcome of the controversy? Who won? We could not possibly say if we have not first agreed on precisely what the shooting was about. I have heard it said that Machlup won the battle but Lester won the war. What this means, however, cannot be known unless we know what the issues and objectives of the war had been. Was it merely to make economics safe for or from marginalism? Were there not several other issues being fought over?

Some of the Major Issues

There were no doubt a good many contentions of all sorts—major, minor, essential, incidental, interpretative, factual, methodological, substantive, and all the rest. To present a complete catalogue of the issues involved would be too ambitious a task for this occasion, but a partial listing might be helpful.

The chief issue, of course, was whether marginal analysis was invalid and ought to be discarded, especially as far as the theory of prices, cost, wages, and employment in manufacturing industry is concerned. This issue, however, implied the question of the correct interpretation of marginal analysis, including the tenets of the marginal-productivity principle. In this connection, differences in the models of the firm customarily used in different kinds of analysis became relevant. Involved here was the question of whether the postulate of maximizing money profits led to conclusions very different from those derivable from assumptions of conduct guided by a variety of largely nonpecuniary considerations.

Underlying all these questions were some issues of general scientific methodology: the legitimacy and usefulness of abstract theorizing on the basis of unrealistic assumptions, or perhaps on the basis of assumptions regarded as "reasonable" thought not "universally true." These issues, in particular, were whether an assumption of profit maximization as the effective objective of the firm in the theoretical model may be accepted as a tenable hypothesis only if it can be verified that all or a majority of those who actually run business firms in the real world agree that this is their only or major objective, that they are capable of obtaining all the information and of performing all the calculations needed for the realization of that objective, and are really carrying out the actions found to be optimal in this fashion; or, alternatively, whether all these tests may be dispensed with and the assumption of profit maximization nevertheless accepted as a fruitful postulate from which conclusions can be derived which correspond with what can be observed in the records of prices and quantities.

Concerning the empirical testing of theoretical conclusions, there were issues of the validity of surveys through mailed questionnaires and of the proper interpretation of responses to various types of questions about managerial judgment. In the background of the whole controversy, but undoubtedly of pervasive significance, was the comparative acceptability of empirical findings to the effect that the elasticity of demand for labor was virtually zero and of the conventional theoretical inference that the elasticity was normally above zero.

Realizing how manifold were the issues of the controversy, one can appreciate that no clear decision can be made about its outcome. Some of the issues had been raised decades or centuries before 1946 and were not decided in this confrontation one way or the other. Attacks on the assumption of maximizing behavior and on the lack of realism in price theory have occurred with great regularity ever since "economic man" and similar postulates were introduced. The running battles between the classical and the historical schools were largely on these points. The *Methodenstreit* of 1883-84 dealt essentially with the same issues. And in the United States, institutionalism may be seen as a movement animated by the same spirit of protest against abstract theory.

However, the particular form of explicit marginalism (under the name of "theory of the firm") which became the target of the attacks of 1939 and 1946 had only come into being in the 1930's—if one suppresses the memory of the great master of 1838 [9]. Ironically, some interpreter of recent history of economic thought—I have forgotten who it was—regarded the 1933-34 versions of the theory of the firm [8] [32] [41] as the theorists' concession to institutionalism, as attempts to supplement the neoclassical model of the firm under atomistic competition with some "more realistic" models allowing for a greater variety of conditions. It was this theory of the profit-maximizing firm in all sorts of market positions, in monopolistic and oligopolistic competition as well as in pure and perfect competition, that was attacked by the researchers in Oxford; and it was the marginal-productivity principle in the explanation of the demand for labor on the part of the individual firm that was the prime target of the attack of 1946.

If the chief aim of the attack was to force the abandonment or subversion of marginalism, and if the chief aim of the defense was to turn back the subversive forces and secure the reign of marginalism once and for all, then, to be sure, the war of 1946 ended in a draw. Look at the textbooks and you will find that marginalism has continued to dominate the teaching of microeconomics, perhaps though with occasional reservations and references to current attempts at greater real-

ism. But look at the journals and monographs and you find that research on alternative approaches to the theory of the firm is regularly reported with the implication that a superior theory may eventually replace marginalism. This replacement, however, according to the proponents of the best-known alternatives to marginalism, is expected chiefly with regard to industries where firms are few and competition is ineffective. The marginalist solution of price determination under conditions of heavy competition is not seriously contested.

In pointing this out, I am not trying to claim that marginal analysis is invincible and forever irreplaceable. If I follow the philosophy of science which, instead of pronouncing theories "false" or "true," distinguishes only between those "rejected" and those "still open to criticism" [30, pp. 246-48], the only victory that can be claimed for the cause of marginalism is that it is still open to criticism. I must go beyond this and concede that some anti-marginalist suggestions have led in recent years to a number of revisions in the marginal analysis of the firm which amount to the incorporation of other goals besides money profits into expanded marginalist objective functions.

The Alternative Approaches

In their arguments against the profit-maximization model the various alternative approaches to the theory of the firm are very much alike; only their positive programs can distinguish them.

The program of behaviorism is to reject preconceptions and assumptions and to rely only on observation of overt behavior. Thus, behaviorism rejects the assumption of marginal analysis that economic action is directed by the objective to maximize the attainment of ends with given means, and that business action can be deduced from a postulate that firms attempt to maximize money profits. Instead, we are directed to *observe* how businessmen really act and by what processes they reach decisions.

Perhaps it is not entirely fair to suggest here an association between "behaviorism" and the working program of the proponents of a "behavioral theory of the firm" [10]. In any case, behavioral research proposes to observe and study the "real processes," in the sense of a "well-defined sequence of behaviors" by which decisions are reached in "actual business organizations." The hope—faithfully inductive—is to develop a theory "with generality beyond the specific firms studied" [10, p. 2]. Such a theory will be based on "four major sub-theories" regarding "organizational goals, organizational expectations, organizational choice, and organizational control" [10, p. 21]. It is assumed that five organizational goals—a production goal, an inventory goal, a sales goal, a market-share goal, and the profit goal—become the sub-

ject of bargaining among the various members of the "coalition" which make up the business organization but that the goals are continually adapted and are being pressed with varying force [10, pp. 40-43]. The behavior theory of the firm, with regard to the determination of prices and outputs, will run in terms of a "quasi resolution of conflict" within the organization, of an "adaptively rational, multiple-objective process" with responses to "short-run feedback on performance" and with continuing "organizational learning" [10, pp. 269-70].

This behavioral approach has been characterized as striving for "realism in process," in contrast to approaches aiming at more "realism in motivation" [48, p. 11]. Such realism in motivation is felt to be needed chiefly because of the separation of ownership and control in the modern corporation, whose managements have great power and wide discretion.

In principle, I could expect three different views to be taken regarding the relative independence of corporation management: (1) Whereas owners would run their business chiefly with a view to a maximum of money profits, managers run it with several supplementary and partly competing goals in mind. (2) Whereas owners, especially wealthy ones, would often allow nonprofit considerations to enter their decision-making, managers have a sense of dedication and identification with the business that makes them the more single-minded seekers of profits. (3) Even if managers are inclined to indulge in seeking other goals as long as profits look satisfactory, they are as professionals, trained in the art and science of management, able to make better profits than the owners could ever hope to make running their own show.

What consequences can be drawn from this? One attitude would be to stick with the assumption of profit maximization because it is the simplest and is applicable with much less detailed information to the largest field.¹ Another attitude would be to insist on starkest realism with a complete catalogue of goals and indices of their effectiveness in each firm. A third attitude would be to select two or three of the most

¹ "To use marginalism in the theory of the firm it is not necessary to assert that firms attempt to maximize money profits only nor to deny that a goodly portion of all business behavior may be nonrational, thoughtless, blindly repetitive, deliberately traditional, or motivated by extra-economic objectives. It merely presupposes that the 'rational-economic' portion of business conduct is by and large sufficiently important to affect what is going on in the world to an extent large enough to warrant analysis; and that the substitution of money profits for a composite of pecuniary and nonpecuniary rewards simplifies the analysis so much that the gain in expediency far exceeds the loss in applicability" [23 pp. 30-31]. A similar view is expressed by Scitovsky: "Empirical studies of businessmen's behavior suggest the need for modifying or qualifying the assumption of profit maximization here and there, rather than scrapping it altogether. Accordingly, . . . we shall retain the assumption that the firm aims at maximizing its profit. But we shall regard this assumption as a working hypothesis rather than as a universal rule" [37, p. 111].

important managerial objectives of a type that can be reduced to quantitative analysis and to combine them in a single manageable "objective function." This third approach merges marginalism with managerialism in that it integrates money profits with other managerial goals within one formula of "maximizing behavior."

The question is whether managerial marginalism is prescribed for general application or only for so-called noncompetitive cases. Its most prominent proponents prefer to use the old formula, based on profit maximization, in situations where competition is effective and managerial discretion therefore narrowly circumscribed. In the next sections we shall discuss matters that at first blush may seem unrelated to this issue but on reflection can shed indirect light on it.

The Analogy of the Theoretical Automobile Driver

One of the best remembered points in my exposition was the use of an analogy designed to warn against mistaking theoretical variables and their links for realistic descriptions of observable processes. This was the analogy of the "theory of overtaking" automobiles on the highways [21, pp. 534-35].

Analogies are often misleading, but in this particular case it served its main purpose: to show that the theoretical variables need not be estimated and the theoretical equations need not be solved through actual calculation by the actors in the real world whose idealized types are supposed to perform these difficult operations in the models constructed for the explanation of recorded observations.² The critics of marginal analysis believed they had refuted it if they could show that the exact numerical calculations of marginal magnitudes—cost, revenue, productivity—were difficult or impossible to perform by real decision-makers.

Yet, my analogy was only partially successful. An implication which should have been obvious has been widely overlooked: that the type of action assumed to be taken by the theoretical actor in the model under specified conditions need not be expected and cannot be predicted actually to be taken by any particular real actor. The empiricist's inclination is to verify the theoretically deduced action by testing individual behavior, although the theory serves only to explain and predict effects of mass behavior.

We may illustrate this again by means of the same analogy, the theory of overtaking. Assume a change of driving conditions occurs, say, that the roads have become wet and slippery and fog has reduced visi-

² The theoretical automobile driver had to estimate, among other things, the speeds of three vehicles and the distances between them, and to perform calculations involving potential acceleration and a few other things, before he could decide to overtake the truck ahead of him. An actual driver simply "sizes up" the situation and goes ahead.

bility. Theory enables us to predict that traffic will be slower and accidents more frequent, but it does not enable us to predict that any particular driver will drive more slowly or have an accident. The model of the reactions of the individual driver was not designed to explain the actual driving of any particular operator but only to explain the observable consequences of the observed change of conditions by deducing from the model the theoretical reactions of a hypothetical driver.

Our analogy can also show us the limitations of the model: the prediction will hold only if there is a large number of automobiles on the road. If only a very few cars are around, there may be no accident and there need not be a reduction in their speed. Conceivably, the operators may all be good and self-confident drivers. Marginal analysis of hypothetical driver reaction will suffice for explaining and predicting the consequences of a change in driving conditions if the number of automobiles on the highways is large. If the number is small, behavioral research will be needed, though it may or may not be worth the cost.

Still another use can be made of our analogy: to show the vast differences in the scope of questions to which answers can or cannot be expected with the aid of a given theory, for example, from the theory of overtaking as sketched in my article. Compare the following four questions: (1) How fast will traffic move? (2) How fast will the automobile driven by Mr. X move? (3) How will the speed of traffic be affected by fog? (4) How will the speed of Mr. X's driving be affected by fog?

The theory sketched by me offers no answer to the first question, because each of the variables specified may have very different values for different cars and drivers; it has no answer to the second question, and only a suggestion, a rebuttable presumption, for answering the fourth question, because the theory is not really concerned with particular persons or their actions and reactions. The theory is equipped only to answer the third question, regarding the effects of a change in driving conditions on automobile traffic in general, and even this answer will be qualitative only, without good clues to numerical results. It may be interesting to get answers to all four questions, but since Question 3 can be answered with a fraction of the information that would be needed to answer the other questions, it would be foolish to burden the models designed for Question 3 with irrelevant matters, or to reject such models because they cannot do what they are not designed to do.³

³ A behavioral theory of automobile driving would probably study the process by which the decision to pass a truck is arrived at in a sequence of bickering among the members of the family: Mama and Sis trying to argue against taking an unnecessary risk, Sonny egging on his Dad to speed up and pass the truck "crawling" ahead of them. Moreover, the theory would not be satisfied with "explaining" the decision to overtake but it would

Confusion of Purposes

The same sort of confusion about the scope of problems and models for their solution has been fostered in recent writings on the theory of the firm: models have been condemned or rejected because they could not be used for purposes for which they had not been designed, and significant differences in the questions to be answered have been obscured or underemphasized.

Let us again pose four typical questions and see which of them we might expect to answer with the aid of "price theory." (1) What will be the prices of cotton textiles? (2) What prices will the X Corporation charge? (3) How will the prices of cotton textiles be affected by an increase in wage rates? (4) How will the X Corporation change its prices when wage rates are increased?

Conventional price theory is not equipped to answer any but the third question; it may perhaps also suggest a rebuttable answer to the fourth question. But Questions 1 and 2 are out of reach. We could not obtain all the information that would be required for their answers and there is, therefore, no use burdening the models with variables remaining silent and inactive throughout the show.

We ought to guard against an easy misunderstanding of our denial that conventional price theory can predict actual prices of specified goods. Prediction of future prices of a particular commodity may in fact be quite manageable if we know its present price. It should be obvious, however, that this is Question 3, not Question 1. Or, one may be able to predict prices on the basis of good information on production cost. But this presupposes that we know the demand for the commodity and assume it will remain unchanged; which again comes down essentially to evaluations of changes of some variables with others held constant, that is, to Question 3.

If the number of firms producing cotton textiles is large and the X Corporation does not supply a very large part of the aggregate output of the industry, price theory may suggest an answer to Question 4, although this is not the purpose of the theory and there may be a considerable chance for the suggested answer to be wrong. The point is that a model of a theoretical firm in an industry consisting of a large number of firms can do with a much smaller number of assumptions, provided the model is used to predict, not the actual reactions of any one particular firm, but only the effects of the hypothetical reactions of numerous anonymous "reactors" (symbolic firms). If it were to be applied to ~~predictions of reactions of a particular firm~~, the model would have to

also wish to determine the speed of driving, the frequency and length of stops at roadside stands, and all the rest.

be much more richly endowed with variables and functions for which information could be obtained only at considerable effort and with results that may or may not be worth the cost of the required research.

My charge that there is widespread confusion regarding the purposes of the "theory of the firm" as used in traditional price theory refers to this: The model of the firm in that theory is not, as so many writers believe, designed to serve to explain and predict the behavior of real firms; instead, it is designed to explain and predict changes in observed prices (quoted, paid, received) as effects of particular changes in conditions (wage rates, interest rates, import duties, excise taxes, technology, etc.). In this causal connection the firm is only a theoretical link, a mental construct helping to explain how one gets from the cause to the effect.⁴ This is altogether different from explaining the behavior of a firm. As the philosopher of science warns, we ought not to confuse the *explanans* with the *explanandum*.

Misplaced Concreteness

To confuse the firm as a theoretical construct with the firm as an empirical concept, that is, to confuse a heuristic fiction with a real organization like General Motors or Atlantic & Pacific, is to commit the "fallacy of misplaced concreteness." This fallacy consists in using theoretic symbols as though they had a direct, observable, concrete meaning.

In some fields, investigators are protected from committing the fallacy, at least with regard to some of their problems, by the fact that a search for any empirical counterpart to the theoretical construct seems hopeless. Thus, some physicists working on particle theory were able

⁴ The same statement can be made about the household. The "household" in price theory is not an object of study; it serves only as a theoretical link between changes in prices and changes in labor services supplied and in consumer goods demanded. The hypothetical reactions of an imaginary decision-maker on the basis of assumed, internally consistent preference functions serve as the simplest and heuristically satisfactory explanation of empirical relationships between changes in prices and changes in quantities. In other words, the household in price theory is not an object of study.

Behavioral studies of real households are something entirely different. A realistic, behavioral theory of the household might conceivably distinguish the large, children-dominated household from a simpler, father-dominated one. The decisions in the children-dominated household, where mother frequently and father occasionally try to exercise some influence, are probably not consistent, since different preference systems are made explicit at various times, with varying decibels and gestures deployed to make them prevail over the preferences of other members of the family.

One can imagine studies on the behavior of particular households selected at random or in structured samples. If the researcher learns that a spoiled brat in a family wants to eat nothing but beef and throws a tantrum every time his mother tries to feed him other kinds of meat, a reduction in the price of chicken will probably not substantially increase the consumption of chicken in this family. Thus, the weight of the child's taste in the decision process of the family can explain a low elasticity of its demand for chicken. But none of this has much bearing on general price theory.

to answer the question "Does the Neutrino Really Exist?" [11, pp. 139-41] laconically with "Who cares?" and to explain that any belief in the "real existence" of atoms, electrons, neutrinos, and all the rest, would hold up the progress of our knowledge. Some biologists working in genetics warned, after empirical genes were discovered, that these "operational genes" should not be confused with the "hypothetical genes," which had been useful constructs in explanatory models before the discovery of any empirical referents [42, p. 814]. Economists, however, know for sure that firms exist as empirical entities and, hence, they have a hard time keeping the theoretical firm and the empirical firm apart.

For certain economic problems the existence of the firm is of the essence. For example, if we study the size distribution of firms or the growth of the firm, the organization and some of its properties and processes are the very objects of the investigation. In such studies we insist on a high degree of correspondence between the model (the thought-object) and the observed object. For other problems, however, as for problems of competitive-price theory, any likeness between the theoretical construct of the firm and the empirical firm is purely coincidental.

Economists trained in scientific methodology understand this clearly. I might quote a dozen or more writers, but will confine myself to one quotation, which states that "in economic analysis, the business firm is a postulate in a web of logical connections" [15, p. 196]. Let me add the statement of another writer, who however was plaintiff rather than advocate when he wrote that "It is a fascinating paradox that the received theory of the firm, by and large, assumes that the firm does not exist" [45, p. 249].

Here is what I wrote on one of the several occasions when I have discussed this problem:

... the firm in the model world of economic micro-theory ought not to call forth any irrelevant associations with firms in the real world. We know, of course, that there are firms in reality and that they have boards of directors and senior and junior executives, who do, with reference to hundreds of different products, a great many things—which are entirely irrelevant for the microtheoretical model. The fictitious firm of the model is a "uni-brain," an individual decision-unit that has nothing to do but adjust the output and the prices of one or two imaginary products to very simple imagined changes in data [26, p. 133].

I went on, of course, to say that this purely fictitious single-minded firm, helpful as it is in competitive-price theory, will not do so much for us in the theory of monopoly and oligopoly. To explain and predict price reactions under monopoly and oligopoly we need more than the

construct of a profit-maximizing reactor.⁵ I shall come back to this after discussing the demands for "more realistic" assumptions where they are plainly irrelevant and therefore out of place.

Realistic Models of the Firm under Competition

Many of the proponents and protagonists of a more realistic theory of the firm are quite aware of the fact that the managerial extension and enrichment of the concept of the firm was not needed except where firms in the industry were large and few, and not under the pressure of competition. There are many very quotable statements to this effect.⁶

Too many students, however, want a realistic model of the firm for all purposes. They forget the maxim of Occam's Razor that unnecessary terms in a theory be kept out (or shaved off). These students seem to miss in a simplified model the realistic trimmings of the observable world; they distrust such a model because it is obviously

⁵ You may wonder whether I have changed my mind on these matters. Incidentally, I hold that it is important for scholars and scientists to have an open mind, and the only evidence showing that they do are instances in which they have actually changed their minds. On this particular issue, however, I cannot oblige. Whether I am right or wrong, I have been consistent regarding these points. Let me quote from an article I wrote 28 years ago: "The problem of oligopoly is by definition the problem of the effects of the actions of few, giving a greater importance to the behavior of each member of the group. . . . The theory of the oligopoly price involves an interpretation of the significant motives behind the actions of a small number of people. . . . Even the most superficial theory will have to include many more ideal types of behavior in order to handle the problem of *few* sellers than it takes to handle the problem of a *mass* of competitive sellers" [20, p. 235].

On the other hand, I must plead guilty to a charge of the same error of misplaced concreteness against which I have just warned. It occurred in a sentence in which I spoke of various magnitudes (subjectively) "perceived or fancied by the men whose decisions or actions are to be explained (the business men) . . ." [21, p. 521]. If this sentence referred only to oligopolistic or monopolistic behavior, it would not be so bad, for, as I said above, the theoretical constructs of decision-makers in this case have a closer correspondence to real businessmen than the constructs in the theory of competitive prices. But the sentence was supposed to apply to the constructs of the firm in any position whatever. Hence it was a misleading sentence in that (1) it gave the impression that the decision-makers in question were *real* men (real businessmen, whom you could interview) and (2) it said that the actions of these men were to be explained, whereas the purpose of the theory was not to explain observed actions but only observable *results* of imagined (postulated) reactions to observable events.

I apologize for this error. Not that I do not approve of a busy shuttle-traffic between the domain of theoretical construction and the domain of empirical observation, but we must never fail to specify the side of the frontier on which we happen to be. The theoretical terms may have empirical referents (counterparts), but to believe, or allow an impression of belief, that the two are identical is a methodological fallacy.

⁶ "When the conditions of competition are relaxed . . . the opportunity set of the firm is expanded. In this case, the behavior of the firm as a distinct operating unit is of separate interest. Both for purposes of interpreting particular behavior within the firm as well as for predicting responses of the industry aggregate, it may be necessary to identify the factors that influence the firm's choices within this expanded opportunity set and embed these in a formal model" [48, pp. 2-3].

"descriptively false." In view of this sentimental hankering for realism, it may be helpful to survey some of the inclusions which various writers have proposed in order to meet the demands for greater realism in the "theory of the firm," and to examine their relevance to the theory of competitive price. The following considerations are supposed to supplement, qualify, restrict, or replace the objective of maximizing money profits.

(1) Entrepreneurs and managers cannot be expected to have an inelastic demand for leisure; indeed, one must assume that this demand is income-elastic so that higher profit expectations will cause them to sacrifice some income for the sake of more leisure [36, p. 356]. (2) Managers are anxious to avoid resentment on the part of their colleagues and subordinates and will, therefore, not enforce their orders with the sternness required for maximization of profits; similarly, minor functionaries do not want to disturb the routines of their superiors and, hence, they often abstain from suggesting improvements which would maximize profits [31, p. 452]. (3) Managers are more interested in their own salaries, bonuses, and other emoluments, than in the profits of the firm or the income of its owners [27, pp. 226-27]. (4) The realization of certain asset preferences (for example, liquidity as against inventories and fixed assets) may be in conflict with profit maximization [5, p. 99]. (5) The flow and biased screening of information through the various levels of management may cause systematic misinformation resulting in earnings far below the maximum obtainable [27, p. 229]. (6) The objective of maintaining control in the hands of the present control group may require a sacrifice of profit opportunities [31, p. 455]. (7) The preference for security may be so strong that even relatively conservative ways of making higher profits are eschewed [12, pp. 270-71]. (8) The striving for status, power, and prestige may be such that it results in conduct not consistent with a maximum of profit [1, p. 145] [28, p. 207] [13, p. xii] [27, p. 227]. (9) The wish to serve society, be a benefactor, or soothe one's social conscience, may militate against actions or policies that would maximize profits [7, pp. 16-17] [13, pp. 339-40]. (10) The instinct of workmanship [46, p. 187], a desire to show professional excellence [1, p. 146], a pervasive interest in feats of engineering, may lead to performance in conflict with highest possible profits. (11) Compromises among the different goals of executives with different interests—production, sales, personnel relations, finance, research and development, public relations, etc.—are sure to "compromise" the objective of maximum profits [10, p. 29]. (12) A variety of influences may be exerted on management decisions, perhaps pulling in different directions and possibly away from maximum profits, as for example influences from labor organizations, suppliers of materials,

customers, bankers, government agencies [13, p. 340] [12, p. 270] [28, pp. 195-205].

I shall not prolong this catalogue even if it is far from complete. Let us admit that each of the possible deviations from maximum profit may be "real" in some circumstances. But how effective and significant are they? If the industry is effectively competitive—and it does not have to be "purely" competitive or "perfectly" competitive—is there much of a chance that the direction in which firms react, through their decisions regarding prices, inputs and output, to a change in conditions would be turned around by any of the "forces" listed? Before we say apodictically no, we should examine a few of the reservations.

Security and Managerial Coordination

Let us single out two items which have been given especially wide play: the "objective of security" and the question of "managerial coordination."

The demand for the recognition of a separate "security motive" conflicting with the profit motive deserves a good discussion. But when I prepared for it, I reread what I had written on this subject and found that I could not improve on it. Will you do me the favor of reading it [23, pp. 51-53 and 424-28] and, if you like it, make your students read it?

That there are no business profits without risks and that there is not much point in treating the two quite separately; that it would be silly to call a decision one of profit-maximizing if it increased risk and uncertainty so much as to reduce the chance of survival; that the notion of long-run profits comprises all considerations of risks of loss; that, in terms of my automobile-driving analogies, only a fool would assume that maximization of speed means driving 120 miles an hour regardless of curves and bumps; these are some of the things that have to be said in this connection. But the most essential point to be made is that in the economics of *adjustment to change* the issues of security, survival, and maximum profit are merged. How primitive again to confuse new ventures and daring moves with mere responses to stimuli, obvious reactions to change. If a change in conditions calls for a certain reaction in the name of maximum profits, the very same reaction is called for also in the name of security of survival.

The other matter is of a more "behavioral" nature: the coordination of different goals and judgments on the part of different members of the management and the deviations from profit maximization that may be involved in the process. Frankly, I cannot quite see what great difference organizational matters are supposed to make in the firm's price reactions to changes in conditions. Assume, for example, the import duties on foreign products competing with the products of domes-

tic industry are raised, with a resulting increase in the demand for the products of the firm. Why should the clashes and compromises of divergent opinions reverse the direction of the change that would be "dictated" by the simple rule of profit maximization? Perhaps one vice president wants to raise prices without increasing output, while another wants to increase output without (at least at the moment) raising prices. No matter what their compromise will be, it is likely to conform with what the simple rule suggests. But if not, so what? Remember we are talking about industries with more than a few firms and with free entry.⁷

Other Qualifications to Competitive Price Theory

Substitution between income and leisure looks like the strongest reason for a qualification in cases in which the change in conditions is such that not only the locus of maximum profits is shifted but also the amount of profit obtainable is changed. Take again the example of a tariff increase shutting out foreign competition. The firms in the industry will find that given outputs will now fetch higher prices and that increased outputs can be sold at prices higher than those prevailing before tariffs were raised. And profits will be higher in any case, so that managers—even owner-managers—will be inclined to relax their efforts. Yet would anybody seriously argue that the substitution of leisure (coffee breaks, cocktail parties, golf) for potential profits would be such that total output would be reduced instead of increased? It is not a likely story, and where the industry consists of several or many firms, the small probability vanishes quickly. What remains of the argument is that total output would increase, in reaction to the tariff increase, somewhat less than it would if the managers were eager beavers and did not relax in their efforts when profits increased. Thus, the elasticity of supply of the products in question is a little smaller. But since we do not know how much it would be anyhow, the unknown subtraction from an unknown number should not cause the economic theorist any serious anxieties. (And if the politicians who push for the tariff increase decide to push less hard if we tell them that their friends in the industry will enjoy some of the added protection in the form of more leisure and recreation, we would not really mind.)

Even if formal accuracy demanded that we accept the maximization of the decision-maker's total utility as the basic assumption, simplicity and fruitfulness speak for sticking with the postulate of maximization of money profits for situations in which competition is effective. The question is not whether the firms of the real world will *really* max-

⁷ A great champion of more realistic theories of the firm summed up his reflections on their implications for general economics with this statement: "We shall not be far wrong in concluding . . . that the impact of more realistic theories of the firm on static price analysis is likely to be small" [6, p. 42].

imize money profits, or whether they even *strive* to maximize their money profits, but rather whether the *assumption* that this is the objective of the theoretical firms in the artificial world of our construction will lead to conclusions—"inferred outcomes"—very different from those derived from admittedly more realistic assumptions.

The second qualification in my list—regarding bosses, colleagues and subordinates—is quite irrelevant, except perhaps for questions of welfare economics, where it matters whether firms "really" do all they can to maximize efficiency. For theories concerned with *changes* in prices, inputs, and outputs in response to *changes* to conditions (of production, resource availability, and product demand) the strictness with which efficiency is watched in the firm does not matter. The effects of the tariff increase in our illustration, or the effects of changes in wage rates, interest rates, tax rates, and so forth, are if there is effective competition, essentially independent of the relations among the various levels in the managerial hierarchy of the firm.

It would take too much time here to go through our entire list of reservations. Anybody who makes the effort will find that some of the "realistic assumptions" proposed for inclusion in the theory can affect (by an unknown amount) the magnitude but not the direction of any change that is likely to result from a specified change in conditions; and that other assumptions will not even do that much. In short, they are all irrelevant for purposes of competitive price theory.

Oligopoly, Monopoly, and Managerial Discretion

I repeat: In the theory of competitive price the "real existence" of firms is irrelevant; imaginary (postulated) agents pursuing a simple (postulated) goal react to assumed changes in conditions and thereby produce (or allow us to infer) changes in prices, inputs, and outputs [24, pp. 13-14]. The correspondence between these inferences (deduced changes) and actual observations (observed changes in prices, inputs, and outputs, following observed changes in conditions) is close for two reasons: (1) The number of firms in the real world is so large that it suffices if some of them react as posited by the theory; and (2) the profits of firms are only about "normal," that is, excess profits are about zero, because of competitive pressures from newcomers (oligopolistic pressures [23, pp. 211-23]), so that profits below the maximum obtainable would in fact be net losses in an economic sense.

These two reasons do not hold in the theories of oligopoly and monopoly price.⁸ For these theories the real existence of firms (that is, an

⁸ The idea that profit maximization is the appropriate hypothesis for the theory of competitive price but not necessarily for the theory of monopoly or oligopoly price has been expressed repeatedly over the last century.

Pareto, for example, said that "pure economics" cannot tell us anything about the con-

empirical counterpart to the theoretical construct) is required, because the explanation of changes in prices, inputs, and outputs is at the same time an explanation of decisions of some particular firms, in the sense of organizations of men acting in particular, sometimes unpredictable, ways. Various attempts have been made to develop patterns of oligopolistic and monopolistic conduct and to correlate these patterns with types of organization or with types of personalities exercising ultimate decision-making power. The success has thus far been small; even if the decision-making (say, pricing) in a particular firm was sometimes satisfactorily modeled (for example, in a simulated computer program), the model has usually not been transferable to other cases, to predict decisions in other firms. I do not recall, moreover, that the behavior patterns in these cases were shown to be inconsistent with the postulate of profit maximization.

Under these circumstances, retreat to simpler, less realistic models of firms in oligopoly and monopoly positions is indicated. The first approach is to apply the polypolistic model, in full awareness that the actual facts are entirely different. In many instances the use of the polypolistic model for situations which in our judgment would merit to be labeled as oligopolistic will still yield satisfactory explanations and predictions. Where this is not so, the analyst will resort to the use of models of oligopolistic or monopolistic firms, postulating the simplest possible pattern of action and reaction, dispensing with all peculiar attitudes and "special" strategies. Only where these simple models of oligopolistic and monopolistic firms yield quite unsatisfactory predictions will the analyst need to go further, to more special types of behavior, provided he finds it worth while. It depends on the research interests and on the problems under examination how much effort one wishes to invest in behavioral research where the findings hold little promise of yielding generalizations of wide applicability.

There are, however, some simple models of oligopolistic behavior

tinuing shifts of position of competing oligopolists, and we have to turn to "the observation of facts," which would show us the variety of possibilities [29, pp. 601-2].

Schumpeter, in 1928, had this to say about the dichotomy: "We have much less reason to expect that monopolists will . . . charge an equilibrium price than we have in the case of perfect competition; for competing producers *must* charge it as a rule under penalty of economic death, whilst monopolists, although having a *motive* to charge the monopolistic equilibrium price, are not forced to do so, but may be prevented from doing so by other motives" [33, p. 371].

Finally, Scitovsky in 1951 stated that "not only does the monopolist's secure market position enable him to relax his efforts of maximizing profit, but his very position may prevent his aiming at maximum profit. He may regard his immunity from competition as precarious or be afraid of unfavorable publicity and public censure; and for either reason, he may judge it wiser to refrain from making full use of his monopoly position. We conclude, therefore, that although in some cases the monopolist will aim at maximizing his profit . . . in other cases—which may well be the important ones—he will refrain from maximizing profit" [37, p. 377].

which seem to be of sufficiently wide applicability. A model that equips the oligopolistic decision-maker not under heavy competitive pressure with an objective of gross-revenue ("sales") maximization, subject to the constraint of satisfactory net-revenue ("profit") [2, p. 49], succeeds in explaining the lack of response to some cost-increasing events observed in several instances. There are other simple models explaining the same phenomenon, and one may think of good reasons for finding one model or another more satisfactory. If the sales-maximization hypothesis can explain a greater variety of observed responses or nonresponses than other hypotheses can, and if it seems to correspond better with self-interpretations offered by interviewed businessmen, it merits acceptance, at least for the time being.

An alternative to the maximization of sales is the maximization of the growth rate of sales [3, p. 1086]. This hypothesis is especially interesting because it involves an endogenous relation with profits: while some of the growth of gross revenue may encroach on profits, it does so with an automatic limit in that profits are needed to finance the investment required for the growth of sales.

Another extension of the objective function proposed on the basis of behavioral research combines two managerial preferences for specific expenses of the firm with the usual profit motive. The two additional motives are expenditures for staff personnel and expenditures for managerial emoluments; both figure prominently in the utility functions of executives of companies which, sheltered from competitive pressures, make enough profits to allow management to indulge in these personal desires [48, pp. 38-60].

All these "managerial-discretion models" are simple and sufficiently general to allow relatively wide application. We shall have more to say about them later.

Effective Competition and Managerial Discretion

In mapping out the area of applicability for theories of managerial discretion, we have spoken of "oligopoly," "monopoly," and of "firms not under heavy competitive pressure." These are rather vague guideposts, but unfortunately the literature has not been very helpful in ascertaining precisely what it is that allows or restricts the exercise of wide managerial discretion.

Some writers stress the size of the firm, suggesting that it is only in the *large* firm that management can exercise discretion. Others stress the condition of *diffused ownership* as the one that affords management the opportunity of pursuing objectives other than maximization of profits. Those who stress oligopoly as the domain for which objective functions richer than profit maximization are needed are usually not quite specific as to their criterion of an oligopoly position: it may be

fewness of firms active in the same industry, or the subjective state of awareness of the *interdependence of price making* often characterized as "conjectural variation," or simply the *absence of aggressive competition for increasing shares in the market*. Others again stress *closed entry*, or absence of newcomers' competition, as the essential condition for a profit level sufficiently comfortable to allow managers to indulge in the satisfaction of objectives other than maximization of profits.

To combine all these conditions would probably be far too restrictive; it would confine the application of managerial-discretion models to large firms with diffused ownership, few competitors, full awareness of interdependence in pricing, absence of aggressive efforts by existing competitors to increase their market shares, and little danger of new competitors entering the field. The size of the firm may actually not be relevant, and diffused ownership may not be a necessary condition for some deviations from profit maximization to occur, say, in the interest of larger sales or larger expenditures for staff. Fewness of competitors may be more significant, chiefly because the danger of newcomers' competition is likely to be small where the number of firms has been few and continues to be few; partly also because the few competitors may have learnt that aggressive price competition does not pay. The essential conditions, it seems to me, are these two: that no newcomers are likely to invade the field of the existing firms, and that none of the existing firms tries to expand its sales at such a fast rate that it could succeed only by encroaching on the business of its competitors.

Competition from newcomers, from aggressive expansionists, or from importers is sometimes called "heavy," "vigorous," or "effective." The simplest meaning of these adjectival modifiers is this: a firm is exposed to heavy, vigorous, or effective competition if it is kept under continuing pressure to do something about its sales and its profits position. Under this "competitive pressure" the firm is constantly compelled to react to actual or potential losses in sales and/or reductions in profits, so much so that the firm will not be able to pursue any objectives other than the maximization of profits—for the simple reason that anything less than the highest obtainable profits would be below the rate of return regarded as normal at the time.

I am aware of a defect in this definition: its criterion is lodged in the effect rather than in an independently ascertainable condition. Perhaps, though, "effective" is quite properly defined in this fashion, namely, by whether certain effects are realized: competition is effective if it continually depresses profits to the level regarded as the minimum tolerable. What makes it effective is not part of the definition, but has to be explained by the conditions of entry, aggressive attitudes on the part of existing firms, or imports from abroad.

If my reasoning is accepted, several formulations proposed in the

literature will have to be amended. Managerial discretion will be a function, not of the independence of the management from the control of the owners, but chiefly of the independence of the management from urgent worries about the sufficiency of earnings. If one insists, one may still say that all managers are primarily interested in their own incomes. But, since it is clear that their long-term incomes are jeopardized if profits go below the acceptable rate of return, maximization of managerial incomes and maximization of profits come to the same thing if competition is effective.⁹

There can be no doubt about the fact that competition is not effective in many industries and that many, very many, firms are not exposed to vigorous competition. It follows that managerial discretion can have its way in a large enough number of firms to secure wide applicability of well-designed managerial-discretion models—or to invite the use of managerial total-utility models.

I was fully aware, when I wrote my 1946 article, that there were many qualifications and exceptions to the principle of profit maximization.¹⁰ But I considered it hopeless for predictive purposes to work with total-utility maximization and I did not see the possibility of combining a few selected managerial goals with the profit motive.

Marginalism Extended: Total Utility

In order to show how hopeless it is to construct a comprehensive total-utility model and obtain from it definite predictions of the effects of changes in conditions upon the dispositions of the managers, one merely has to visualize the large variety of possible "satisfactions" and the still larger variety of things that may contribute to their attain-

⁹ For competition to be effective it is not necessary that competition is either pure or perfect or that all or any of the markets in which the firm buys or sells are perfect.

¹⁰ Several of my statements, if I presented them without source reference, might well be mistaken for quotations from critics of marginalism, including behavioralists and managerialists. Here are samples [21]: ". . . a business man is motivated by considerations other than the maximization of money profits"; "it is preferable to separate the non-pecuniary factors of business conduct from those which are regular items in the formation of money profits" (p. 526); "one may presume that producing larger production volumes [or] paying higher wage rates . . . than would be compatible with a maximum of money profits may involve for the business man a gain in social prestige or a certain measure of inner satisfaction"; "it is not impossible that considerations of this sort substantially weaken the forces believed to be at work on the basis of a strictly pecuniary marginal calculus"; for patriotic reasons during the war "many firms produced far beyond the point of highest money profits"; "the conflict of interests between the hired managers and the owners of the business" may call for "important qualifications" (p. 527); "the interest of the former in inordinately large outlays or investments may be capable of descriptions in terms of a pecuniary calculus, but it is not maximization of the firm's profits which serves here as the standard of conduct" (pp. 527-28); "maximization of salaries and bonuses of professional managers may constitute a standard of business conduct different from that implied in the customary marginal analysis of the firm"; and "the extent to which the two standards would result in sharply different action under otherwise similar conditions is another open question in need of investigation" (p. 528).

ment. The satisfactions consist not only in receiving money incomes, immediate or deferred, and various incomes in kind, but also in distributing incomes to others and in gaining prestige, power, self-esteem, as well as in enjoying a good conscience and other pleasurable feelings.

What makes things really complicated is that the creation of these satisfactions is related to very different flows of funds into and out of the firm: some to gross revenue (sales volume), others to net revenue; some to profits distributed, others to profits retained; some to investment outlays, others to company expenses. The managers' immediate money incomes and some of the emoluments received in kind are partly at the expense of profits, partly at the expense of corporate income taxes (and every change in tax rates changes the trade-off ratios.) The same is true of several other company expenses which add to the prestige, power, and self-esteem of the managers. Special mention may be made of the provision of stock options for managers, which are either at the expense of the owners' equity (through watering down their stock) or at the expense of potential capital gains on treasury stock earmarked for such stock options, but which, on the other hand, may be a powerful force aligning the managers' personal interests with the goal of maximizing the net profits of the firm.

The point of it all is that the total utility of managers can be increased by decisions which increase expenses at the expense of profits. (Of course, this is confined to situations where profits are high enough to stand encroachments by avoidable expenses—to situations, that is, where the firm is not hard-pressed by competition.) The question is how various changes in conditions will affect managerial decisions on inputs, outputs, and prices if the objectives of management include the gratification of preferences for certain expenses of the firm that compete with the maximization of profits.¹¹

¹¹ Instead of cataloguing the various contributions to the "utility" of the management and their relationships to the sources and uses of the firm's funds, one may wish to classify the expenses of the firm with reference to "discretionary" decisions of the management influenced by the decision-makers' preferences. Here is a tentative classification of this sort:

1. Expenses required for the production of (a) current output of unchanged size, (b) additional current output, with marginal cost not exceeding marginal revenue (hence, contributing to higher profits), and (c) additional current output, with marginal cost exceeding marginal revenue (hence, reducing profits).
2. Expenses not required for the production of current output, but increasing the productive capacity or efficiency of the firm for future production.
3. Expenses for managerial personnel in the form of (a) salaries and bonuses, and (b) services rendered to them for their convenience and pleasure.
4. Expenses not required for either current or future production, but (a) expected of a profitable firm as a social service, and only slightly promoting the public image of management, (b) widely recognized as contributing to the social or national benefit and as indicative of the public spirit of the management, (c) contributing chiefly to the gratification of personal desires of supervisory and managerial personnel, and (d) largely

For purposes of illustration let us reproduce in a literary form the utility function of a management (perhaps of its "peak coordinator" [28, pp. 190-91]) in full control and confident that stockholders will not make any fuss as long as the firm makes a "normal" profit and pays out a fair share of it in dividends. Total utility, which the manager by his decisions will try to maximize, will be a function of a large number of variables, by virtue of the contributions they make to his pride, prestige, self-esteem, conscience, comfort, feeling of accomplishment, material consumption, and anticipations of future benefits and pleasures. Among the variables may be total profits of the firm, growth rate of profits, rate of profits to investment, total sales, growth rate of sales, increase in market share, dividends paid out, retained earnings, increase in market value of stock, price-earnings ratio of stock, investment outlay, salary and bonus received, stock options received (capital gains), expense accounts (consumption at company expense), services received (automobile, chauffeur, lovely secretary, theatre tickets, conferences at resorts), size of staff, expenses for public relations and advertising, expenses for research and development, technological and other innovations, leadership in wage increases and good industrial relations, expenses for public or private education and health, other contributions to public interest and patriotic causes, free time for leisure and recreation, and indications of influence over government, industry, and society. This list of variables is, of course, only representative, not exhaustive.¹²

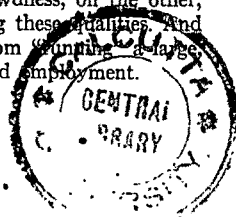
Now what can one do with a utility function of this sort? Will it be of much use in telling us what the firm will do with its freedom of action if it has to respond to a change in conditions?

The answer will depend partly on a simple condition, namely, whether the acceptable trade-off ratios between all the factors contributing to total utility remain unchanged, or approximately the same, if any one of them, say, total profit, increases. If this were the case, we could shout hurrah or sigh a sigh of relief (depending on our temperament). For, if the marginal rates of substitution among all the various "utilifactors" are constant, the distribution of funds among them will remain unchanged with changes in conditions that increase or decrease the total of funds available. Only if the cost of any of the factors

wasteful, that is, contributing nothing, and economizing nothing but managerial effort or capability.

This list may be suggestive of the actions that may have to be taken when, after years of ease and growth, the firm finds its profits declining or disappearing.

¹² Perhaps there ought to be a place on the list for some gratifications that are more stable, less subject to quantitative variation, such as the pleasure of being known for honesty and fairness, on the one hand, and for sharpness and shrewdness, on the other, or at least the pleasure of being convinced of having and exercising these qualities. And last, though not least, there is the general feeling of gratification from running a large, well-known profitable, widely respected firm with growing assets and employment.



changed, say, the cost of staff personnel and, hence, the cost of prestige and other benefits that accrue from having a sizable staff, would the marginal rates of substitution be adapted to the new cost relation. In such a case we might also perhaps be able to tell the kind of response of the decision-makers.

Alas, the condition that the marginal rates of substitution are independent of the total funds available is not likely to be satisfied; in addition, certain types of change in conditions have the bad habit of affecting at the same time funds available and relative costs of utilifactors. For example, an increase in the corporate income tax will change the trade-off ratio between expensable outlays and profits in favor of avoidable expenses.

Marginalism Extended: Choice of Maximanda

If we were interested only in a formal solution, and perhaps in a proof of "existence" of an equilibrium position, we might be satisfied with the maximization of total utility by those who effectively run the firm. If, however, we want to predict the direction of the changes which a given change in conditions is likely to bring about, then mere formalism will not be enough. For predictive purposes we need *more* to go by with the help of *fewer* variables. Maximization of money profits is certainly the simplest "objective function," but it works only in the case of firms exposed to vigorous competition. The management of a firm that makes more than enough money need not go all out to maximize profits; it can afford to do a few other things that it likes, such as serving what by its own lights it regards as the national interest or indulging in other luxuries.

Would this imply "giving up" the principle of marginalism in the theory of the profitable firm? This is chiefly a semantic question. I have been inclined to use a more extended definition. In 1946, I called marginalism "the logical process of finding a maximum" [21, p. 519]. I did not say that it had to be maximization of money profits—though I struggled hard to justify the use of profit maximization in all cases. In the meantime several writers have shown that profit maximization may not be a completely unambiguous objective, even where it is used in splendid isolation from all competing goals, in that it may refuse to yield unambiguous conclusions regarding the effects of certain changes, such as the effects of changes in profit taxes. In addition, it has been shown that several workable "objective functions" can be developed that give plausible results with a few relatively simple terms added. Any of these functions that can be maximized, with or without specific constraints, would still be a part of marginal analysis.

The choice of the *maximandum* is of course a pragmatic matter: we should prefer one that yields sufficiently good approximations to what

we consider reasonable on the basis of empirical research, with wide applicability and fruitfulness and with great simplicity. The compromise among these goals that we accept is, admittedly, a somewhat "subjective" standard of selection, but perfectly in line with the standard accepted in all scientific fields. Concessions to any one of these desiderata must be at the expense of the others.

Let us list some of the alternative *maximanda* that have been suggested and are available for our choice: Total quasi-rents over a short period of time (But how short? This is good only for a freshman course); total quasi-rents during the service-life of existing fixed assets (But is a replaceable part of a machine a fixed asset? This works only for a one-hoss shay); present value of all profits (after taxes) expected in the future, discounted at a "normal" or "competitive" rate; internal rate of return to equity; equity of controlling stockholders; present values of retained earnings; growth rate of equity; gross rate of total assets; growth rate of gross revenue (sales); gross revenue (sales), if net revenues (profits) are satisfactory (over what period of time?); salaries, bonuses, and other accruals (including services in kind) to management, over their entire lives; all accruals to management plus expenditure for staff personnel, compatible with minimum profits; all accruals to management, consistent with satisfactory profits and gradually rising prices of corporate stock; and, of course, the present values of the various combinations of flows mentioned.

Surely a much longer list could be prepared, but there is no use to this. The point should be clear: profit maximization proper may mean a variety of things—several entries apply to money profits—and in addition there are a few other *maximanda* of possible relevance. Incidentally, if profits or accruals to stockholders are not explicitly included in some of the entries, let no one believe that they are really out of the picture. No management could try to maximize its own accruals in the long run if it completely disregarded the interests of the stockholders. Hence, all *maximanda* are subject to the constraint of some minimum benefits to the owners of the business in the form of dividends, capital gains, or both.¹³

Subjective Information and the Charge of Tautology

I have a few remaining tasks, and one of them is to lay a ghost, one that has long played tricks on economists and led them astray. He has

¹³ The four "managerial" variables included in the list—sales, growth of sales, expenses for staff, and emoluments to the management—may well be the most important deviations from profit maximization, although I may easily be persuaded of the existence of other "extravagances" of management. Among the managements of our large corporations there are so many civic-minded men, bursting with social responsibility and cocksure of their ability to know what is in the national interest, that I incline to the thought that rather serious deviations from the profit motive occur in the area of virtuous striving for the so-called

done this in their discussions of the subject of information, its availability, its uncertainty, and its subjectivity. I mean, of course, information available to the "firm," and this raises the question whether we mean the firm as a purely theoretical construct or the firm as an organization of real people or anything else.

The firm as a theoretical construct has exactly the kind of information the theorist chooses to endow it with in order to design a good, useful theory. The firm as an organization of real people has the information system that it actually happens to have and which, in some instances, the management scientists (operations researchers) have succeeded in developing. For purposes of competitive price and allocation theory, it does not make much difference whether the information which we assume the firm to have concerning the conditions of supply, production, and demand under which it works is correct or incorrect, as long as we may safely assume that any *change* in these conditions is registered correctly. If we want to inquire into the effects of a change in wage rates or tax rates or something of this sort, we must of course take it for granted that the decision-makers who supposedly react to the change have taken notice of it. But whether their "previous" store of information—from which they started when the change occurred—was accurate or not will only in exceptional instances make a qualitative difference to the reactions.

This important difference between information about conditions and information about changes in conditions has eluded several writers, who shouted "tautology" when they confronted my statements about the subjectivity of information. They reasoned like this: If firms act on the basis of information which is entirely subjective, then *anything* they do may be said to follow from whatever they believe they know: hence, the assumption of subjectivism defeats any explanatory purposes. This is a sad confusion. In teaching elementary economics we ought to be able to make our students grasp the difference between the shape and position of a curve, on the one hand, and the shift of a curve, on the other. The direction of the effects which we derive from the shift is usually, though not always, independent of the shape and position of the original curve. We need not fuss about the curve reflecting "accurate information" if we only want to see what happens when the curve shifts in a certain direction.

common good. I hope I am not excessively naive if I believe that the excess profits secured through restrictions on competition are to no small extent used for what the discretionary managers believe to be worthy causes. But I see no way of formulating any hypotheses that would enable us to predict either just what the firms' outlays in the public interest will be or how they will affect total output in the long run. I suppose that Boulding's witty question, "do we maximize profit subject to the constraints of morality or do we maximize virtue subject to the constraints of satisfactory profits? [7, p. 17] was not intended to suggest an answer with empirically fertile conclusions.

Since ghosts are hardy creatures, the laying of this one will probably not constitute a once-and-for-all execution. We shall probably see him again thumbing his nose at us in the next textbook or in the next issue of one of our journals.

Imperfect Information and the Question of "Satisficing" Behavior

The same confusion sometimes encumbers the discussions about the alleged "imperfection" of knowledge available to firms for their rational decision-making [39, pp. xxiv-xxvi, 40-41, 81-83, 241-42] and the screens and blockages in "the flow of information through the hierarchies of the organization" [27, pp. 228-29]. But what can be "imperfect" about the information on, say, a tax increase? Why should it take special theories of bureaucracy to explain how the news of a wage increase "flows" through various hierarchical levels up or down or across? Yet this, and this alone, is the information that is essentially involved in the theory of prices and allocation, since it is the *adjustment to such changes* in conditions for which the postulate of maximizing behavior is employed.

One can understand, of course, how the confusion arose. The proponents of managerial analysis have the creditable ambition to reorganize firms in such a way that their managements can really, as a matter of actual fact, maximize the results of their performance, not only in adjusting to changes in conditions, but also in making the most rational arrangements on the basis of the *complete environment* in which they operate.¹⁴ Incidentally, not only "normative micro-economics," as management science has been called [40, p. 279], has this ambition; many propositions of welfare economics are also based on such presuppositions.

As a matter of fact, the interesting distinction made between "satisficing" and "maximizing" or "optimizing" behavior [39, pp. xxiv-xxvi] [40, pp. 262-65] had its origin in precisely the same issue; management, realizing the complexity of the calculations and the imperfection of the data that would have to be employed in any determination of "optimal" decisions, cannot help being satisfied with something less: its behavior will be only "satisficing." What behavior? The mere adjustment to a simple change or the coordinated, integrated whole of its activities? Evidently, only the latter is the overly ambi-

¹⁴ "Economic man deals with the 'real world' in all its complexity," says Herbert Simon [39, p. xxv]. The *homo oeconomicus* I have encountered in the literature was not such a perfectionist. Incidentally, even Simon's "economic man," two years before the ambitious one just quoted, did not have "absolutely complete," but only "impressively clear and voluminous" knowledge of the "relevant aspects of his environment" [38, p. 99]. My point is that we ought to distinguish perfect or imperfect knowledge of (a) the entire environment, (b) the relevant aspects of the entire environment, (c) the relevant changes in environmental conditions.

tious aim. The theory of prices and allocation, viewed as a theory of adjustment to change, does not call for impossible performances.¹⁵ I ask you to remember what I spelled out, twenty years ago, about the difference between exact estimates and calculations, on the one hand, and "sizing up" in nonnumerical terms, on the other [21, pp. 524-25, 534-35]. And I ask you to realize how many more good predictions can be made on the basis of the assumption that firms try to maximize their profits than on the basis of the assumption that they want no more than satisfactory profits. Take one illustration: if an easy-money policy is introduced, we expect that some firms will increase their borrowings, some firms will increase their purchases, some firms will sell at higher prices, and some firms will increase their output. But if everybody was satisfied before the change, we cannot infer any of these things. On the other hand, if we assume that firms prefer a larger profit to a smaller one, all the mentioned consequences follow from the simple model.

The Twenty-one Concepts of the Firm

Several times in this paper I have spoken of the fallacy of misplaced concreteness, committed by mistaking a thought-object for an object of sense perception, that is, for anything in the real, empirical world. My warnings might have given rise to another confusion, namely, that there are only two concepts of the firm. There are many more, and I do not wish to suppress altogether my strong taxonomic propensities. I shall offer a list of ten different contexts calling for even more different concepts, some theoretical, some more empirical.

One of my favorite philosophers, who was a past-master of the art of making fine distinctions, enumerated 13 concepts of "pragmatism" [18], 66 concepts of "nature" [19, pp. 447-56], and "a great number" of concepts of "God."¹⁶ I am sure there are at least 21 concepts of the firm employed in the literature of business and economics,

¹⁵ Suppose the government imposes a 15 per cent surcharge on all import duties. The theory of the profit-maximizing firm will without hesitation tell us that imports will decline. What will the theory of the satisficing firm tell us? "Models of satisficing behavior are richer than models of maximizing behavior, because they treat not only of equilibrium but of the method of reaching it as well. Psychological studies of the formation and change of aspiration levels support propositions of the following kinds. (a) When performance falls short of the level of aspiration, search behavior (particularly search for new alternatives of action) is induced. (b) At the same time, the level of aspiration begins to adjust itself downward until goals reach levels that are practically attainable. (c) If the two mechanisms just listed operate too slowly to adapt aspirations to performance, emotional behavior—apathy or aggression, for example—will replace rational adaptive behavior" [40, p. 263]. I admit that this is an unfair use of the theory of satisficing, but I wanted to show that everything has its place and no theory can be suitable to all problems. I suspect, however, that Simon's theory of satisficing behavior will yield neither quantitative nor qualitative predictions.

¹⁶ Lovejoy Denied Approval by Senate Group," *The Baltimore Sun*, April 1, 1951.

but I shall exercise great forbearance and confine myself to a selection. Everyone may join in the game and fill in what I leave out. I shall first state the context, then delimit the concept, and finally add a few words of explanation.

1. In the theory of competitive prices and allocation, the firm is *an imaginary reactor to environmental changes*. By "imaginary" I mean to stress that this a pure construct for which there need not exist an empirical counterpart. By "reactor" I mean to deny that this robot or puppet can ever have a will of his own: he is the theorist's creature, programmed to respond in the predetermined way.

2. In the theory of innovation and growth, the firm is *an imaginary or a typical reactor or initiator*. Depending on which theory one has in mind, we see that several combinations are possible. In the theory of "entrepreneurial innovation" by men of very special qualities [34, pp. 78-94] the entrepreneur is neither imaginary nor a mere reactor; he is a typical initiator. By "typical" I do not refer to the ideal type of German sociology [47, p. 44] [35, pp. 20-63, 81] [25, pp. 21-57], but rather to the common-sense kind of person that many of us have met in person or, at least, have heard about. On the other hand, there are also theories of "induced invention"—assuming latent inventive-ness (though an invention can never be a mere reaction)—and theories of "induced growth," employing the construct of the imaginary reactor.

3. In welfare economics, the firm is *an imaginary or a typical reactor or initiator with accurate knowledge of his opportunities*. Depending on the proposition in question, all combinations are again possible, but in any case a new requirement is introduced: accurate knowledge of the environmental conditions on the part of all reactors and initiators. For, in contrast to the theory of price and allocation, the welfare theorist wants to ascertain, not only in which direction price, input, and output will move in response to a change, but also whether this move will increase or reduce welfare. For such an exercise it is no longer irrelevant whether the subjective information of the firms is correct or false.

4. In the theory of oligopoly and monopoly, the firm is *a typical reactor and initiator in a small (or zero) interacting group*. I have explained earlier why a theory of oligopoly with nothing but imaginary reactors may not be widely applicable.

5. In the theory of organization (or bureaucracy), the firm is *a typical cooperative system with authoritative coordination*. I have accepted this formulation from one of the authorities [28, p. 187] and thus may disclaim responsibility for it.

6. In management science (or the art of business management), the firm is *a functional information system and decision-making system*

for *typical business operations*. The normative nature of management science should be stressed. Several management scientists include operations research among the agenda of management science. I take this to mean that the principal techniques of operations research of such matters as inventory problems, replacement problems, search problems, queueing problems, and routing problems have to be mastered by the management scientist. He should, however, make a distinction between the science and its application: the science deals with typical systems, but is applied to particular cases.

7. In operations research and consultation, the firm is *an actual or potential client for advice on optimal performance*. In this context the reference is not to the techniques and principles of operations research but rather to the particular projects planned or undertaken.

8. In accounting theory, the firm is *a collection of assets and liabilities*. It should be clear how different this concept is from most of the others.

9. In legal theory and practice, the firm is *a juridical person with property, claims, and obligations*. This may be a very deficient formulation; I defer to the experts, who will surely correct it.

10. In statistical description (such as the Census of Manufactures) the firm is *a business organization under a single management or a self-employed person with one or more employees or with an established place of business*. I have adopted here the definition used by the U.S. Census.

This exercise should have succeeded in showing how ludicrous the efforts of some writers are to attempt *one* definition of *the* firm as used in economic analysis, or to make statements supposedly true of "the" firm, or of "its" behavior, or what not. Scholars ought to be aware of equivocations and should not be snared by them.

A Sense of Proportion

I hope there will be no argument about which concept of the firm is the most important or the most useful. Since they serve different purposes, such an argument would be pointless. It would degenerate into childish claims about one area of study being more useful than another.

I also hope the specialist who uses one concept of the firm will desist from trying to persuade others to accept his own tried and trusted concept for entirely different purposes. The concept of the firm in organization theory, for example, need not at all be suitable for accounting theory or legal theory; and I know it is not suitable for either competitive price theory or for oligopoly theory.

Most of the controversies about the "firm" have been due to misun-

derstandings about what the other specialist was doing. Many people cannot understand that others may be talking about altogether different things when they use the same words.

I am not happy about the practice of calling any study just because it deals with or employs a concept of the firm "economics" or "micro-economics." But we cannot issue licenses for the use of such terms and, hence, must put up with their rather free use. My own prejudices balk at designating organization theory as economics—but other people's prejudices are probably different from mine, and we gain little or nothing from arguing about the correct scope of our field.

Now what conclusions from all our reviewing may we draw on the conflicts between marginal analysis, behavioral theory, and managerial theory of the firm? Fortunately, not much time is being wasted on descriptive studies of a narrowly behaviorist kind, in the sense of recording observed behavior without any prior theoretical design. Most proponents of behavioral studies of the firm are too competent theorists for that. As far as the proponents of managerial theories are concerned, they have never claimed to be anything but marginalists, and the behavior goals they have selected as worthy for incorporation into behavior equations, along with the goal of making profits, were given a differentiable form so that they could become part of marginal analysis.¹⁷ Thus, instead of a heated contest between marginalism and managerialism in the theory of the firm, a marriage between the two has come about.

Not all marriages, these days, are permanent; divorces are frequent. Whether this marriage will last or end in divorce will depend chiefly on what offspring it will produce. If the match of the profit hypothesis with the various managerial hypotheses proves fertile of sufficiently interesting deductions, the prospects of a lasting marriage are good.

It is not easy to judge the future sterility or fertility of this marriage between marginalism and managerialism, because most of us are inclined to underrate the kinds of problem on which we have never

¹⁷ While under profit maximization $MR - MC = 0$, sales maximization requires that $MR = 0$; hence, for some of the output sold marginal revenue is less than marginal cost, which cuts into profits. A minimum-profit constraint sets a limit to this.

In the case of maximization of the growth rate of sales the limit on nonremunerative selling is built into the objective itself because a growth of productive assets is required to support the growth of sales, and the acquisition of these assets presupposes a sufficiency of profits, either for internal financing or as a basis for outside finance [3, pp. 1086-87]. If at any time sales were pushed too hard at the expense of profits, there would arise a shortage of funds for acquiring the productive assets needed for producing more output. Thus no separate minimum-profit constraint has to be imposed, since it is inherent in the objective of maximization of the growth of sales. It should be understood, however, that the growth rate of assets under this objective is still less than it could be under straight profit maximization. (This shows why we should never speak of the "growth of the firm" without specifying by what criterion we measure it.)

worked: we have a bias in favor of our own research experience. Most of the researchers on behavioral versions of the theory of the firm look for their problems to the records of selected large corporations. They take it for granted that their theory must be designed to explain and predict the behavior of these firms. This, however, is less so in the case of economists engaged in the analysis of relative prices, inputs, and outputs. They look for their problems to the records of entire industries or industrial sectors. To be sure, some industries are dominated by large corporations, yet the accent of the analysis is not on the behavior of these firms but at best on some of the results of that behavior. Where the focus is not on the behavior of the firm, a theory that requires information on particular firms to be "plugged in" seems to them less serviceable than a more general theory, at least as long as only qualitative, not numerical, results are sought. Hence, even if the "partial-equilibrium analyst" knows full well that the actual situation is not a really competitive one, he probably will still make a first try using the competitive model with good old-fashioned profit maximization. And if the results appear too odd, appropriate qualifications may still be able to take care of them more simply than if he had started with a cumbersome managerial model. (In saying this, I am showing my bias.)

It is revealing to ask what kind of theory we would apply, at least in a first approximation, if we were called upon to predict the results of various kinds of public-policy measures. For questions regarding short-run effects of changes in the corporation income tax (or an excess-profits tax) I believe a strong case can be made in favor of a model of the firm with some managerial variables. If the problem is whether an increase in cigarette taxes is likely to be fully shifted onto the consumer or what portion of it may be absorbed by the producers, I may feel safer with a model that includes managerial objectives. If, however, the problem is what qualitative effects an increase in the import duty on a material used in several industries will have on its imports and on the prices and outputs of the various products of the industries in question, I would be inclined to work with the simple hypothesis of profit maximization. I would find it far too cumbersome in this case to go down to the level of the "real" firms; I could probably not obtain the necessary data and, even if I did, I might not be able to rely on the composite results obtained from a firm-by-firm analysis. The old theory of the firm, where all firms are pure fictions, may give me—in this case—most of the answers, in a rough and ready way, not with any numerical precision, but with sufficient reliability regarding the directions of change.

I conclude that the choice of the theory has to depend on the prob-

lem we have to solve.¹⁸ Three conditions seem to be decisive in assigning the type of approach to the type of problem. The simple marginal formula based on profit maximization is suitable where (1) *large groups* of firms are involved and nothing has to be predicted about particular firms, (2) the effects of a *specified change* in conditions upon prices, inputs, and outputs are to be explained or predicted rather than the values of these magnitudes before or after the change, and nothing has to be said about the "total situation" or general developments, and (3) only *qualitative answers*, that is, answers about directions of change, are sought rather than precise numerical results. Managerial marginalism is more suitable to problems concerning particular firms and calling for numerical answers. And, I am sure, there are also some problems to which behavioral theory may be the most helpful approach. My impression is that it will be entirely concerned with particular firms and perhaps designed to give answers of a normative, that is, advisory nature.

It looks as if I had prepared the ground for a love feast: I have made polite bows in all directions and have tuned up for a hymn in praise of peaceful coexistence of allegedly antagonistic positions. But I cannot help raising a question which may tear open some of the wounds of the battle of 1946. The question is whether the effects of an effective increase in minimum wages upon the employment of labor of low productivity can, at our present state of knowledge, be fruitfully analyzed with any other model than that of simple marginalism based on unadulterated profit maximization.

If I answer in the negative, does this mean that we are back at the old quarrel and have not learned anything? It does not mean this. Deficiencies in marginal analysis have been shown and recognized; and a great deal of good empirical as well as theoretical work has been accomplished. But the deficiencies dealt with were not just those which the critics twenty years ago attacked. That attack questioned the applicability of marginal analysis to the employment effects of wage increases in industries with many firms presumably under heavy competition [16, pp. 64, 75-77]. In such circumstances the managerial theories of the firm, according to their proponents, do not apply. On this narrow issue, therefore, the old-type marginalist cannot retreat.

¹⁸ As a matter of fact, it will also depend on the research techniques which the appointed analyst has learned to master; we can eliminate this bias by assuming an ideal analyst equally adept in all techniques.

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SOME NEW RESULTS IN THE MEASUREMENT OF CAPACITY UTILIZATION

By L. R. KLEIN AND R. S. PRESTON*

I. Statement of the Problem

The concepts of capacity output and rate of capacity utilization have been prominent in recent discussions of U.S. economic potential. These are important descriptive concepts for the economy as a whole and merit careful attention in statistical measurement. They are also very difficult concepts to measure and are not yet well established in our economic annals. Capacity variables have been significant ingredients in the econometric models constructed at the Wharton School, and a great deal of effort has been devoted to development of a measure which would reliably serve a wide range of uses in applied econometrics [2] [6, pp. 53-59].

The Wharton School Index of capacity utilization is constructed by a very simple procedure which enables us to provide quick and frequent estimates of this useful economic statistic. Briefly, the method involves marking off cyclical peaks for each of thirty component indexes of the Federal Reserve Board's Index of Industrial Production and then fitting linear segments between successive peaks. Between the present time period and the last established peak, the previous linear segment is extrapolated at its established slope unless the index in the present period exceeds the extrapolated trend line. In this case a new slope is computed by fitting a new linear segment from the last cyclical peak to the present value of the index. Revisions will be continuously made in the last linear segments until there is a cyclical downturn in the index of industrial production. This establishes a new peak, and the process thus begins again. The trend lines through peaks, we assume, represent an index of capacity output, on a base of actual output, namely the same base as that used in the F.R.B. index.

In constructing the Wharton Series our basic data are quarterly series constructed from three-month averages of the F.R.B. seasonally adjusted monthly industry indexes [5]. A monthly index is being constructed and tested, but we shall deal here with the quarterly index. Normally, peak values must exceed immediately preceding and following quarterly values, but we must deal with some special cases. When the index of output is unchanged for two or more successive peak

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quarters, we choose the first of the two as the capacity peak. When output declines from a peak and then rises again, with the decline no more than one quarter in duration, the greater of the two high values is selected as a single capacity peak.

The ratio of actual output to the trend line (linear segments) fitted through peaks is called capacity utilization. For each quarter, such ratios are computed for each of the thirty component industries and then averaged, using F.R.B. index weights, to reach our economy-wide index of capacity utilization.

A principal objection to this method of estimating capacity utilization is that some peaks may be marked off as full capacity utilization peaks when in fact there may have been considerable underutilization of capacity. The objections have been summarized by A. Phillips [3] in his survey of various alternative capacity measures. To be practical and specific, the general presumption among economists is that the recovery from the 1957-59 recession was incomplete, and the subsequent peaks in many sectors represent points of substantial underutilization of capacity. The Wharton School Index of capacity utilization is therefore thought to be biased upwards since 1958-59.

It is the purpose of this study to develop an alternative measure of capacity and capacity utilization at the industry level from production functions. This is done for a sample of 11 of the 30 industries which make up the Wharton Index. We then compare the results from this newer method with those obtained by the method described above. An adjustment for those component Wharton Indexes which are found to be subject to bias drift is developed. Finally, a method is developed to adjust the aggregate Wharton Index for bias drift, based on our results of the sample of 11 industries studied.

II. *The Estimation of Capacity and Capacity Utilization From Production Functions*

Another approach to the problem of capacity estimation is by the estimation of cost or production functions by sector. In the case of cost functions, it has often been suggested that the point of minimum average cost may represent full capacity output (in a competitive environment). The estimation of cost functions, by sectors, may be a fruitful step in capacity estimation, but there is a problem in obtaining a sharply defined minimum point for empirical average cost functions.

In this study we approach the problem through the use of production functions. For each sector we define actual output by the conventional production function relationship.

$$(1) \quad X_t = Ae^{\gamma t} L_t^\alpha K_t^\beta v_t$$

where X_t is actual output at time t , L_{ut} is manhours employed at time t , K_{ut} is real capital utilized at time t , $e^{\gamma t}$ is a proxy for technical change, and v_t is the disturbance at time t .

Full capacity output is defined as

$$(2) \quad X_{c,t} = A e^{\hat{\gamma}t} L_t^{\hat{\alpha}} K_t^{\hat{\beta}},$$

where $X_{c,t}$ denotes full capacity real output at time t , L_t is available manhours at time t (in practice, frictional unemployment is allowed for), K_t is fully utilized real capital at time t , and $e^{\gamma t}$ is a proxy for technical change.

The function f is the same in (1) as in (2), apart from the fact that parameters in (1) have been replaced by their estimates in (2). The disturbance $\log v_t$ is given its expected value (zero) in (2). In our particular case, errors in estimating $X_{c,t}$ will be caused by measurement error for L_t and K_t , by misspecification of the equation, or by biased parameter estimates of (1). Assuming that L_t and K_t are measured without error, that the relationship has not been misspecified, and that our estimating method produces unbiased estimates of the parameters, we may regard $X_{c,t}$ as error free.

III. *The Estimation of Series on Utilized Capital Stock and Capacity Manhour Inputs by Industrial Sector*

Among the many time honored problems in the estimation of production functions there are two particularly thorny ones involved in the present study. It is very difficult to determine K_{ut} in (1) and L_t in (2). If there were a large number of full capacity peak points for each industry, (2) could be estimated directly from a sample of peak observations on $X_{c,t}$, L_t , and K_t . Many difficulties would thus be overcome simultaneously. We considered the possibility of making direct estimates of (2) from the very small sample of post-war full capacity peak point observations, but the smallness of the sample made the results too unstable for practical use. Equation (1) is the production function, whose statistical study was initiated by the great pioneering investigations of Paul Douglas. The problem in the present context is that capital is not fully utilized over the business cycle; therefore estimates of the stock of capital in existence (the only ones we have directly) do not furnish a suitable input variable during periods of under-capacity utilization. We do have direct estimates of X_t and L_{ut} , the latter being expressed in manhours.

If we had estimates of (1), we could compute $X_{c,t}$ as values on the production function surface corresponding to full capacity inputs of L_t and K_t . It is difficult to obtain estimates of the series K_{ut} in estimating

(1), and it is similarly difficult to obtain estimates of the series on L_t in estimating X_{e_t} from (2). Since we are estimating production functions and associated capacity values by industry, we have the difficult task of determining values of L_t for each industry.

We have an industrial distribution of actual manhour employment. From this we can aggregate and adjust this aggregate for induced labor force participation and the national rate of unemployment to arrive at an economy-wide supply of manhour series, but we do not know the supply of manhours to each sector. Such manhour data are not directly available.

Lacking estimates of capital utilization by industry, we are forced to use a proxy. Different measures have been used in previous studies, e.g., Carl Christ used the rate of new investment as such a proxy, on the assumption that new investment would be determined by the percentage of unutilized capital [1]. We have used a more direct proxy, namely the rate of manhour employment by industry. This, in concept, follows the procedure of Robert Solow [4]. It is apparent that the measurement of K_{u_t} cannot be completed until L_t is measured for each industry in our sample. Both measurement problems are tied together.

To relate capital utilization to manhour utilization, we assume, as Solow has done,

$$(3) \quad \frac{K_{u_t}}{K_t} = \frac{L_{e_t}}{L_t}.$$

Equation (3) is not an unreasonable assumption. It can be shown that this relationship in combination with the Cobb-Douglas form of the production function will enable us to relate the rate of unemployment directly to the rate of capacity utilization. In this case it can be shown that logarithms of estimated industry utilization rates are linear functions of logarithms of industry employment rates. The parameters in these relationships are those of the Cobb-Douglas production function and vary from industry to industry.

This can be seen very quickly. If we assume that the production function is Cobb-Douglas and that capacity output is defined as in (2), with K_{u_t} defined as in (3), then we find that

$$\frac{X_t}{X_{e_t}} = \left(\frac{L_{e_t}}{L_t} \right)^{\alpha+\beta} v_t.$$

For any given industry the degree of homogeneity of this function is the same as that of the production function for the industry. Increasing returns to scale in the production function would then be indirectly

supported by the observation that proportional changes in unemployment rates result in more than proportional changes in the utilization of capacity.

Although we are limited to computing production functions for only eleven sectors, our restriction being availability of capital data, the construction of series on L_t for each industrial sector, cover 31 sectors (both manufacturing and nonmanufacturing). These 31 sectors include all the industries included as component sectors of the Industrial Production Index plus all nonmanufacturing categories, except agriculture and the self-employed. As a result, the construction of series on L_t for each industrial sector involved a larger sample of sectors than the final computation of production functions. It is felt that, in the construction of L_t , as many sectors as possible should be taken into account in the aggregation to a national figure.

Our first step is to estimate full-employment (full capacity) manhours for the economy as a whole. We shall then allocate this total among sectors. Let us define (in terms of men—not manhours) L_t as total labor force at full capacity, L_{e_t} as employed labor force, L_{v_t} as involuntarily unemployed labor force, and L_{f_t} as frictionally unemployed labor force. We shall measure full capacity labor force as the value of L_t which satisfies (4), given L_{e_t} , L_{v_t} , and L_{f_t} .

$$(4) \quad L_t = L_{e_t} + L_{v_t} + L_{f_t} + \delta[L_t - L_{e_t} - L_{f_t}],$$

where δ is the coefficient of response of labor supply to employment opportunities. This makes full capacity labor force the sum of employed and unemployed persons plus an induced amount depending on the level of unemployment. We may solve (4) for L_t as follows.

$$(4a) \quad (1 - \delta)(L_t - L_{f_t}) = (1 - \delta)L_{e_t} + L_{v_t}$$

$$(4b) \quad L_t - L_{f_t} = L_{e_t} \left[1 + \left(\frac{1}{1 - \delta} \right) \left(\frac{L_{v_t}}{L_{e_t} + L_{v_t} + L_{f_t}} \right) \right]$$

so that

$$(4c) \quad L_t - L_{f_t} = L_{e_t} \{ 1 + (\mu_t - \mu_{f_t}) / [(1 - \delta)(1 - \mu_t)] \},$$

where

$$(4d) \quad \mu_t = (L_{v_t} + L_{f_t}) / (L_{e_t} + L_{v_t} + L_{f_t})$$

and

$$(4e) \quad \mu_{f_t} = L_{f_t} / (L_{e_t} + L_{v_t} + L_{f_t}).$$

We note that μ_t is equal to the total unemployment rate and μ_{ft} is equal to the frictional unemployment rate. We shall measure full employment manhours, as actual manhours used (L_{et} expressed in hours) adjusted by

$$(5) \quad 1.0 + (\mu_t - \mu_{ft}) / [(1 - \mu_t)(1 - \delta)].$$

This gives us a measure of potential manhours less frictional manhours. The above adjustment factor is observable from published employment series provided we have estimates of μ_{ft} and δ .

We choose μ_{ft} as 2.67 per cent, the low rate of unemployment observed in the record quarter of 1953. We place δ at .25, an estimate which is consistent with the labor supply equation estimated in econometric models developed at the Wharton School in which the capacity utilization indexes are intended to be used [2].

The labor supply equation estimated in [2] is

$$(6) \quad L_{et} + L_{vt} + L_{ft} = 61.2 + .226t - .310(L_{vt} + L_{ft}).$$

We can see that the coefficient of .310 in this labor supply equation suggests a value of δ in (4) of .25. Write down the above expression (4) for L_t as

$$(7a) \quad L_t = L_{et} + L_{vt} + L_{ft} + \delta(L_t - L_{et} - L_{vt} - L_{ft} + L_{vt}).$$

Rearranging terms, we find

$$(7b) \quad L_{et} + L_{vt} + L_{ft} = L_t - \left(\frac{\delta}{1 - \delta}\right)(L_{vt} + L_{ft}) + \left(\frac{\delta}{1 - \delta}\right)L_{ft}.$$

We assume that the frictional unemployment rate is a constant proportion of fully employed labor force. Thus we can write

$$(7c) \quad L_{ft} = kL_t,$$

and we can rewrite (7b) as

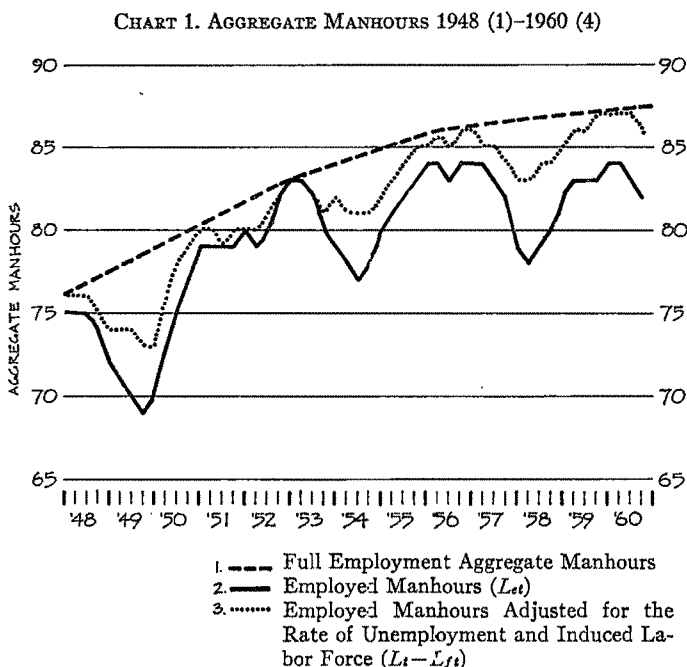
$$(7d) \quad L_{et} + L_{vt} + L_{ft} = \left(1 + \left(\frac{\delta}{1 - \delta}\right)k\right)L_t - \left(\frac{\delta}{1 - \delta}\right)(L_{vt} + L_{ft}).$$

The coefficient of $(L_{vt} + L_{ft})$ in (6) is therefore equal to $-\delta/(1 - \delta)$. This suggests an estimate of δ of about .25. We therefore evaluate

$$(8) \quad L_t - L_{ft} = L_{et} \left[1.0 + \frac{\mu_t - .0267}{(1.0 - \mu_t)(.75)} \right]$$

as our estimate of available manhours, where L_{et} results from the aggregation over our 31 industries in each time period.

It was found, after the series $L_t - L_{f,t}$ was computed from the series $L_{e,t}$, that this aggregate supply series still had cycles in it. This is in part due to the fact that our arguments in the previous section were carried out in terms of people and the final adjustment (8) was made to a man-hour concept. We correct this in the following fashion. On a priori grounds the aggregate supply of manhours should have no cycle, but should be a smooth monotonic increasing function of time, reflecting additions to the labor force as the economy grows. The cycle in $L_t - L_{f,t}$ may



be due to variations in overtime and short shift work which is not reflected in hiring and firing policy. At peaks of the series $L_t - L_{f,t}$ man-hour employment resulting from overtime is at its peak; therefore, by fitting linear segments through peaks we approximate the smooth monotonic increasing trend of the aggregate supply of manhours for the 31 sectors as a whole.

These results are summarized in Chart 1. In Chart 1, series 1 is full employment aggregate manhours. This series has resulted from the linear interpolation of the peaks of $L_t - L_{f,t}$. Series 2 is actual manhours, in our notation $L_{e,t}$. Series 3 is, in our notation, $L_t - L_{f,t}$.

Our problem now is to redistribute series 1 in Chart 1 over our 31 industries. In our sample of 31 industries, the distribution of employment changes over time due to changes in relative factor rewards (wage

rates), caused by shifts in demand and changes in worker tastes. The demand for a factor is a derived demand, derived from the demand for the product. With the aggregate supply function stable over time, the industry distribution of manhours is then determined by the relative importance of each sector in the production of aggregate output. We approximate the trend in *capacity input* of labor to each industry by first approximating the trend in the share each sector received of the aggregate at full employment peaks. By fitting linear segments between these computed shares we can approximate the trend in the share of total manhours used by each sector at peak demand points.

It is important to understand that these linear segments were not fitted between peaks of the individual sector series, but between the benchmark manhour shares that each industry got at points where series 2 on Chart 1 peaked. We now argue that these trend series represent the full capacity share for each of our 31 sectors at any point in time. We multiply these percentages into series 1 to arrive at the trend in capacity manhours for each of our individual sectors. Aggregate unemployment or induced labor force participation is distributed over industrial sectors in accordance with the trend in the *share* of each sector in the aggregate. As a result, these sectors whose share in the aggregate is increasing will also receive an increasing share of unemployment or induced labor force participation. Furthermore, those industries that have larger shares in the aggregate will receive larger shares of aggregate unemployment or induced participation. This, of course, is a simplifying assumption.

IV. *The Estimation of Production Functions*

We do not assume constant returns to scale and thus place no restrictions on $\alpha + \beta$. There is much evidence in favor of this procedure in the case of the American economy, although there are some investigators who impose the restriction $\alpha + \beta = 1$. If we assume cost minimization rather than profit maximization behavior, to allow for the presence of increasing returns to scale, it is possible to estimate the ratio β/α first from factor share data. Direct estimates of (1) lead to problems of multicollinearity and least squares bias. In the cost minimization equation

$$(9) \quad \frac{q_t K_{u_t}}{w_t L_{e_t}} = \frac{\beta}{\alpha} + z_t,$$

where $q_t K_{u_t}$ is nonwage income originating in period t , $w_t L_{e_t}$ is wage income originating in period t , and z_t is a disturbance in period t , we have an estimate of β/α as

$$(10) \quad \left(\frac{\hat{\beta}}{\alpha}\right) = \frac{1}{T} \sum_{t=1}^T \frac{q_t K_{ut}}{w_t L_{et}}.$$

We then estimate A , α and γ from the regression

$$(11) \quad \text{Log}_e(x_t) = \text{Log}_e(A) + \gamma t + \alpha \left[\text{Log}_e(L_{et}) + \left(\frac{\hat{\beta}}{\alpha}\right) \text{Log}_e(K_{ut}) \right] + \text{Log}_e v_t.$$

With estimates \hat{A} , $\hat{\gamma}$, $\hat{\alpha}$ and $\hat{\beta}$, where $\hat{\beta} = \hat{\alpha}(\hat{\beta}/\hat{\alpha})$, we can now compute capacity output from (2), given K_t and L_t for each industry.

Using single equation least-squares regression methods to estimate (11), we shall obtain biased estimates of coefficients, as the error $\text{log}_e v_t$ is not independent of $\text{log}_e(L_{et}) + (\hat{\beta}/\hat{\alpha}) \text{log}_e(K_{ut})$. We recognize the existence of this bias but do not take it into account in the capacity estimation procedure. The ratio of observed values of X_t to the computed values of X_{et} are our estimates of capacity utilization rates, R_t .

$$R_t = \frac{X_t}{X_{et}}.$$

V. The Method Used To Adjust The Component Wharton Indexes

Denote the individual Wharton School Indexes by Wh_t . As will be seen in Section IX, the time profile of the R_t and Wh_t series are remarkably similar. Both series deal with essentially the same phenomenon, but in some sectors the Wh_t series drifts higher than the R_t series, especially in recent years. This is the bias we are seeking to measure.

We correlate the two series to find

$$(12) \quad Wh_t = a + bR_t + ct.$$

Primarily, we are not interested in the difference in the level between Wh_t and R_t , for we think the Wh_t series establishes a reasonable level in the period before 1957. We are especially interested in the tendency of the Wh_t series to drift upwards in recent years. Therefore, where the estimated regression coefficient c is significant and positive, we assume that an element of bias has been found. The adjusted Wharton Index Wh_t^A is then computed for those industries where bias is found as

$$(13) \quad Wh_t^A = Wh_t - ct.$$

We make this adjustment to all sector Wh_t series where a significant and positive c coefficient is found.

VI. *The Method Used to Adjust the Aggregate Wharton Index*

As mentioned earlier our sample of industries studied deals only with a subgroup of those industries which make up the aggregate Wharton Index. We now develop a method which will enable us to draw a conclusion about the bias in the Aggregate Wharton Index on the basis of our sample subgroup. As mentioned earlier, this subgroup is composed of 11 industries.

Let r_i be the F.R.B. weight for the i -th industry. For the 11 industries in our sample, form (for $t=t_0$)

$$(14) \quad \frac{\sum_{i=1}^{11} r_i (Wh_i - c_i)}{\sum_{i=1}^{11} r_i} = \frac{\sum_{i=1}^{11} r_i Wh_i}{\sum_{i=1}^{11} r_i} - \frac{\sum_{i=1}^{11} r_i c_i}{\sum_{i=1}^{11} r_i},$$

$$(14a) \quad = Wh^S - \bar{c},$$

where S stands for sample subgroup. If we could do this for all industries in the index we would have

$$(15) \quad \sum_{i=1}^{11} r_i Wh_i - \sum_{i=1}^{11} r_i c_i + \sum_{i=12}^{30} r_i Wh_i - \sum_{i=12}^{30} r_i c_i = Wh^A,$$

$$\text{since } \sum_{i=1}^{30} r_i = 1.0.$$

We can combine the first and third terms to get

$$(16) \quad \sum_{i=1}^{30} r_i Wh_i - \sum_{i=1}^{11} r_i c_i - \sum_{i=12}^{30} r_i c_i = Wh^A.$$

But we do not know the last term on the left hand side

$$\sum_{i=12}^{30} r_i c_i.$$

If we assume that subgroup 1 to 11 and 12 to 30 would each give the same average correction term, we can write

$$(17) \quad \frac{\sum_{i=1}^{11} r_i c_i}{\sum_{i=1}^{11} r_i} = \bar{c} = \frac{\sum_{i=12}^{30} r_i c_i}{\sum_{i=12}^{30} r_i}$$

OR

$$\bar{c} \sum_{i=1}^{11} r_i = \sum_{i=1}^{11} r_i c_i, \quad \text{and} \quad \bar{c} \sum_{i=12}^{30} r_i = \sum_{i=12}^{30} r_i c_i.$$

As a result we can now write

$$(18) \quad Wh^A = \sum_{i=1}^{30} r_i Wh_i - \bar{c} \sum_{i=1}^{11} r_i - \bar{c} \sum_{i=12}^{30} r_i.$$

This reduces to $Wh^A = Wh - \bar{c}$, where Wh is the unadjusted 30 sector weighted average, Wh^A is the adjusted 30 sector weighted average, and \bar{c} is defined above as the mean adjustment for the 11 sectors.

We assume that the bias accumulated in the Wh_i index by 1960 terminates a tendency of the Wharton School Index to drift upwards and make adjustment after that date by the fixed amount applied to 1960.

VII. *Description of The Data*

Our basic data series used in the present study are the Federal Reserve Board's monthly index of industrial production on a 1958 base, the F.R.B. industry weights, a quarterly series on total man-hours used by each sector, a quarterly series on GNP originating in 1954 dollars for each sector, a quarterly series on fixed capital in 1954 dollars for each sector, a yearly series on national income originating for each sector, and a yearly series on wages, salaries and supplements for each sector.

Three-month averages were constructed from the monthly series (seasonally adjusted) of the F.R.B. index to yield a quarterly series of the industrial production index. For this series the sample period runs from 1947-1 to 1965-2. For all other series, the sample period runs from 1948 to 1960. The quarterly series on total manhour employment are aggregated from series on average weekly hours and average number of employees. The capital stock series is essentially a series on constant dollar net investment accumulated from an initial period estimate of the stock of capital. In the estimation of (β/α) , income of nonlabor factors was computed by a residual method, namely as national income originating minus wages, salaries and supplements.

In two cases the Federal Reserve Board's classification did not agree with our production function breakdown. These were Food and Beverages and Gas and Electric Utilities. This was remedied by computing a weighted average of Food and Beverages of the Wharton Index using F.R.B. weights and by computing a weighted average of Gas and Electric Utilities of the Wharton Index using F.R.B. weights. In nine other F.R.B. categories we have comparable data. For seventeen other F.R.B. categories we have no comparable production function data.

TABLE 1.—PRODUCTION FUNCTION COEFFICIENTS
 $X_t = A e^{\gamma t} L_t^\alpha K_t^\beta v_t'$

| | $\widehat{\log A}$ | $\hat{\alpha}$ | $\hat{\beta}$ | $\widehat{(\beta/\alpha)}$ | $\hat{\gamma}$ | \bar{R}^2 |
|---------------------------|--------------------|-----------------|---------------|----------------------------|------------------|-------------|
| Primary metals | -3.848 (.892) | 1.141 (.058) | .371 | .325 | .0001 (.0005) | .888 |
| Non-electrical machinery | -.291 (.692) | .953 (.047) | .240 | .252 | .0020 (.0004) | .929 |
| Electrical machinery | .122 (.810) | .924 (.058) | .228 | .247 | .0049 (.0008) | .974 |
| Autos and equipment | 1.935 (1.233) | .699 (.075) | .387 | .553 | .0039 (.0009) | .755 |
| Stone clay and glass | -.941 (1.179) | .988 (.086) | .273 | .276 | .0031 (.0006) | .936 |
| Food and beverages | 12.750 (1.177) | .070 (.075) | .022 | .320 | .0059 (.0002) | .972 |
| Textile mill products | .111 (.856) | .898 (.060) | .192 | .214 | .0096 (.0006) | .853 |
| Paper and allied products | -3.651 (1.052) | 1.115 (.072) | .429 | .385 | .0006 (.0007) | .985 |
| Chemicals | 5.947 (1.701) | .418 (.099) | .251 | .601 | .0130 (.0012) | .971 |
| Rubber products | 1.056 (.813) | .839 (.063) | .241 | .287 | .0043 (.0004) | .939 |
| Public utilities | -3.791 (2.225) | .763 (.101) | .763 | 1.000 | .0084 (.0016) | .995 |

The 11 groupings with which we finally ended included 62 per cent of the total F.R.B. weight, including all the most important categories of the Wharton Aggregate Index.

VIII. *The Estimated Production Functions*

In Table 1 we present the estimated production functions for the 11 sectors we are dealing with in this study. In all but two industries we find that the data suggest that increasing returns to scale is the proper assumption about the degree of homogeneity of the production function. Chemicals, and Food and Beverages show decreasing returns to scale. Among all the industries studied, Food and Beverages yields the least reasonable estimate of α and β . Most of the variance of X_t is explained by a straight trend, our proxy for technical change. All the estimates of

γ are positive and significant except for Primary Metals and Paper and Allied Products, where the coefficient is not significant.

IX. *The Time Profiles of the Wh_t and R_t Series and the Isolation of the Bias Drift by Industry*

In Table 2 we give the series $R_t(1)$, the series $Wh_t(2)$ and the difference $R_t - Wh_t(3)$. Although in the isolation of the bias drift primary emphasis will be placed on the regression results, in the final analysis both the inspection of Table 2 and the regression results will play a part in determining whether a particular sector series is subject to drift.

For Primary Metals, both series agree on all turning points. The time path of the two series is similar until the 1959 steel strike. At this point the R_t series takes on higher peak and trough values. The computed regression result is

$$Wh_t = 12.682 + .917R_t - .297t \quad \bar{R}^2 = .9330.$$

(3.613) (.038) (.033)

Although the coefficient of time is significant, its sign indicates a downward bias in the Wharton Index. This result is due to the extremely high peaks which the R_t series reaches in the late fifties. As a result of the steel strike in 1959 and the associated production for inventories, it is felt that the Wh_t series is approximately correct, not subject to a bias drift, since the peak at 1959-2 was close to a full capacity peak on a priori grounds. The height of the R_t series in the 1958-1960 range is due to a computed downturn in the level of capacity. This resulted in forcing the R_t series higher than the Wh_t series at this peak compared to other capacity peaks. This computed downturn in the R_t series begins in 1958. This may be seen by looking at Table 2. Here we see the difference between R_t and Wh_t . This difference, until 1958, exhibits no clear trend; after 1958 there is a sharp increase in the difference. On the hypothesis that the 1959 peak was a capacity peak due to the steel strike and that the prediction of the R_t series after 1958 is false, we conclude that Primary Metals is not subject to a bias drift.

For Non-Electrical Machinery the time profile of the two series is quite similar. Both series agree on all major turning points except for the trough of the 1958 recession where there is one quarter difference. There is a bit of disagreement in the 1959-1960 range. By looking at Table 2 it is evident that there is no upward drift of the Wh_t series. Although there seems to be a systematic cycle, there is no evidence of a persistent upward drift in the Wh_t series. This is supported by the computed regression.

$$Wh_t = 9.217 + .906R_t - .141t \quad \bar{R}^2 = .8475,$$

(4.904) (.054) (.034)

TABLE 2—PERCENTAGE UTILIZATION OF CAPACITY BY INDUSTRIAL SECTOR
1948-1 TO 1960-4

(1) = R_t ; (2) = Wh_t ; (3) = $R_t - Wh_t$

| Quarter | Primary Metals | | | Nonelectrical Machinery | | | Electrical Machinery | | |
|---------|----------------|--------|-------|-------------------------|--------|-------|----------------------|--------|--------|
| | (1) | (2) | (3) | (1) | (2) | (3) | (1) | (2) | (3) |
| 48-1 | 93.44 | 95.99 | -2.55 | 92.79 | 94.88 | -2.09 | 88.24 | 93.96 | -5.72 |
| 48-2 | 92.45 | 94.83 | -2.38 | 90.75 | 93.60 | -2.85 | 87.86 | 90.05 | -2.19 |
| 48-3 | 96.52 | 98.89 | -2.37 | 92.17 | 92.48 | -.31 | 85.80 | 88.53 | -2.73 |
| 48-4 | 95.29 | 97.41 | -2.12 | 88.48 | 89.67 | -1.19 | 81.50 | 83.91 | -2.41 |
| 49-1 | 91.72 | 93.39 | -1.67 | 80.75 | 84.02 | -3.27 | 75.76 | 76.14 | -.38 |
| 49-2 | 80.13 | 81.38 | -1.25 | 72.91 | 75.76 | -2.85 | 71.29 | 71.83 | -.54 |
| 49-3 | 76.18 | 76.95 | -.77 | 71.42 | 71.24 | .18 | 73.71 | 73.34 | .37 |
| 49-4 | 58.88 | 59.15 | -.27 | 67.56 | 66.54 | 1.02 | 72.67 | 73.91 | -1.24 |
| 50-1 | 81.94 | 81.79 | .15 | 72.33 | 68.98 | 3.35 | 74.10 | 80.98 | -6.88 |
| 50-2 | 94.62 | 94.05 | .57 | 78.93 | 75.87 | 3.06 | 79.96 | 89.86 | -9.90 |
| 50-3 | 101.43 | 100.00 | 1.43 | 91.15 | 82.55 | 8.60 | 86.15 | 100.00 | -13.85 |
| 50-4 | 100.66 | 98.78 | 1.88 | 92.37 | 87.12 | 5.25 | 90.38 | 94.44 | -4.06 |
| 51-1 | 100.98 | 98.69 | 2.29 | 92.48 | 90.76 | 1.72 | 86.59 | 92.52 | -5.93 |
| 51-2 | 102.61 | 100.00 | 2.61 | 96.88 | 93.91 | 2.97 | 86.14 | 87.48 | -1.34 |
| 51-3 | 101.39 | 99.32 | 2.07 | 99.63 | 96.00 | 3.63 | 82.28 | 75.39 | 6.89 |
| 51-4 | 96.94 | 96.00 | .94 | 104.31 | 98.12 | 6.19 | 84.76 | 79.07 | 5.69 |
| 52-1 | 94.88 | 95.40 | -.52 | 102.04 | 98.86 | 3.18 | 89.57 | 82.85 | 6.72 |
| 52-2 | 72.51 | 73.86 | -1.35 | 99.66 | 97.98 | 1.68 | 90.52 | 84.43 | 6.09 |
| 52-3 | 77.02 | 79.21 | -2.19 | 97.10 | 95.07 | 2.03 | 95.11 | 88.74 | 6.37 |
| 52-4 | 96.82 | 100.00 | -3.18 | 103.22 | 98.39 | 4.83 | 101.01 | 96.40 | 4.61 |
| 53-1 | 95.45 | 99.04 | -3.59 | 105.66 | 100.00 | 5.66 | 99.96 | 100.00 | -.04 |
| 53-2 | 95.88 | 99.89 | -4.01 | 105.31 | 99.70 | 5.61 | 103.21 | 100.00 | 3.21 |
| 53-3 | 95.44 | 99.12 | -3.68 | 101.09 | 98.13 | 2.96 | 99.19 | 97.13 | 2.06 |
| 53-4 | 84.27 | 87.26 | -2.99 | 92.14 | 91.70 | .44 | 87.93 | 87.14 | .79 |
| 54-1 | 74.20 | 76.40 | -2.20 | 88.09 | 85.27 | 2.82 | 85.83 | 81.76 | 4.07 |
| 54-2 | 72.79 | 74.21 | -1.42 | 84.31 | 82.92 | 1.39 | 83.37 | 82.08 | 1.29 |
| 54-3 | 73.55 | 74.54 | -.99 | 85.49 | 82.10 | 3.39 | 84.38 | 84.32 | .06 |
| 54-4 | 78.13 | 78.75 | -.62 | 80.84 | 80.39 | .45 | 88.91 | 86.72 | 2.19 |
| 55-1 | 88.46 | 88.71 | -.25 | 84.78 | 82.00 | 2.78 | 89.81 | 88.89 | .92 |
| 55-2 | 96.71 | 96.51 | .20 | 87.53 | 87.02 | .51 | 90.64 | 91.41 | -.77 |
| 55-3 | 100.31 | 99.27 | 1.04 | 91.70 | 90.42 | 1.28 | 93.72 | 93.70 | .02 |
| 55-4 | 102.16 | 100.00 | 2.16 | 94.74 | 94.52 | .22 | 92.87 | 94.49 | -1.62 |
| 56-1 | 101.26 | 98.13 | 3.13 | 96.23 | 95.66 | .57 | 93.48 | 94.40 | -.92 |
| 56-2 | 100.67 | 96.84 | 3.83 | 97.96 | 97.07 | .89 | 101.94 | 99.19 | 2.75 |
| 56-3 | 78.49 | 75.34 | 3.15 | 99.21 | 100.00 | -.79 | 99.49 | 99.82 | -.33 |
| 56-4 | 100.46 | 96.75 | 3.71 | 97.38 | 98.96 | -1.58 | 99.21 | 99.70 | -.49 |
| 57-1 | 96.60 | 93.67 | 2.93 | 92.85 | 97.07 | -4.22 | 96.36 | 97.24 | -.88 |
| 57-2 | 93.25 | 89.99 | 3.26 | 93.73 | 93.33 | .40 | 96.13 | 94.45 | 1.68 |
| 57-3 | 93.37 | 89.70 | 3.67 | 92.19 | 91.29 | .90 | 98.04 | 94.09 | 3.95 |
| 57-4 | 80.86 | 77.37 | 3.49 | 83.40 | 83.64 | -.24 | 87.90 | 87.53 | .37 |
| 58-1 | 64.46 | 61.35 | 3.11 | 72.67 | 73.82 | -1.15 | 78.71 | 80.17 | -1.46 |
| 58-2 | 64.93 | 61.25 | 3.68 | 74.81 | 69.90 | 4.91 | 75.27 | 75.53 | -.26 |
| 58-3 | 75.06 | 69.67 | 5.39 | 77.10 | 71.95 | 5.15 | 80.65 | 81.29 | -.64 |
| 58-4 | 84.93 | 77.11 | 7.82 | 77.86 | 73.31 | 4.55 | 85.46 | 84.76 | .70 |
| 59-1 | 96.89 | 86.24 | 10.65 | 84.98 | 76.81 | 8.17 | 88.64 | 89.15 | -.51 |
| 59-2 | 114.86 | 100.00 | 14.86 | 94.08 | 84.28 | 9.80 | 94.71 | 94.25 | .46 |
| 59-3 | 56.70 | 48.78 | 7.92 | 93.35 | 87.36 | 5.99 | 94.04 | 97.90 | -3.86 |
| 59-4 | 78.81 | 66.63 | 12.18 | 93.64 | 85.41 | 8.23 | 94.55 | 96.76 | -2.21 |
| 60-1 | 111.07 | 92.74 | 18.33 | 99.21 | 84.57 | 14.64 | 96.43 | 100.00 | -3.57 |
| 60-2 | 95.22 | 78.05 | 17.17 | 97.33 | 83.37 | 13.96 | 91.59 | 97.91 | -6.32 |
| 60-3 | 84.74 | 68.89 | 15.85 | 94.13 | 83.55 | 10.58 | 89.40 | 94.29 | -4.89 |
| 60-4 | 76.52 | 61.22 | 15.30 | 92.29 | 78.57 | 13.72 | 86.23 | 88.87 | -2.64 |

TABLE 2—(Continued)

| Quarter | Autos and Parts | | | Stone Clay and Glass | | | Food and Beverages | | |
|---------|-----------------|--------|-------|----------------------|--------|-------|--------------------|-------|------|
| | (1) | (2) | (3) | (1) | (2) | (3) | (1) | (2) | (3) |
| 48-1 | 91.81 | 90.73 | 1.08 | 96.21 | 94.29 | 1.92 | 101.51 | 94.28 | 7.23 |
| 48-2 | 91.48 | 87.74 | 3.74 | 95.66 | 95.64 | .02 | 101.68 | 94.53 | 7.15 |
| 48-3 | 95.03 | 89.52 | 5.51 | 95.97 | 95.36 | .61 | 99.82 | 92.63 | 7.19 |
| 48-4 | 97.24 | 89.15 | 8.09 | 93.09 | 93.03 | .06 | 100.43 | 93.20 | 7.23 |
| 49-1 | 91.49 | 81.92 | 9.57 | 85.98 | 87.72 | -1.74 | 99.93 | 92.78 | 7.15 |
| 49-2 | 91.11 | 79.45 | 11.66 | 86.86 | 80.86 | 6.00 | 99.65 | 92.68 | 6.97 |
| 49-3 | 98.16 | 83.48 | 14.68 | 85.67 | 79.87 | 5.80 | 99.17 | 92.39 | 6.78 |
| 49-4 | 84.35 | 70.16 | 14.19 | 84.73 | 81.20 | 3.53 | 99.13 | 92.59 | 6.54 |
| 50-1 | 89.04 | 72.81 | 16.23 | 90.46 | 84.15 | 6.31 | 98.01 | 91.82 | 6.19 |
| 50-2 | 110.85 | 88.69 | 22.16 | 99.86 | 91.10 | 8.76 | 99.39 | 93.05 | 6.34 |
| 50-3 | 127.03 | 100.00 | 27.03 | 109.36 | 96.44 | 12.92 | 101.94 | 95.44 | 6.50 |
| 50-4 | 121.10 | 96.03 | 25.07 | 111.01 | 100.00 | 11.01 | 100.70 | 94.46 | 6.24 |
| 51-1 | 112.97 | 90.41 | 22.56 | 109.01 | 100.00 | 9.01 | 102.77 | 96.10 | 6.67 |
| 51-2 | 101.68 | 82.30 | 19.38 | 112.66 | 100.00 | 12.66 | 98.47 | 92.78 | 5.69 |
| 51-3 | 86.29 | 70.92 | 15.37 | 106.11 | 96.84 | 9.27 | 98.73 | 92.99 | 5.74 |
| 51-4 | 81.65 | 68.20 | 13.45 | 99.14 | 92.75 | 6.39 | 98.69 | 92.95 | 5.74 |
| 52-1 | 77.69 | 65.68 | 12.01 | 98.26 | 91.51 | 6.75 | 99.89 | 93.88 | 6.01 |
| 52-2 | 76.95 | 65.87 | 11.08 | 95.49 | 90.17 | 5.32 | 99.93 | 93.71 | 6.22 |
| 52-3 | 60.46 | 52.30 | 8.16 | 97.61 | 90.02 | 7.59 | 99.98 | 93.85 | 6.13 |
| 52-4 | 85.35 | 74.65 | 10.70 | 98.92 | 92.75 | 6.17 | 100.73 | 94.92 | 5.81 |
| 53-1 | 90.96 | 80.43 | 10.53 | 102.52 | 94.07 | 8.45 | 99.24 | 93.92 | 5.32 |
| 53-2 | 92.85 | 82.82 | 10.03 | 97.96 | 92.86 | 5.10 | 98.27 | 93.26 | 5.01 |
| 53-3 | 90.77 | 80.47 | 10.30 | 95.69 | 91.34 | 4.35 | 99.39 | 94.46 | 4.93 |
| 53-4 | 82.37 | 72.87 | 9.50 | 90.56 | 87.96 | 2.60 | 98.11 | 93.55 | 4.56 |
| 54-1 | 77.86 | 69.22 | 8.64 | 88.40 | 85.23 | 3.17 | 97.73 | 93.50 | 4.23 |
| 54-2 | 78.14 | 69.76 | 8.38 | 92.14 | 85.01 | 7.13 | 98.54 | 94.60 | 3.94 |
| 54-3 | 74.08 | 66.35 | 7.73 | 92.51 | 86.44 | 6.07 | 98.84 | 95.06 | 3.78 |
| 54-4 | 78.48 | 70.73 | 7.75 | 93.88 | 88.26 | 5.62 | 97.78 | 94.03 | 3.75 |
| 55-1 | 100.50 | 90.61 | 9.89 | 97.28 | 91.99 | 5.29 | 98.85 | 94.79 | 4.06 |
| 55-2 | 109.49 | 98.58 | 10.91 | 99.58 | 95.79 | 3.79 | 100.49 | 96.37 | 4.12 |
| 55-3 | 111.61 | 100.00 | 11.61 | 98.90 | 98.24 | .66 | 100.19 | 96.13 | 4.06 |
| 55-4 | 106.69 | 95.05 | 11.64 | 100.19 | 99.36 | .83 | 101.51 | 97.76 | 3.75 |
| 56-1 | 91.46 | 81.20 | 10.26 | 100.10 | 99.48 | .62 | 101.87 | 98.46 | 3.41 |
| 56-2 | 81.31 | 72.31 | 9.00 | 98.54 | 100.00 | -1.46 | 101.56 | 98.38 | 3.18 |
| 56-3 | 78.02 | 69.73 | 8.29 | 97.12 | 97.64 | -.52 | 100.97 | 97.86 | 3.11 |
| 56-4 | 82.28 | 73.80 | 8.48 | 97.97 | 98.25 | -.28 | 100.94 | 97.90 | 3.04 |
| 57-1 | 89.84 | 80.45 | 9.39 | 97.87 | 96.34 | 1.53 | 99.61 | 96.50 | 3.11 |
| 57-2 | 87.85 | 78.00 | 9.85 | 94.92 | 95.12 | -.20 | 99.21 | 96.16 | 3.05 |
| 57-3 | 88.16 | 77.84 | 10.32 | 92.06 | 94.69 | -2.63 | 99.46 | 96.15 | 3.31 |
| 57-4 | 80.82 | 70.49 | 10.33 | 85.57 | 90.76 | -5.19 | 97.70 | 94.93 | 2.77 |
| 58-1 | 68.72 | 58.61 | 10.11 | 79.35 | 81.15 | -1.80 | 97.41 | 94.81 | 2.60 |
| 58-2 | 62.84 | 52.81 | 10.03 | 83.56 | 83.16 | .40 | 98.82 | 96.27 | 2.55 |
| 58-3 | 62.50 | 52.16 | 10.34 | 90.80 | 91.26 | -.46 | 99.24 | 96.65 | 2.59 |
| 58-4 | 74.90 | 61.47 | 13.43 | 87.74 | 90.51 | -2.77 | 98.93 | 96.75 | 2.18 |
| 59-1 | 90.31 | 72.00 | 18.31 | 92.30 | 93.32 | -1.02 | 99.07 | 97.38 | 1.69 |
| 59-2 | 101.88 | 79.98 | 21.90 | 101.52 | 100.00 | 1.52 | 101.31 | 99.23 | 2.08 |
| 59-3 | 100.01 | 77.29 | 22.72 | 100.25 | 99.99 | .26 | 100.55 | 98.81 | 1.74 |
| 59-4 | 81.58 | 61.92 | 19.66 | 91.35 | 97.66 | -6.31 | 99.97 | 97.78 | 2.19 |
| 60-1 | 117.01 | 87.79 | 29.22 | 95.24 | 96.88 | -1.64 | 100.60 | 98.38 | 2.22 |
| 60-2 | 112.73 | 82.51 | 30.22 | 96.44 | 95.93 | .51 | 100.61 | 98.31 | 2.30 |
| 60-3 | 109.27 | 79.29 | 29.98 | 93.56 | 93.69 | -.13 | 100.96 | 97.80 | 3.16 |
| 60-4 | 101.76 | 72.60 | 29.16 | 88.29 | 90.62 | -2.33 | 100.38 | 97.94 | 2.44 |

TABLE 2—(Continued)

| Quarter | Textile Mill Products | | | Paper and Allied Products | | | Chemicals | | |
|---------|-----------------------|--------|-------|---------------------------|--------|-------|-----------|--------|-------|
| | (1) | (2) | (3) | (1) | (2) | (3) | (1) | (2) | (3) |
| 48-1 | 101.75 | 95.62 | 6.13 | 97.10 | 93.69 | 3.41 | 97.57 | 96.31 | 1.26 |
| 48-2 | 106.59 | 100.00 | 6.59 | 97.36 | 94.02 | 3.34 | 96.86 | 95.25 | 1.61 |
| 48-3 | 92.94 | 95.68 | -2.74 | 94.71 | 91.51 | 3.20 | 96.45 | 94.26 | 2.19 |
| 48-4 | 92.28 | 88.21 | 4.07 | 90.83 | 87.69 | 3.14 | 93.51 | 90.70 | 2.81 |
| 49-1 | 87.98 | 85.35 | 2.63 | 85.67 | 82.35 | 3.32 | 89.88 | 86.56 | 3.32 |
| 49-2 | 79.79 | 78.33 | 1.46 | 81.06 | 77.60 | 3.46 | 84.71 | 81.10 | 3.61 |
| 49-3 | 86.64 | 85.53 | 1.11 | 85.56 | 81.48 | 4.08 | 85.26 | 81.12 | 4.14 |
| 49-4 | 94.50 | 93.04 | 1.46 | 90.18 | 85.37 | 4.81 | 88.85 | 84.21 | 4.64 |
| 50-1 | 93.73 | 91.93 | 1.80 | 92.11 | 86.83 | 5.28 | 92.64 | 87.68 | 4.96 |
| 50-2 | 94.55 | 92.33 | 2.22 | 96.16 | 90.26 | 5.90 | 98.32 | 92.89 | 5.43 |
| 50-3 | 100.04 | 97.80 | 2.24 | 102.07 | 95.32 | 6.75 | 103.65 | 97.70 | 5.95 |
| 50-4 | 101.18 | 99.25 | 1.93 | 104.81 | 97.17 | 7.64 | 105.95 | 99.49 | 6.46 |
| 51-1 | 101.52 | 100.00 | 1.52 | 107.54 | 98.73 | 8.81 | 106.63 | 99.75 | 6.88 |
| 51-2 | 99.01 | 98.32 | .69 | 106.33 | 100.00 | 6.33 | 106.77 | 100.00 | 6.77 |
| 51-3 | 88.37 | 87.94 | .43 | 96.54 | 88.48 | 8.06 | 104.24 | 98.15 | 6.09 |
| 51-4 | 84.81 | 84.46 | .35 | 90.11 | 83.34 | 6.77 | 101.43 | 95.91 | 5.52 |
| 52-1 | 87.31 | 87.12 | .19 | 91.02 | 85.01 | 6.01 | 101.20 | 96.45 | 4.75 |
| 52-2 | 86.89 | 86.86 | .03 | 88.12 | 82.97 | 5.15 | 98.82 | 94.75 | 4.07 |
| 52-3 | 93.85 | 93.56 | .29 | 89.37 | 84.39 | 4.98 | 99.49 | 96.01 | 3.48 |
| 52-4 | 96.77 | 95.79 | .98 | 94.18 | 88.85 | 5.33 | 100.46 | 97.81 | 2.65 |
| 53-1 | 96.40 | 94.45 | 1.95 | 94.73 | 89.53 | 5.20 | 99.92 | 98.14 | 1.78 |
| 53-2 | 99.83 | 96.76 | 3.07 | 96.66 | 91.42 | 5.24 | 101.08 | 100.00 | 1.08 |
| 53-3 | 95.40 | 92.17 | 3.23 | 96.69 | 91.63 | 5.06 | 98.92 | 97.96 | .96 |
| 53-4 | 85.30 | 82.76 | 2.54 | 92.81 | 88.09 | 4.72 | 94.27 | 94.13 | .14 |
| 54-1 | 83.46 | 81.38 | 2.08 | 91.32 | 86.79 | 4.53 | 90.92 | 91.23 | -.31 |
| 54-2 | 85.36 | 83.22 | 2.14 | 92.19 | 88.00 | 4.19 | 89.93 | 88.81 | 1.12 |
| 54-3 | 88.25 | 86.00 | 2.25 | 93.06 | 89.09 | 3.97 | 90.65 | 89.78 | .87 |
| 54-4 | 90.73 | 88.01 | 2.72 | 94.02 | 90.48 | 3.54 | 91.87 | 91.71 | .16 |
| 55-1 | 93.69 | 90.29 | 3.40 | 96.84 | 93.44 | 3.40 | 95.42 | 95.65 | -.23 |
| 55-2 | 96.06 | 92.09 | 3.97 | 99.72 | 96.57 | 3.15 | 98.72 | 98.26 | .46 |
| 55-3 | 97.40 | 92.85 | 4.55 | 101.43 | 98.29 | 3.14 | 100.61 | 99.71 | .90 |
| 55-4 | 99.35 | 94.44 | 4.91 | 102.63 | 100.00 | 2.63 | 101.71 | 100.00 | 1.71 |
| 56-1 | 99.92 | 94.64 | 5.28 | 101.59 | 99.42 | 2.17 | 100.12 | 99.51 | .61 |
| 56-2 | 98.62 | 92.98 | 5.64 | 100.66 | 98.85 | 1.81 | 100.66 | 99.86 | .80 |
| 56-3 | 98.13 | 92.16 | 5.97 | 99.61 | 98.50 | 1.11 | 100.85 | 100.00 | .85 |
| 56-4 | 97.44 | 91.25 | 6.19 | 97.27 | 96.95 | .32 | 98.91 | 98.89 | .02 |
| 57-1 | 95.38 | 89.07 | 6.31 | 95.19 | 95.62 | -.43 | 98.70 | 100.00 | -1.30 |
| 57-2 | 95.56 | 89.82 | 5.74 | 93.08 | 94.12 | -1.04 | 98.24 | 98.68 | -.44 |
| 57-3 | 95.12 | 89.93 | 5.19 | 92.64 | 94.40 | -1.76 | 98.01 | 98.05 | -.04 |
| 57-4 | 88.42 | 83.86 | 4.56 | 90.99 | 93.33 | -2.34 | 95.02 | 95.04 | -.02 |
| 58-1 | 83.68 | 79.45 | 4.23 | 86.66 | 89.38 | -2.72 | 89.10 | 89.34 | -.24 |
| 58-2 | 86.56 | 82.31 | 4.25 | 86.22 | 89.22 | -3.00 | 88.62 | 88.67 | -.05 |
| 58-3 | 93.55 | 89.29 | 4.26 | 90.97 | 94.36 | -3.39 | 92.50 | 91.86 | .64 |
| 58-4 | 93.86 | 89.85 | 4.01 | 92.63 | 95.94 | -3.31 | 93.61 | 92.18 | 1.43 |
| 59-1 | 97.88 | 94.99 | 2.89 | 92.33 | 96.01 | -3.68 | 94.37 | 92.92 | 1.45 |
| 59-2 | 103.33 | 99.56 | 3.77 | 95.69 | 98.76 | -3.07 | 98.88 | 96.01 | 2.87 |
| 59-3 | 104.08 | 100.00 | 4.08 | 96.68 | 100.00 | -3.32 | 100.04 | 96.90 | 3.14 |
| 59-4 | 98.13 | 95.05 | 3.08 | 94.49 | 97.09 | -2.60 | 98.30 | 95.41 | 2.89 |
| 60-1 | 98.18 | 94.58 | 3.60 | 94.95 | 97.11 | -2.16 | 98.06 | 94.46 | 3.60 |
| 60-2 | 98.94 | 94.64 | 4.30 | 93.59 | 95.54 | -1.95 | 100.44 | 95.09 | 5.35 |
| 60-3 | 95.96 | 91.58 | 4.38 | 92.15 | 94.27 | -2.12 | 98.87 | 93.72 | 5.15 |
| 60-4 | 84.98 | 83.85 | 1.13 | 90.18 | 91.47 | -1.29 | 95.44 | 90.98 | 4.46 |

TABLE 2—(Continued)

| Quarter | Rubber Products | | | Public Utilities | | |
|---------|-----------------|--------|-------|------------------|-------|-------|
| | (1) | (2) | (3) | (1) | (2) | (3) |
| 48-1 | 98.33 | 90.98 | 7.35 | 97.56 | 99.20 | -1.64 |
| 48-2 | 95.32 | 88.39 | 6.93 | 96.86 | 97.40 | -.54 |
| 48-3 | 95.06 | 88.34 | 6.72 | 98.06 | 97.34 | .72 |
| 48-4 | 90.43 | 84.23 | 6.20 | 98.69 | 96.95 | 1.74 |
| 49-1 | 86.13 | 79.97 | 6.16 | 96.64 | 93.98 | 2.66 |
| 49-2 | 82.50 | 76.44 | 6.06 | 94.55 | 91.95 | 2.60 |
| 49-3 | 79.96 | 74.59 | 5.37 | 94.90 | 92.02 | 2.88 |
| 49-4 | 82.74 | 78.55 | 4.19 | 95.45 | 91.67 | 3.78 |
| 50-1 | 86.50 | 84.18 | 2.32 | 98.02 | 93.05 | 4.97 |
| 50-2 | 92.44 | 92.08 | .36 | 99.91 | 94.42 | 5.49 |
| 50-3 | 100.17 | 100.00 | .17 | 99.86 | 94.26 | 5.60 |
| 50-4 | 102.02 | 99.50 | 2.52 | 100.62 | 95.43 | 5.19 |
| 51-1 | 98.14 | 93.45 | 4.69 | 102.19 | 97.17 | 5.02 |
| 51-2 | 99.70 | 93.84 | 5.86 | 103.04 | 98.09 | 4.95 |
| 51-3 | 95.82 | 89.81 | 6.01 | 102.52 | 97.76 | 4.76 |
| 51-4 | 89.40 | 84.17 | 5.23 | 102.33 | 97.46 | 4.87 |
| 52-1 | 90.50 | 85.77 | 4.73 | 102.32 | 97.45 | 4.87 |
| 52-2 | 88.34 | 84.20 | 4.14 | 100.67 | 95.49 | 5.18 |
| 52-3 | 89.95 | 85.64 | 4.31 | 101.72 | 96.55 | 5.17 |
| 52-4 | 99.03 | 93.95 | 5.08 | 102.77 | 97.62 | 5.15 |
| 53-1 | 100.64 | 94.41 | 6.23 | 101.50 | 97.18 | 4.32 |
| 53-2 | 100.22 | 93.37 | 6.85 | 101.85 | 98.33 | 3.52 |
| 53-3 | 94.87 | 88.20 | 6.67 | 100.98 | 98.09 | 2.89 |
| 53-4 | 84.55 | 79.17 | 5.38 | 98.09 | 95.41 | 2.68 |
| 54-1 | 84.23 | 79.57 | 4.66 | 97.45 | 95.14 | 2.31 |
| 54-2 | 88.85 | 84.30 | 4.55 | 97.01 | 95.16 | 1.85 |
| 54-3 | 81.66 | 77.90 | 3.76 | 97.61 | 95.57 | 2.04 |
| 54-4 | 91.85 | 88.44 | 3.41 | 97.48 | 95.68 | 1.80 |
| 55-1 | 96.92 | 94.34 | 2.58 | 97.80 | 96.18 | 1.62 |
| 55-2 | 101.61 | 100.00 | 1.61 | 98.24 | 96.60 | 1.64 |
| 55-3 | 98.02 | 96.57 | 1.45 | 99.57 | 98.07 | 1.50 |
| 55-4 | 101.53 | 99.25 | 2.28 | 99.99 | 98.95 | 1.04 |
| 56-1 | 94.94 | 92.15 | 2.79 | 100.05 | 99.26 | .79 |
| 56-2 | 92.41 | 89.25 | 3.16 | 100.92 | 99.81 | 1.11 |
| 56-3 | 92.44 | 89.31 | 3.13 | 99.25 | 98.34 | .91 |
| 56-4 | 93.52 | 91.00 | 2.52 | 98.33 | 97.92 | .41 |
| 57-1 | 94.42 | 92.83 | 1.59 | 98.60 | 97.99 | .61 |
| 57-2 | 89.99 | 89.40 | .59 | 98.00 | 98.06 | -.06 |
| 57-3 | 92.18 | 91.76 | .42 | 99.12 | 99.12 | 0.00 |
| 57-4 | 87.10 | 86.06 | 1.04 | 96.98 | 97.26 | -.28 |
| 58-1 | 76.41 | 74.90 | 1.51 | 94.73 | 95.67 | -.94 |
| 58-2 | 77.98 | 76.10 | 1.88 | 93.09 | 94.30 | -1.21 |
| 58-3 | 87.77 | 85.11 | 2.66 | 94.23 | 95.52 | -1.29 |
| 58-4 | 94.21 | 90.60 | 3.61 | 93.95 | 95.76 | -1.81 |
| 59-1 | 96.96 | 92.71 | 4.25 | 95.72 | 97.58 | -1.86 |
| 59-2 | 94.34 | 89.84 | 4.50 | 95.95 | 98.42 | -2.47 |
| 59-3 | 106.14 | 100.00 | 6.14 | 95.85 | 97.63 | -1.78 |
| 59-4 | 99.08 | 92.03 | 7.05 | 95.49 | 98.24 | -2.75 |
| 60-1 | 100.43 | 91.74 | 8.69 | 96.64 | 99.70 | -3.06 |
| 60-2 | 101.17 | 91.22 | 9.95 | 96.16 | 99.06 | -2.90 |
| 60-3 | 97.28 | 86.74 | 10.54 | 95.32 | 98.26 | -2.94 |
| 60-4 | 91.51 | 80.42 | 11.09 | 94.22 | 96.47 | -2.25 |

where the coefficient of time is of the wrong sign. We conclude that Non-Electrical Machinery is not subject to an upward drift.

By looking at the results for Electrical Machinery in Table 2 we see an example of the bias we are looking for. For Electrical Machinery from 1951-3 onward, there is a persistent downward drift in the difference $R_t - Wh_t$. This means that the Wharton series has been drifting upwards relative to the R_t series. Computing the appropriate regression for the period 1951-3 to 1960-4 we have

$$Wh_t = -7.337 + 1.022R_t + .234t \quad \bar{R}^2 = .9316.$$

(4.416) (.047) (.029)

The coefficient of time in this equation is significant and of the right sign. On this evidence we conclude in the case of Electrical Machinery that the Wh_t series is subject to drift since 1951-3.

In the case of Autos and Equipment the two series are extremely similar, with agreement at all turning points. By looking at Table 2 it is apparent that there is no systematic drift in the Wh_t series. The regression results yield

$$Wh_t = 20.214 + .684R_t - .193t \quad \bar{R}^2 = .8418.$$

(4.338) (.044) (.044)

Although the coefficient of time is significant, it is of the wrong sign. We conclude therefore that for Autos and Equipment the Wh_t series is subject to no bias drift.

For Stone, Clay and Glass the time path of the two series is very similar. Table 2 reveals that there is a persistent upward drift in the Wh_t series. This drift begins in 1950-3 and continues through 1960-4. The regression for the period 1950-3 to 1960-4 yields

$$Wh_t = 8.690 + .821R_t + .286t \quad \bar{R}^2 = .8330.$$

(5.965) (.057) (.033)

The coefficient of time is significant. For Stone, Clay and Glass, an element of bias has been found.

Food and Beverages reveal the most marked trend in the Wh_t series that we have noted thus far. The time profiles of the two series are extremely similar except for the persistent upward drift in the Wh_t series. This is revealed in Table 2, where there is a marked downward drift in the difference $R_t - Wh_t$. Computing the appropriate regression for the period 1948-1 to 1960-4 we have

$$Wh_t = 3.980 + .885R_t + .114t \quad \bar{R}^2 = .9628.$$

(4.447) (.045) (.004)

The coefficient of time is both significant and of the right sign. Food and Beverages is subject to drift.

For Textile Mill Products the time profile and agreement of turning points is remarkably similar. By looking at Table 2 a slight downward drift in the difference $R_t - Wh_t$ is detected since 1957-1. This suggests that there has been a slight upward drift in the Wh_t series since 1957. Computing the appropriate regression for the period 1957-1 to 1960-4, we have the result

$$Wh_t = .490 + .933R_t + .204t \quad \bar{R}^2 = .9847.$$

(3.019) (.033) (.041)

Paper and Allied Products show a marked upward drift in the Wh_t series from 1951 to 1960. This upward drift is clearly shown in Table 2. Apart from this drift, the time path of Wh_t is similar to that of the R_t series. Computing the regression for the period 1951-1 to 1960-4, we have

$$Wh_t = .176 + .916R_t + .298t \quad \bar{R}^2 = .9530.$$

(3.618) (.037) (.015)

Although Chemicals and Allied Products have a tendency to drift upwards from the period 1951 to 1958, this upward drift does not continue, as seen in Table 3. The regression for the entire period is,

$$Wh_t = 11.488 + .842R_t + .055t \quad \bar{R}^2 = .8312.$$

(5.278) (.054) (.018)

Although the coefficient of t is significant and has the right sign, we conclude that this series is not subject to bias drift. This result is based on the visual inspection of Table 2 rather than the regression result. We feel that the Table reveals the picture of a cycle rather than systematic drift. It is the long down swing in this cycle that yields the results of the regression, which we feel do not lead to correct judgment on trend drift.

For Rubber Products the time paths of the two series show agreement in all turning points as can be seen by inspection of Table 2. Table 2 reveals no systematic drift in the difference between the Wharton series and R_t . Computing the appropriate regression for the period 1948-1 to 1960-4, we find

$$Wh_t = 3.068 + .919R_t + .001t \quad \bar{R}^2 = .8549.$$

(4.919) (.053) (.024)

It is apparent from Table 2 that Public Utilities is subject to an upward drift. Table 2 suggests that this drift did not begin until 1950. Computing the appropriate regression for the period 1950-3 to 1960-4, we find

TABLE 3—THE ADJUSTMENT OF THE WHARTON AGGREGATE INDEX

| Quarter | Unadjusted Aggregate Wharton Index (1) | Eleven Sector Weighted Average, No Adjustment (2) | Eleven Sector Weighted Average, Six Adjusted (3) | (2)–(3) (4) | (1)–(4) (5) |
|---------|--|---|--|----------------|----------------|
| 1948–1 | 94.88 | 94.86 | 94.85 | .02 | 94.86 |
| 2 | 93.87 | 93.88 | 93.84 | .04 | 93.83 |
| 3 | 93.16 | 93.35 | 93.29 | .06 | 93.10 |
| 4 | 90.92 | 91.08 | 91.01 | .08 | 90.84 |
| 1949–1 | 86.62 | 86.79 | 86.69 | .10 | 86.53 |
| 2 | 82.33 | 81.70 | 81.59 | .12 | 82.22 |
| 3 | 81.22 | 81.45 | 81.32 | .13 | 81.09 |
| 4 | 79.21 | 78.97 | 78.81 | .15 | 79.06 |
| 1950–1 | 82.46 | 83.62 | 83.44 | .17 | 82.29 |
| 2 | 87.88 | 89.90 | 89.71 | .19 | 87.69 |
| 3 | 93.37 | 95.49 | 95.27 | .22 | 93.15 |
| 4 | 93.72 | 95.61 | 95.34 | .27 | 93.44 |
| 1951–1 | 94.09 | 95.88 | 95.54 | .34 | 93.75 |
| 2 | 93.14 | 94.83 | 94.43 | .41 | 92.73 |
| 3 | 90.16 | 91.25 | 90.75 | .50 | 89.66 |
| 4 | 89.56 | 90.19 | 89.60 | .60 | 88.97 |
| 1952–1 | 90.64 | 90.86 | 90.18 | .68 | 89.96 |
| 2 | 88.22 | 87.80 | 87.03 | .77 | 87.44 |
| 3 | 89.58 | 88.13 | 87.27 | .86 | 88.71 |
| 4 | 95.08 | 94.74 | 93.79 | .95 | 94.13 |
| 1953–1 | 95.79 | 95.62 | 94.57 | 1.04 | 94.75 |
| 2 | 96.19 | 96.15 | 95.01 | 1.14 | 95.06 |
| 3 | 95.22 | 94.84 | 93.61 | 1.23 | 93.99 |
| 4 | 89.79 | 88.99 | 87.67 | 1.32 | 88.47 |
| 1954–1 | 86.62 | 85.36 | 83.95 | 1.41 | 85.21 |
| 2 | 86.45 | 85.05 | 83.55 | 1.50 | 84.95 |
| 3 | 86.32 | 85.23 | 83.64 | 1.59 | 84.73 |
| 4 | 87.49 | 86.75 | 85.07 | 1.68 | 85.81 |
| 1955–1 | 90.91 | 91.18 | 89.41 | 1.77 | 89.14 |
| 2 | 93.74 | 94.90 | 93.04 | 1.86 | 91.88 |
| 3 | 95.17 | 96.44 | 94.49 | 1.95 | 93.21 |
| 4 | 96.43 | 97.62 | 95.58 | 2.04 | 94.38 |
| 1956–1 | 95.31 | 96.21 | 94.07 | 2.14 | 93.18 |
| 2 | 95.21 | 95.97 | 93.74 | 2.23 | 92.99 |
| 3 | 93.90 | 93.47 | 91.15 | 2.32 | 91.58 |
| 4 | 95.64 | 95.80 | 93.39 | 2.41 | 93.23 |
| 1957–1 | 95.27 | 95.14 | 92.63 | 2.51 | 92.76 |
| 2 | 94.10 | 93.29 | 90.67 | 2.61 | 91.49 |
| 3 | 93.32 | 93.01 | 90.30 | 2.71 | 90.61 |
| 4 | 88.49 | 87.82 | 85.01 | 2.81 | 85.68 |
| 1958–1 | 82.02 | 80.81 | 77.90 | 2.91 | 79.11 |
| 2 | 80.92 | 79.64 | 76.62 | 3.01 | 77.90 |
| 3 | 85.09 | 83.32 | 80.20 | 3.11 | 81.97 |
| 4 | 87.32 | 85.77 | 82.55 | 3.21 | 84.10 |
| 1959–1 | 89.86 | 89.37 | 86.06 | 3.32 | 86.54 |
| 2 | 94.02 | 94.50 | 91.09 | 3.42 | 90.61 |
| 3 | 90.55 | 89.65 | 86.13 | 3.52 | 87.03 |
| 4 | 89.66 | 89.04 | 85.42 | 3.62 | 86.04 |
| 1960–1 | 93.13 | 94.27 | 90.55 | 3.72 | 89.41 |
| 2 | 91.35 | 91.66 | 87.84 | 3.82 | 87.53 |
| 3 | 89.52 | 89.22 | 85.30 | 3.92 | 85.60 |
| 4 | 86.12 | 85.21 | 81.19 | 4.02 | 82.10 |

$$Wh_t = 7.307 + .869R_t + .199t \quad \bar{R}^2 = .9163.$$

$$(4.408) \quad (.043) \quad (.010)$$

We have examined the 11 industries under study and found six of these subject to a bias drift. Electrical Machinery has been drifting upward since 1951-3, Food and Beverages since 1948-1, Stone, Clay and Glass since 1950-3, Textile Mill Products since 1957-1, Paper and Allied Products since 1951-1, and Public Utilities since 1950-3. We assume that the remaining industrial sectors in our sample are not subject to drift.

Let us stop here to make some observations. We have found in all eleven industrial sectors studied, only one, Textile Mill Products, in which the bias drift conforms to previous suppositions about the nature and extent of the drift in the Aggregate Wharton Index. Only in Textile Mill Products do we find a significant upward drift in the Wharton Index commencing in the period after 1957. In one other industry, this drift begins as far back as 1948 and seems to originate for the most part in the period around 1951 to 1953. This, of course, is the Korean War period. Our results suggest that peaks in the years subsequent to the Korean War period have not been full capacity peaks. This result seems plausible; after 1951-1953 the economy's rate of unemployment has never fallen to the level reached in 1951-1953, even in the 1955-56 period.

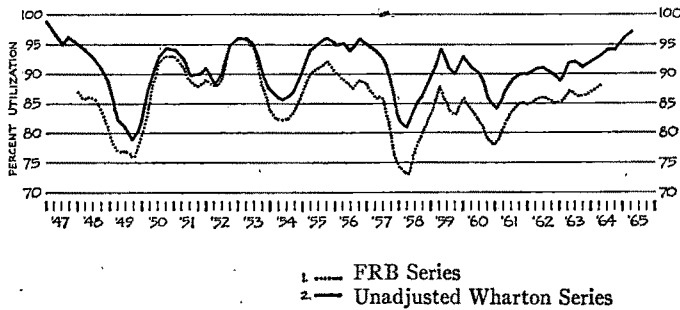
X. The Adjustment of the Aggregate Index

In Table 3 we present the results of adjusting the Aggregate Wharton Index. Column one is the 30-sector weighted average (using F.R.B. weights) of the original Wharton Component Sector Indexes. This is the series from which we wish to eliminate the bias drift. Column two is an 11-sector weighted average of the unadjusted Wharton Indexes under study. Column three is the 11-sector weighted average of the Wharton Sector Indexes with zero adjustment in five and adjustment by the amount $-ct$ over the period during which the other six were found to be subject to a biased drift.

On inspecting column (4), which is the empirical counterpart of expression (17), it is apparent that by 1953 the Wharton Index is one point too high, and by 1960 this drift has increased to 4.02 points. In terms of the Aggregate Index, the 1953 peak is now at 95 compared to 96, the 1956 peak is now at 93 compared with 96 and the controversial 1960 peak is now at 89 compared with 93.

It is now clear that the Wharton bias may have been more the result of a long term buildup due to the fact that the 1956 and 1960 peaks were not as high as the 1951-1953 peak, rather than the result of some short-term phenomenon associated with the lack of a full cyclical recovery in

CHART 2. PER CENT UTILIZATION OF CAPACITY 1947-1965



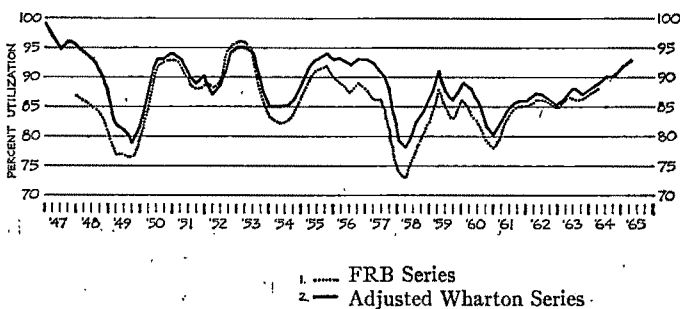
1960. This phenomenon probably intensified the drift, but our results place the roots of the bias earlier in the 1950's.

XI. *Comparison of the Adjusted and Unadjusted Wharton Series with Another Measure of Capacity Utilization*

Peter Gajewski and Frank de Leeuw have constructed a utilization of capacity series designed for comparison with the manufacturing components of the F.R.B. industrial production index. For comparative purposes and contrast with the Wharton Index their series can be called the F.R.B. series. Their series is based on three indicators, the Commerce Department's series measuring manufacturing capital stock in 1954 dollars, the McGraw-Hill index of manufacturing capacity, and the December-January averages of the Federal Reserve output index for manufacturing divided by McGraw-Hill estimates of the end-of-year "rates of operation." Details of the procedure used and a discussion of the results are given in [6]. We will deal with a comparison of the Gajewski-de Leeuw series, denoted by I_t , with the adjusted and unadjusted Wharton series.

Charts 2 and 3 give a graphic display of the adjusted and unadjusted Wharton Index in comparison with the I_t series. In Charts 2 and 3,

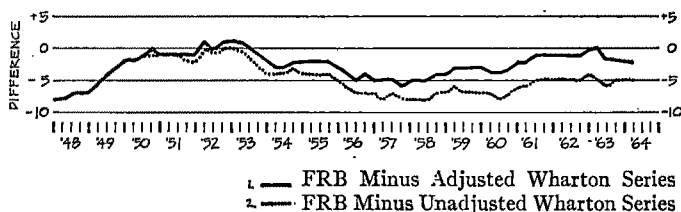
CHART 3. PER CENT UTILIZATION OF CAPACITY 1947-1965



series 1 is the I_t Index. The unadjusted Wharton Index is found in Chart 2 as Series 2. The adjusted Wharton Index is Series 2 in Chart 3.

It is apparent by inspection of these two charts that the Wharton series, either adjusted or unadjusted, agrees in all turning points with the Gajewski-de Leeuw series. The cyclical pattern is extremely close. By comparing Chart 2 with Chart 3, the bias drift of the Wharton Index, in relation to the series I_t , is not only apparent, but also there is a clear indication that this drift has been reduced by our adjustment method. Another view of this is also given Chart 4. Here we see the difference $I_t - Wh_t^A$ and the difference $I_t - Wh_t$. The second difference is series 2. Series 2 has a clear negative trend in it. This suggests that the unad-

CHART 4. DRIFT IN THE DIFFERENCE 1948-1964



justed Wharton series has been drifting away from the I_t series. This is supported by the regression result

$$Wh_t - I_t = 2.6798 + .0559t \quad R^2 = .1583.$$

(.6375) (.0156)

The coefficient of t is positive and significant. The difference which results after the adjustment of the Wharton index shows no clear trend. This is supported either by inspection of Chart 4, Series 1, or the regression result

$$Wh_t^A - I_t = 3.445 - .0243t \quad \bar{R}^2 = .0254.$$

(.558) (.0149)

Here the coefficient of t is of the wrong sign and is barely significant suggesting that in relation to the series I_t the new Wharton Index is not subject to any bias drift.

XII. Conclusions

We feel compelled to stress in bare form and all together the main bold assumptions that we have made in this paper. It is a strong assumption to claim that capital and labor are utilized, by industry, at the same

rate; that our method of allocating labor force at full capacity among industries is correct; that firms minimize cost in a competitive factor market; that production follows the Cobb-Douglas law; and that our data properly measure the relevant economic concepts.

Some of these assumptions can be relaxed. Possibly other proxies for capital utilization can be tested, but it is unlikely that we shall actually be able to obtain direct estimates of capital utilization for most sectors. We do contemplate testing alternative laws of production—certainly the CES function. At the same time it will be possible to eliminate some of the effects of least squares bias in parameter estimation of production functions by estimating industry equations for cost minimization and production simultaneously. We can also introduce distributed lag adjustment processes that may improve our results generally, while enabling us to distinguish between short- and long-run capacity.

Data are continuously improving. In the future we should be able to make better industry estimates and to cover more industries.

In spite of these caveats and others that could be drawn, we feel that assumptions and procedures like ours must be followed if any results at all are to be obtained and that our results are plausible. We judge plausibility by the uses to which we have been able to put the measures. As general cyclical measures, our estimates have served well in investment and profit functions that are part of econometric models. It appears to be the case that our series give cyclical information in econometric equations that is not given by unemployment and other cyclically related series.

As to a more basic use of these series for gauging the long-run economic potential of the American economy, we are less definite that our series are the correct ones. Here we rely on arguments of intuitive plausibility. We feel that if the more extreme estimates that show utilization rates considerably below ours were to be raised to full capacity, the resulting targets would be mutually incompatible and highly inflationary.

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STATE DIFFERENTIALS IN INCOME CONCENTRATION

By AHMAD AL-SAMARRIE AND HERMAN P. MILLER*

I. Introduction and Summary

The degree of income concentration varies considerably from state to state. The index of concentration for Mississippi, which has the most unequal distribution of income in the nation, is 50 per cent greater than for Utah, the state with the most equal distribution. Within this range, there are marked differences in income concentration among states which are systematically related to various factors that have a bearing on income distribution—stage of economic development; the degree of industrialization and urbanization; the industrial and occupational mix; resource endowments; the size, skill, age, sex, and race of the labor force; the rate of unemployment; etc. The primary purpose of this paper is to analyze the relationship between these economic variables and the degree of income concentration among states.

There is a long history of speculation among economists and statisticians about the reason for the skewness of the income curve which is often referred to as income inequality or concentration. The theories that have been developed to explain this phenomenon generally fall into two categories, "natural" and "institutional."¹ The former typically attempt to explain income distribution in terms of probability theory. In this sense they treat the income curve as a by-product in the play of natural forces like the distribution of ability and chance which are independent of the form of social or economic organization. These theorems have ranged from a view of the income curve as a joint probability distribution of biological traits transmitted by heredity [2] to the analysis of the curve in terms of a game of chance under certain specified conditions [7] [1] [21].

*The authors are, respectively, economist, Center for Economic Projections, National Planning Association, and chief, Population Division, Bureau of the Census. This paper is based on an unpublished doctoral dissertation by Ahmad Al-Samarrie, The George Washington University, February 1966. The underlying statistics are based in part on unpublished Census Bureau data developed jointly by the authors. To the many persons who gave advice and criticism we wish to express our appreciation. Special thanks are due to Professor Charles Todd Stewart, Jr., of The George Washington University, as well as to members of the National Planning Association staff, particularly Gerhard Colm, Sidney Sonenblum, and Joel Darmstadter.

¹One may add a third category which attributes the income differences among individuals to "consumer preferences" with respect to the willingness to assume risks [5] or to the choice of occupations [17].

The institutional approach attempts to explain income inequality in terms of the social and economic conditions which prevail at a given time and place. The unique stamp of these theories is the emphasis they place on institutional arrangements which they claim help to perpetuate inequality once it is established. An earlier generation of economists represented by Taussig [22, p. 246] and Pigou [20, pp. 650-51] placed great emphasis on the importance of inherited property as well as inherited capacity as causal factors of income inequality. Later empirical studies have supported their view that there is no "law" of income distribution. During the past 30 years major reductions of income inequality have taken place in the United States largely in response to changes in social and economic conditions [12] [8] [15, Ch. 8] [14].

Due to the lack of empirical data, little work has been done to explain why incomes are more concentrated in some areas than others or how and why the degree of concentration for specific localities has changed over time.² Information collected in the past two decennial censuses makes it possible to test various hypotheses regarding the relationship between the degree of income concentration and various socio-economic factors. In this paper an attempt is made to explain state-to-state variations in income concentration by means of a multiple regression analysis involving five variables: the functional distribution of income measured by the per cent of property income in the state personal income; the relative weight of agriculture in the state economy measured by the per cent of total income originating in agriculture; the quality of the labor force measured by median school years completed by adult persons; imperfections in the labor market arising from racial discrimination as measured by the per cent of non-white persons in the population of each state; and the extent of labor participation in the state economy as measured by the per cent of the civilian population employed. In 1949 and 1959 over five-sixths of the state-to-state variation in income concentration was accounted for by these five variables.

Among the variables tested, racial composition appears to be the most important factor in explaining inter-state variations in income

² Simon Kuznets [13] gave some attention to this subject. Based on the state data supplied by Seymour Goodman, Kuznets finds a clear association between the shares of the upper income groups and the level of per capita income—the shares are distinctly higher in low income than in high income states, reflecting similar differences in intersectoral inequality in participation income per worker. Almost the same conclusion emerges when he compares income inequality among the thirteen regions of Italy as well as among countries; in other words, the shares of the upper income groups are higher in less developed countries (regions) than in developed ones.

For international comparisons of income size distribution, see [11] [19].

concentration. Due perhaps to the relatively large outmigration of Negroes from the South, this factor was less important in 1959 than in 1949. The probable continuation of this migration trend along with the recent drive toward social and economic equality for nonwhites may mean a further reduction in the relative importance of racial composition as a factor in income inequality.

The per cent of income originating in agriculture accounted for a smaller proportion of the variance in income concentration from state to state in 1959 than in 1949. This reflects, *inter alia*, the marked decrease in the role of agriculture as a source of income and employment during the postwar years. A further reduction in the role of agriculture in future years may eventually eliminate this factor as an explanatory variable in income concentration.

With the continued dispersion of nonwhites throughout the country and the declining role of agriculture in the economy, these two variables should decline in importance as explanatory factors in income concentration. It is difficult to predict which of the remaining factors will increase in relative importance or whether new variables will enter the scene. It seems likely, however, that the level of education will increase in importance as an explanatory variable since it is now highly correlated with income concentration and seems to be increasing in importance as a determinant of income.

The analysis presented here is based on the 1950 and 1960 Census of Population reports for each of the 48 states and the District of Columbia which show the distribution of families by total money income before taxes. The measure of income inequality used is the Gini concentration ratio.³

Before proceeding with the analysis note should be made of several limitations of the data which require that the findings be interpreted with care. First, the income distribution in each state is based on a single year rather than the average of several years. Year-to-year fluctuations in the incomes of families are wide due to temporary or "transient" factors (unforeseen additions to or subtractions from income), which are assumed to cancel out over a long period of time, and to be uncorrelated with "permanent" income [9, pp. 155-57] [6, Ch. 3].

³ The Gini concentration ratio is a measure of income concentration that is derived from the Lorenz curve which is obtained by plotting the cumulative per cent of units (families) on the *X* axis against the cumulative per cent of aggregate income accounted for by these units on the *Y* axis. If all units had exactly the same incomes, the Lorenz curve would be represented by a diagonal drawn from the point of origin. Curves drawn to actual data invariably fall below this line and the greater the inequality in the distribution of income the greater the area between the diagonal line and the Lorenz curve. The Gini concentration ratio is defined as the proportion of the total area under the diagonal that is between the diagonal and the Lorenz curve. For further information and a diagrammatic illustration see [18].

The use of a single reference year for comparing income inequality may exaggerate the degree of inequality.⁴

Second, the data refer to the distribution of money income among families in each state. As such, they take no account of imputed (non-money) income components, and for differential price (cost-of-living) indexes among geographical units, both of which are pertinent to welfare considerations. There is reason to believe that the exclusion of these factors, particularly of differential cost-of-living indexes, tend, on the average, to exaggerate the income inequality of the agricultural and less urbanized states. Finally, the state income distribution data convey no information on the family size. Such information is relevant to interstate comparison of real income distribution because differences in income concentration among states may partly reflect variations in family size and composition.

II. *Interstate Differences in Income Concentration*

Table 1 shows a ranking of the states by the size of Gini's concentration ratio of pre-tax money income received by families in 1949 and 1959. The states are arranged in an "increasing" order of the inequality coefficient in 1949, i.e., the state with the smallest concentration ratio in 1949 is ranked first and the state with the highest ratio is ranked 49th.

Several inferences may be drawn from this table.

1. With few exceptions, the states with a high degree of industrialization and a high standard of living (as measured by per capita income) had a more equal distribution of income than the relatively low income states (the Southern and predominantly agricultural states). This fact is clearly shown in Table 2, where the states are divided into six groups based on the size of the concentration ratio. The mean (average) concentration ratio is computed for each group and compared with the corresponding average per capita personal income. Except for group II in 1949 and group III in 1959, the inverse relationship between the concentration ratio and the level of per capita personal income is continuous. In other words, per capita income decreases as we

⁴When comparing the income size distributions of different spatial units, the crucial issue is not so much whether there are undue variations in the incomes of various income groups as whether or not there are distinct interstate differences in the year-to-year fluctuations in the incomes of these groups. There are unfortunately no data that shed light on this question. One can conjecture, however, that the incomes of farmers, nonwhites, and older persons are subject to greater short-run variations than those of other income groups. Hence, the states which have more than proportionate shares of these groups (the Plains and the Southeastern States) may tend to have above average fluctuations in income, and thus a relatively exaggerated income inequality.

TABLE 1—GINI'S CONCENTRATION RATIO OF BEFORE-TAX MONEY INCOME RECEIVED
BY FAMILIES IN EACH STATE, 1949 AND 1959^a

| States | Concentration Ratio | | Rank ^b | |
|-------------------|---------------------|------|-------------------|------|
| | 1949 | 1959 | 1949 | 1959 |
| United States | .405 | .371 | — | — |
| Utah | .340 | .312 | 1 | 1 |
| Michigan | .351 | .334 | 2 | 12 |
| Washington | .354 | .329 | 3 | 4 |
| Massachusetts | .356 | .327 | 4 | 3 |
| New Jersey | .360 | .334 | 5.5 | 12 |
| Ohio | .360 | .330 | 5.5 | 6 |
| New Hampshire | .362 | .319 | 7.5 | 2 |
| Wisconsin | .362 | .336 | 7.5 | 14 |
| Pennsylvania | .363 | .339 | 9 | 16.5 |
| Connecticut | .365 | .331 | 10.5 | 8.5 |
| Indiana | .365 | .339 | 10.5 | 16.5 |
| California | .366 | .345 | 12 | 21 |
| Rhode Island | .367 | .332 | 13 | 10 |
| Oregon | .369 | .330 | 14.5 | 6 |
| Wyoming | .369 | .334 | 14.5 | 12 |
| Nevada | .371 | .331 | 16 | 8.5 |
| Illinois | .375 | .348 | 17 | 22 |
| Minnesota | .378 | .362 | 18 | 25.5 |
| Idaho | .381 | .338 | 19 | 15 |
| Iowa | .384 | .372 | 21 | 30 |
| Maryland | .384 | .349 | 21 | 23 |
| Vermont | .384 | .343 | 21 | 18 |
| Maine | .386 | .330 | 23 | 6 |
| New York | .389 | .252 | 24 | 24 |
| Montana | .390 | .344 | 25 | 19.5 |
| West Virginia | .395 | .397 | 26 | 36 |
| Washington, D. C. | .396 | .395 | 27 | 35 |
| Colorado | .398 | .344 | 28 | 19.5 |
| Nebraska | .404 | .371 | 29 | 28.5 |
| Kansas | .412 | .362 | 30 | 25.5 |
| North Dakota | .414 | .373 | 31 | 31 |
| South Dakota | .415 | .391 | 32 | 34 |
| Arizona | .424 | .369 | 33 | 27 |
| Delaware | .425 | .371 | 34 | 28.5 |
| Missouri | .427 | .386 | 35.5 | 33 |
| Virginia | .427 | .398 | 35.5 | 37 |
| Oklahoma | .443 | .403 | 37 | 39.5 |
| North Carolina | .445 | .415 | 38.5 | 41 |
| Texas | .445 | .403 | 38.5 | 39.5 |
| New Mexico | .446 | .379 | 40 | 32 |
| Florida | .453 | .399 | 41 | 38 |
| Kentucky | .454 | .425 | 42 | 47 |
| Tennessee | .459 | .424 | 43 | 45.5 |
| Louisiana | .460 | .420 | 44 | 43 |
| South Carolina | .467 | .421 | 45 | 44 |
| Georgia | .474 | .418 | 46 | 42 |
| Alabama | .475 | .424 | 47 | 45.5 |
| Arkansas | .491 | .437 | 48 | 48 |
| Mississippi | .526 | .466 | 49 | 49 |

^a Calculated from: U.S. Department of Commerce, Bureau of the Census, *United States Census of Population: 1950*, General Characteristics, Part 1 to 50, Table 32; and *United States Census of Population: 1960*, Series PC(1)-1C to 53C, General, Social and Economic Characteristics, Table 65.

^b The states are ranked in an increasing order of Gini's concentration ratio in 1949.

TABLE 2—PER CAPITA PERSONAL INCOME, GROUPS OF STATES BY ASCENDING ORDER OF GINI'S CONCENTRATION RATIO, 1949 AND 1959

| | Groups of States by Size of Concentration Ratio | | | | | |
|---------------------------------|---|-------|-------|-------|-------|-------|
| | I | II | III | IV | V | VI |
| 1949 | | | | | | |
| Gini's concentration ratio | .3585 | .3684 | .3845 | .4072 | .4389 | .4758 |
| Personal income per capita (\$) | 1463 | 1606 | 1384 | 1319 | 1252 | 964 |
| 1959 | | | | | | |
| Gini's concentration ratio | .3284 | .3358 | .3484 | .3729 | .4001 | .4294 |
| Personal income per capita (\$) | 2298 | 2180 | 2290 | 2050 | 1872 | 1428 |

Sources: Gini's concentration ratio is from Table 1. Personal income per capita is from U.S. Dept. of Commerce, Office of Business Economics, *Personal Income by States Since 1929*, Suppl. to *Surv. Cur. Bus.*, Washington, 1956, Table 2, p. 142; and *Surv. Cur. Bus.*, Aug. 1962, 42, Table 2, p. 11.

move from group I (the states with the lowest concentration ratio) to group VI (the states with the highest ratio).⁵

2. There was a decrease in the degree of income inequality for every state except West Virginia over the 1949-59 decade (intrastate decrease in the inequality coefficient). Part of the observed intrastate decline in the inequality coefficient during the 'fifties may be due to changes in the methods of collecting and processing the income data in the 1950 and 1960 Censuses.⁶

3. There was a considerable reranking of the states in the inequality coefficient between 1949 and 1959. Several of the agricultural states (notably Maine, New Hampshire, Oregon, Idaho, Montana, Colorado, and Kansas) moved to lower ranks (experienced greater than average decline in the concentration ratio) during this period. On the other hand, the states with a high degree of industrialization or urbanization—Michigan, New Jersey, Pennsylvania, Indiana, California, Illinois, and D.C.—moved to higher ranks among the states.⁷ With few excep-

⁵ In a study of the relationship between income distribution and taxation Thomas Hopkins also found an inverse correlation between state and income concentration and median income [10].

⁶ The ratio of decennial census money income to the corresponding national income estimated by the Department of Commerce was about 83 per cent in 1949 compared with about 95 per cent in 1959. The greater understatement of income in 1949 than in 1959 was largely due to the manner in which income was estimated. In the 1950 Population Census income information was obtained for the head of the family and for all other family members *as a group*. If income was reported for the head of the family, that amount was used as the total income for the family when no information was obtained for other family members. In the 1960 Census income was obtained separately for each member of the family. When income was not reported for a family member, imputations were made based on persons with the same characteristics—see [16].

⁷ The smaller than average decrease in the income inequality of Michigan and Pennsyl-

tions there was little change in the relative ranks of the southern states during the decade.

4. There was a moderate decrease in the "inter state" differences in the concentration ratio between 1949 and 1959. Table 3 summarizes the changes in the interstate dispersion of the Gini concentration ratio in 1949 and 1959. As can be seen, the standard deviation of state concentration ratio declined from about .043 in 1949 to .037 in 1959, or

TABLE 3—INTER STATE DISPERSION IN THE CONCENTRATION RATIO
OF INCOME RECEIVED BY FAMILIES, 1949 AND 1959

| Interstate Dispersion in the Concentration Ratio | Year | |
|---|------|------|
| | 1949 | 1959 |
| Mean ratio (\bar{x}) | .404 | .368 |
| Standard deviation (σ) | .043 | .037 |
| Coefficient of variation (v) | .106 | .102 |

about 13 per cent. The comparable decline in the coefficient of variation was nearly 5 per cent during the same period. These figures suggest that the states with relatively high concentration ratio in 1949 tended to show somewhat larger decreases in income inequality than the states with relatively low 1949 concentration ratio.

III. *Factors Associated with State Differentials in Income Concentration*

As previously noted, there are marked differences among states in various characteristics that are related to income concentration. States in which a large proportion of income is derived from property are more likely to have a greater concentration of income than those which are less dependent upon this type of income. States in which a large proportion of the population is uneducated or is engaged in low-paid employment are also likely to have a greater concentration of income than those in which education and high-paid employment are more widespread. In this study an attempt is made to examine the relationship between income concentration and various socio-economic characteristics by the use of multiple correlation-regression analysis.

In the analysis which follows, the relationship between the dependent variable (Y) and various independent variables ($X_1, X_2 \dots$) is defined by the equation:

$$Y = a + b_1X_1 + b_2X_2 \dots b_mX_m.$$

vania may have been due to the relatively high unemployment rates in 1959; the same phenomenon may also explain the slight decrease in the inequality of West Virginia's income distribution.

In this equation the dependent variable Y is the Gini concentration ratio for 1959; the independent variables $X_1 - X_{10}$ are described below; a is a constant, and $b_1, b_2 \dots b_m$ are the net regression coefficients.⁸

Y = Gini's concentration ratio of the size distribution of income received by families in each state in 1959.

X_1 = Property income as per cent of state personal income, 1959.

X_2 = Per employee labor income in agriculture as a ratio of per employee labor income in nonagricultural industries, 1959.

X_3 = Labor earnings in agriculture as per cent of total labor earnings from all industries, 1959.

X_4 = Per employee labor income in non-agricultural industries, adjusted for state industry structure, 1959.

X_5 = Per cent of labor force employed as clerical workers, sales workers, or craftsmen, foremen, and kindred workers in each state, 1960.

X_6 = Median school years completed by persons 25 years and over in each state, 1960.

X_7 = Percentage share of the age groups 35-64 years in the labor force of each state, 1960.

X_8 = Per cent of total families headed by females in each state, 1960.

X_9 = Per cent of nonwhite persons in the resident population of each state, 1960.

X_{10} = Civilian employment as a per cent of the civilian population of each state, 1960.

The ten independent variables outlined above were tested in a regression analysis with the state concentration ratio of family income size distribution in 1959. The results are summarized in Table 4.

1. The coefficient of multiple correlation with the ten factors on Y is .927, and the coefficient of determination is .860. This means that the ten so-called "independent" variables account for about 86 per cent of the state-to-state variation in the income inequality in 1959. The unexplained variation of roughly 14 per cent may be due to imperfect measurement, the use of an inappropriate regression equation (linear instead of curvilinear), and/or the exclusion of other relevant independent variables.

2. Measured by the t -values (i.e., the partial regression coefficients divided by their respective standard errors of estimate), only four variables are significantly associated with the dependent variable at the 99 and/or 95 per cent confidence level. These are: the per cent of total earnings originating in agriculture (X_3), the level of education (X_6), the per cent of population that is nonwhite (X_9), and the per cent of the civilian population that is employed (X_{10}). Measured by the "beta"

⁸ Two basic assumptions underlie the statistical analysis: (a) the forty-eight states and D.C. constitute the entire universe in 1959; and (b) the effect of different factors on the independent variables is assumed to be a linear relationship.

coefficients, X_9 is the most important factor in explaining the interstate variations in the concentration ratio (+.44), followed by X_3 (+.38), X_{10} (−.32), and X_6 (−.29).⁹ The other variable with a relatively high t -value is the per cent of property income in the state total personal income (X_1).

3. A close examination of the correlation matrix and the beta co-

TABLE 4—PARTIAL REGRESSION AND CORRELATION COEFFICIENTS, STATE CONCENTRATION RATIO AND RELATED VARIABLES, 1959

| Variables | Partial Regression Coefficients | Standard Error of Regression Coefficients | Partial Correlation Coefficients | Beta Values |
|-----------|---------------------------------|---|----------------------------------|-------------|
| X_1 | +.1673 | (.1245) | +.2131 | +.1098 |
| X_2 | −.0333 | (.0289) | −.1843 | −.1327 |
| X_3 | +.2530* | (.1280) | +.3057 | +.3816 |
| X_4 | −.0139 | (.0371) | −.0608 | −.0650 |
| X_5 | +.1096 | (.2864) | +.0620 | +.1127 |
| X_6 | −.9756* | (.4789) | −.3138 | −.2858 |
| X_7 | −.1121 | (.1429) | −.1262 | −.0646 |
| X_8 | +.2538 | (.2611) | +.1557 | +.1563 |
| X_9 | +.1398** | (.0433) | +.4641 | +.4408 |
| X_{10} | −.4197** | (.1303) | −.4631 | −.3150 |

Intercept (A value) = .6585**

Standard error of intercept = (.1520)

Multiple Correlation Coefficient

R = .9271

Coefficient of determination

R^2 = .8595

Standard error of estimate

S_e = .0159

F -value = 23.25**

* Significant at the 5 per cent level.

** Significant at the 1 per cent level.

efficients indicates that multicollinearity exists among the explanatory variables and that the condition of independence does not hold. Table 5, which presents the simple (zero-order) correlation coefficients among all the variables, shows that there is high correlation among many of the variables. For example, the states in which the per employee compensation in agriculture is relatively high compared to the per employee com-

⁹ The beta coefficients are obtained by multiplying the net regression coefficients by the ratios of the standard deviation of the different independent variables to the standard deviation of the dependent variable. By reducing the net regression coefficients to a common denominator, the beta values enable one to say which independent factor is the more important in explaining variations in the dependent variable. For example, a beta value of a +.44 for X_9 may be taken to mean that with an increase of one standard deviation in the per cent of families headed by nonwhite persons, the concentration ratio increases .44 of one standard deviation.

For a fuller treatment, see [3, pp. 773-74] [4].

TABLE 5—COEFFICIENTS OF SIMPLE CORRELATION AMONG ALL THE VARIABLES, 1959

| | X_1 | X_2 | X_3 | X_4 | X_5 | X_6 | X_7 | X_8 | X_9 | X_{10} | Y |
|----------|-------|--------|--------|--------|---------|---------|--------|--------|---------|----------|--------|
| X_1 | — | | | | | | | | | | |
| X_2 | | +.2575 | -.1007 | +.2728 | +.3995 | +.4519 | +.3434 | -.0102 | -.1569 | +.5730 | -.3381 |
| X_3 | | | +.4333 | +.2382 | +.1016 | +.6842* | -.0175 | -.5379 | -.5294 | +.1990 | -.5178 |
| X_4 | | | | -.2943 | -.7074* | -.0076 | -.3627 | -.5132 | -.2203 | -.2845 | +.1901 |
| X_5 | | | | | +.7919* | +.4264 | +.3444 | -.3322 | -.4782 | +.1579 | -.5463 |
| X_6 | | | | | | +.5083 | +.4850 | +.0227 | -.2485 | +.4710 | -.6091 |
| X_7 | | | | | | | +.1281 | -.4511 | -.4766 | +.5117 | -.7504 |
| X_8 | | | | | | | | -.0049 | -.2783 | +.3949 | -.4151 |
| X_9 | | | | | | | | | +.8236* | +.1395 | +.5034 |
| X_{10} | | | | | | | | | | -.0223 | +.7029 |
| Y | | | | | | | | | | | -.5040 |

* Highly significant.

pensation in nonagriculture (X_2) tend to be those in which the median years of schooling completed by adult persons are also high (X_6). Similarly, there is a negative association between the relative importance of agriculture in the state economy (X_3) and the proportion of persons in the skilled and white collar occupations (X_5).

Given the presence of multicollinearity, all the coefficients and the statistical test may be biased as a result. The standard practice in such a situation is to delete some of the weak explanatory variables, provided of course there are no serious theoretical objections to their exclusion. In this study, five variables were excluded from the analysis— X_2 , X_4 ,

TABLE 6—PARTIAL REGRESSION AND CORRELATION COEFFICIENTS, STATE CONCENTRATION RATIO AND SELECTED EXPLANATORY VARIABLES, 1959

| Variables | Partial Regression Coefficients | Standard Error of Regression Coefficients | Partial Correlation Coefficients | Beta Values |
|-----------|---------------------------------------|---|--|----------------|
| X_1 | + .1789 | (.1130) | + .2347 | + .1173 |
| X_3 | + .1620** | (.0420) | + .5058 | + .2443 |
| X_6 | -1.2387** | (.2828) | - .5555 | - .3629 |
| X_9 | + .1888** | (.0227) | + .7857 | + .5954 |
| X_{10} | - .4035** | (.1107) | - .4858 | - .3028 |

Intercept (A value) = .5905**

Standard error of intercept = (.0336)

Multiple Correlation Coefficient R = .9220

Coefficient of determination R^2 = .8501

Standard error of estimate S_e = .0155

F -value = 48.78**

** Significant at the 1 per cent level.

X_5 , X_7 , and X_8 .¹⁰ The correlation of the remaining five factors with the dependent variable (the concentration ratio) is summarized in Table 6. Several inferences may be drawn from this table:

(a.) There was a very insignificant decrease in the coefficient of determination as a result of the exclusion of the given variables listed above—from .859 to .850. From a statistical point of view, this suggests that the factors related to the industrial affiliation of income earners (X_2 , X_4 , and X_5), and those related to the age and sex of the state labor force (X_7 , and X_8 , respectively), play a minor part in the explanation of state-to-state variation in the concentration of income.

(b.) The deletion of several variables produced a marked increase

¹⁰ The exclusion of these particular variables may be justified on two grounds: (1) they showed extremely low degree of correlation with the dependent variables, as evidenced by the relatively low t -value, partial correlation coefficients, or the beta values; and (2) they were significantly associated with the other independent variables, as indicated above.

in the relative importance of all the retained variables, particularly those relating to education (X_6) and to the proportion of nonwhite persons (X_9). This is clearly shown in the size of the partial correlation coefficients and, to a lesser extent, in the values of beta coefficients. The increase in the relative importance of X_6 was due primarily to the deletion of X_2 and X_5 , variables with which it has significant positive correlation. By the same token, the increase in the relative importance of X_9 was due primarily to the exclusion of X_8 , with which it has a positive association.

(c.) Compared to the initial situation, and using the beta coefficients as a frame of reference, the level of education (X_6) replaced the percentage share of the total earnings originating in agriculture (X_3) as the second most important factor in state-to-state variations in income inequality. There was no change in the relative ranks of race and per capita civilian employment. Although there was a moderate increase in the relative importance of property income (X_1), its association with

TABLE 7—PARTIAL REGRESSION AND CORRELATION COEFFICIENTS, STATE CONCENTRATION RATIO AND SELECTED EXPLANATORY VARIABLES, 1949

| Variables | Partial Regression Coefficients | Standard Error of Regression Coefficients | Partial Correlation Coefficients | Beta Values |
|-----------|---------------------------------------|---|--|----------------|
| X_1 | -.4031** | (.1214) | +.4517 | +.2409 |
| X_3 | +.1640** | (.0300) | +.6430 | +.3444 |
| X_6 | -.9757** | (.2728) | -.4788 | -.2455 |
| X_9 | +.2570** | (.0246) | +.8471 | +.6900 |
| X_{10} | -.4706** | (.0985) | -.5887 | -.3313 |

Intercept (A value) = .5770**

Standard error of intercept = (.0367)

Multiple Correlation Coefficient R = .9337

Coefficient of determination R^2 = .8719

Standard error of estimate S_e = .0164

F -value = 58.52**

** Significant at the 1 per cent level.

the state concentration ratio continued to be statistically insignificant.

The above analysis was repeated for 1949 using the variables previously described. The results, shown in Table 7, are basically similar to those obtained for 1959. The coefficient of multiple correlation for the five factors was .933 for 1949 as compared with .922 for the following decade. Several differences, however, may be noted in the relative importance of several of the explanatory variables.

1. The association between property income (X_1) and income concentration was positive in both years, but it was significant only in 1949. The decline in the importance of this variable is partly due to a

narrowing of state differentials in the proportion of property income in relation to total income during the 'fifties, reflecting, *inter alia*, the freemovement of productive factors and industries among geographic units.

2. Agriculture (X_3) was more important in the explanation of interstate variation in income inequality in 1949 than in 1959. The marked decrease in the share of agriculture in the national economy (a consequence of a rapid pace of mechanization in agriculture and a relatively inelastic demand for agricultural commodities) was perhaps the main contributing factor.

3. There was a significant inverse correlation between educational attainment (X_6) and the concentration ratio in both years. Education, however, was a more significant explanatory variable in 1959 than it was ten years earlier, as a result perhaps of the general rise in educational attainment.

4. The racial composition of the population was the single most important explanatory variable in both years, although this factor was less significant in 1959 than in 1949. The decrease in the importance of this variable is due largely to the out-migration of nonwhites from the South.

5. The per cent of the population that was employed (X_{10}) was a significant explanatory variable in both years, although it was of greater importance in 1949 than in 1959. This change is probably due to the observed tendency for states to become more similar in the proportion of the civilian population employed during the ten-year period.

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A THEORY OF GROWTH WITH TECHNOLOGICAL CHANGE

By JAROSLAV VANEK*

A rather limiting feature of many of the formal growth models which have gained prominence—such as those of Professors Harrod [4], Domar [1], and Solow [6]—is that, to the extent that these models yield an equilibrium growth path, that growth path is one where per capita income either remains unchanged, or if it changes, it does so only because of an increasing efficiency of labor. In other words, if there is to be an increase in per capita income, this is so because in the production function a factor reflecting technical change (depending on time or otherwise) appears only as a term multiplying the labor input. The purpose of this paper is to explore a different growth theory—about which comparatively little is known¹—incorporating technical progress saving capital and labor in equal proportions (i.e., Hicks-neutral progress) and growth of productive resources.² At the same time, the theory presented here includes some of the other theories as special cases.

While what follows is certainly not an adequate description of the infinitely complex process of growth and development, nonetheless it is hoped that it may clarify some aggregative aspects of that process. In fact, as we will see in Section 4, the theory seems to be consistent with a number of key trends usually observed in growing economies, and can provide the rationale for several basic relationships among these trends.

I. *Definition of the Growth Model*

The growth theory we want to explore in this paper is exceedingly simple. It is a combination of the Solow growth model (with constant

* The author, professor of economics at Cornell University, expresses his grateful appreciation to his colleague Ta-Chung Liu for useful discussion of the subject treated in this paper. An outgrowth and further generalization of the theory presented in this article appeared in the *Economic Journal* of December 1966.

¹ For evidence of this see [3].

² At this point I should like to acknowledge an interesting contribution by C. E. Ferguson [2], which appeared after this paper was first submitted for publication. Starting from its definition, Ferguson derives the natural rate of growth of income for the Hicks-neutral growth situation. His result is the same as that obtained in this paper (relation [11]) for the "asymptotic" rate of growth of income. Ferguson also derives a number of other interesting expressions for the natural rate of growth which supplement the analysis of this paper.

Unfortunately, the relevance of Ferguson's natural rate for the general Hicks-neutral theory of growth is not made clear in his article. As we show later in this paper, in the general Hicks-neutral case (excluding that involving a Cobb-Douglas function, which is also Harrod-neutral), the actual rate of growth of income cannot be equal to the natural rate except possibly in isolated instances of infinitesimal duration. Under such conditions, the natural rate can be of interest to us only if we can learn from it something about the actual growth rates.

technology) [3] and the assumption of neutral technological progress, proceeding at a constant rate. The equations defining the model are as follows:

$$(1) \quad Y = Ae^{at}F(K, N)$$

where Y represents aggregate net output, A is a given constant, a is the constant rate of neutral technological progress, t is time measured from the base period, and K and N capital and labor inputs respectively.³ F is assumed to be unit-homogeneous, so that we can rewrite relation (1) as

$$(2) \quad Y = Ae^{at}Nf(K/N)$$

F is subject to a diminishing and nonnegative marginal rate of substitution, except for possible rates zero or infinity.⁴ Unless otherwise specified later in our discussion, saving (S) is taken to be a constant proportion s of national output, so that

$$(3) \quad S = sY$$

Finally, we make the customary assumption that saving equals investment, that is,

$$(4) \quad S = dK/dt$$

Dividing both sides of relation (2) by N so as to obtain per capita income, differentiating that equation with respect to time and dividing by per capita income we obtain a fundamental relation between the rates of growth, namely

$$(5) \quad y = a + \phi(k - n)$$

where y is the rate of growth of per capita income, k that of capital, and n that of population. The term ϕ can either be interpreted as the share of total income earned (under fully competitive conditions) by capital, or as the elasticity of per capita income with respect to a change in the capital-labor ratio, that is,

$$(6) \quad \phi = \frac{df/d(K/N)}{f/(K/N)}$$

In relation (5) n is a prescribed parameter, and is assumed constant in most of our subsequent discussion.

³ Throughout this paper we assume full employment and no change in the quality of labor, or in the population structure. Consequently, N can be understood interchangeably as labor-input or as total population. K is total capital stock, having the same physical nature as national product, and infinite durability.

⁴ That is, except for the case where for some capital-labor ratios the isoquants become parallel to an axis.

Once n is given, relations (2) through (4) together with prescribed initial levels of the three variables K_0 , N_0 , and $t=0$ allow us to trace the growth path for both income (or per capita income) and the capital stock. In the following section we examine these growth paths, primarily with reference to alternative initial conditions, while in the subsequent section our principal purpose is the analysis of the asymptotic properties of these growth paths, that is, the analysis of the limits imposed on the

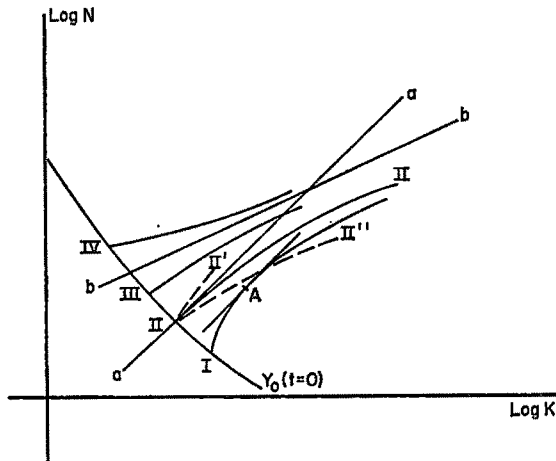


FIGURE 1

rates of growth (if any) as the initial conditions recede to an increasingly distant past.

II. Description of the Growth Path

The assumption of linear homogeneity of the function F simplifies our task of describing alternative growth paths corresponding to the theory just defined. We do not have to consider all the possible combinations of initial endowments (at time zero) of capital K_0 and labor N_0 . It suffices to select arbitrarily one equal-product line, corresponding to $t=0$ and $Y=Y_0$, and consider as initial conditions only points on that contour, marked by $Y_0(t=0)$ in Figure 1. Whatever growth path then is shown for these initial conditions must also, *ceteris paribus*, pertain to any other initial conditions (K_0 and N_0), the scales of the two axes being adjusted by a common factor, in such a way as to make the alternative initial conditions fall on the isoquant as drawn in the diagram.

It will be observed that both the K -axis and the N -axis have a logarithmic scale. With a rate of growth of N constant, equal to n , paths along a 45 degree line, such as aa , indicate a constant rate of growth of

capital k , equal to n , while straight lines flatter than aa , such as bb , indicate a constant rate of growth of capital greater than n , and vice versa, for lines steeper than aa . Moreover, convexity towards the N -axis indicates acceleration of the rate of growth of the capital stock, and the opposite convexity deceleration of that rate.⁵

Now consider first the initial conditions II, and study the implications of a growth path aa , comparable to the equilibrium growth path in the Solow model. It will be seen that such a growth path, on the assumptions stated in the preceding section—especially, the assumption of a constant saving rate—is impossible. With constant and equal rates of growth of labor and capital, national income will grow at the rate of $n+a$ (recall that a is the autonomous rate of neutral technological improvement), and saving would have to grow at that same rate; but this contradicts the postulate that capital grows only at the rate $n(=k)$.

In fact, the growth path aa is possible only if s gradually declines over time at a negative rate. Specifically, s would have to be the following function of time:

$$(7) \quad s(t) = \frac{ne^{-at}K_0}{Af(K_0/N_0)N_0}$$

This situation, characterized by steady growth and proportionality of K and N , certainly is not a good description of what happens in the real world. Rather, it strongly suggests that with technological progress, in a world where the saving rate is relatively stable, capital and labor cannot grow at the same rate.

Nevertheless, the result summarized by equation (7) has a relevance for our analysis. Corresponding to initial conditions K_0 and N_0 at point II, there will be, from relation (7), a saving rate s_0

$$(7a) \quad s_0 = \frac{nK_0}{Af(K_0/N_0)N_0}$$

leading to the result that, at time zero, the growth path will have the slope of aa , that is, k will be equal to n . If that saving rate (s_0) is preserved, however, k must increase over time. Thus we obtain the "initial" segment of one growth path, marked II-II in Figure 1. As is indicated by its upward convexity, the rate of growth of capital must be accelerating in the early stages of expansion. On the other hand, it is self-evident that for constant saving rates greater than s_0 we will obtain initial growth paths such as II-II'' in the diagram. For saving rates falling short of s_0 , on the other hand, we have paths such as II-II'. If the two alternative saving rates are not too different from s_0 the convexity of

⁵ Note also that although the isoquant $Y_0(t=0)$ was drawn convex to the origin, this is not a necessary pattern in a double-logarithmic plane.

these two paths can be expected to be as indicated in Figure 1, that is, an acceleration of the rate of growth of capital is likely to take place in the neighborhood of point II. However, as we will see presently, this pattern can be reversed for high levels of saving rates.

Let us now briefly consider alternative "initial" labor and capital endowments. Suppose that the saving rate is now fixed at s_0 , so that we already have a part of a growth path, that corresponding to initial endowments marked II in the diagram. Altogether, four alternative cases must be distinguished, characterized by initial endowments I, II, III, and IV.

At point I the same amount of output is produced at time zero as at point II, the same saving rate s_0 pertains, but there is more capital and less labor. The increment in capital stock thus must be the same as at II; however, the rate of growth of capital must be smaller because the existing capital stock is greater. Consequently the slope of the expansion path at I is greater than at II (for path II-II), indicating that initially capital must be growing more slowly than labor (which is growing at a constant rate n). But as we will show in the following section, the rate of growth of k cannot remain indefinitely below that of labor. As indicated in the diagram, the rate of growth of capital will gradually increase, reaching at point A the rate of growth of labor ($k=n$). It cannot remain equal to n for the same reasons that the rate of capital $k=n$ could not have been preserved at point II. Rather it will keep increasing beyond point A , as is indicated by the convexity to the N -axis of the path passing through point I.

For successively lower initial capital-labor ratios (as we move our assumed starting point from I to IV) the initial rate of growth of capital (again with unchanged rate for labor n and the same savings rate s_0) must be successively higher. As will be noticed from the diagram, the slope of the expansion path at III is greater than that at IV. What makes the two situations III and IV different is the rate of acceleration. With initial condition III, capital initially grows faster than labor, and that rate gradually increases. With initial endowments K_0 and N_0 at point IV, the rate of growth of capital is even higher, but, as indicated by the curvature of the path, that rate is diminishing.

Relation (5) permits us now to derive from what has been shown for the rates of growth of capital and labor some corresponding results regarding the rate of growth of income. The initial rate of growth of income per capita (y) will be higher or lower than the rate of advance of technology (a) depending on whether the initial rate of growth of capital (k) is higher or lower than the rate of growth of labor. Accordingly, for initial condition I per capita income will be increasing at a rate inferior to a , for condition II at the same rate as technology, and for initial con-

ditions III and IV per capita income will advance faster than technology.

If in the initial stages the share of income earned by capital (ϕ) does not change significantly—on the limit, if it does not change at all, F being a Cobb-Douglas function—then what has been said above about acceleration of k must hold also for acceleration of y . Thus for initial condition I the rate of growth of per capita income (or, for that matter, aggregate income), while comparatively low, will be increasing, reaching the level α at point A in Figure 1, and keep increasing thereafter. At the other end of the spectrum, for initial condition IV, per capita income will grow at a high but diminishing rate.

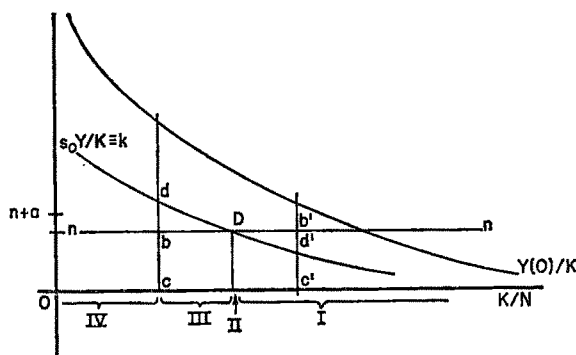


FIGURE 2

III. *The Asymptotic Properties of the Growth Path*

The alternative patterns of growth illustrated in Figure 1 by growth paths corresponding to initial conditions I, II, III, and IV and a given saving rate s_0 strongly suggest that there ought to be some asymptotic rate of growth of capital (constant or variable over time)—such as that corresponding to the line bb or another contour, not necessarily a straight line—towards which any possible growth, corresponding to any initial conditions along Y_0 ($t=0$), would converge over time. Our main task in this section is to identify and to derive such a limiting path. We will do this in two distinct stages. First, we will show the dynamic equilibrium, or self-correcting, forces inherent in the present theory, leading to an asymptotic path. As we do so, many of the results stated in the preceding section will become clearer. Second, using simple mathematics, we will derive the asymptotic growth rates and some related results for the whole class of linear homogeneous functions F , as well as for the specific case of functions F of the Cobb-Douglas type.

Consider Figure 2. The contour marked $Y(0)/K$ represents the

average product of capital, corresponding to the production function at time $t=0$, expressed as a function of the capital-labor ratio, K/N . The lower contour is obtained by multiplying the contour just identified by the saving rate s_0 . As indicated in the diagram, besides being saving per unit of capital, it also is the rate of growth of capital in period zero. The horizontal line mn indicates the rate of growth of labor, assumed constant.

As time advances starting from $t=0$, the two downward sloping contours (i.e., the average product of capital, and the schedule indicating the rate of growth of capital) keep shifting upwards at the constant rate a (say, two per cent of the vertical distances per annum). The mn line, on the other hand, remains unaltered. Simultaneously, as time advances, the capital-labor ratio keeps changing at the rate $k-n$. For example, with initial conditions corresponding to point d , the capital-labor ratio must be advancing at the rate $(cd-cb)$.

The growth process thus can be characterized as a composite continuous movement (1) along the k -schedule, and (2) with the k -schedule. The horizontal component of the first movement is equal to $k-n$, while the vertical movement of the k -schedule proceeds at a constant rate (of technological progress) a .

Consider first initial conditions corresponding to point D , reflecting the situation described by II in Figure 1. In that case, at the particular initial instant of time, the rates of growth of capital and labor are equal. But this cannot last, because, as time goes on, the k -schedule moves upwards, and a positive $k-n$ gap is opened.

Suppose now that the initial capital-labor ratio is higher than that just examined, say, corresponding to point d' . The $k-n$ gap now is negative and the capital-labor ratio must decline. At the same time the k -schedule keeps shifting upwards. Both changes cooperate in closing the $k-n$ gap, and thus, in reversing the movement along the horizontal axis from leftwards to rightwards. That reversal will take place when the traveling points such as d' and b' meet, somewhere to the right of D . Such a point corresponds to point A in Figure 1, and the whole situation corresponds to the path starting from I in that Figure. It is also easy to see now that the rate of growth of capital must be increasing, not only up to but beyond the time when the direction of movement of the capital-labor ratio is reversed.

Consider now initial conditions reflected by some point between d and D , that is, for a capital-labor ratio falling within the range III (comparable to initial conditions III in Figure 1). In this region the $k-n$ gap, while positive, is relatively small, so that the movement to the right along the horizontal axis is comparatively slow. But the k -schedule keeps shifting up at the constant rate a , and this keeps opening

the $k-n$ gap further so that the rightward movement along the horizontal axis is accelerated.

For initial points to the left of d , the situation is different. With a significant $k-n$ gap, the capital-labor ratio moves to the right at a fast rate, but because the rate of vertical movement of the k -schedule in this region (corresponding to IV in Figure 1) is still the constant a —the same as for region III—the rate of increase of the capital-labor ratio, and hence of capital, is declining.

It is evident that at some point the forces causing a deceleration in the rate of growth of capital in region IV and the forces causing an acceleration in region II just balance out, and there will no longer be any tendency, for a while at least, for k to increase or decline further. It is clear from our discussion that starting from any initial conditions the system will naturally converge to such a state. Actually, even at time zero, there must be a point on the k -schedule fulfilling these conditions; namely, that k , while greater than n , will tend initially neither to increase or to decline. Such a point is illustrated by point d in Figure 2. The locus of points, corresponding to points such as d for all possible time periods, traced in the capital-labor plane (see Figure 1), is then our "asymptotic path." It will be clear from the construction of Figure 2 that to each saving rate will correspond another asymptotic path. The rates of growth of capital and per capita income corresponding to that path we may refer to as asymptotic rates, and use for them the notation \bar{k} and \bar{y} . We now turn to the derivation of these asymptotic rates.

Using relation (2), the definition of the rate of growth of capital is

$$(8) \quad \dot{k} (= s_0 Y/K) = s_0 A e^{at} (N/K) f(K/N)$$

An extreme value of k (maximum or minimum), corresponding to a limiting point such as d in Figure 2, implies $dk/dt=0$, that is,

$$(9) \quad 0 = s_0 A e^{at} (N/K) [a + (n - k) + (k - n)\phi] f(K/N)$$

Dividing both sides of the equation by the product of terms outside of the square bracket and solving for k , we obtain what we have previously referred to as the asymptotic rate of growth of capital, that is,

$$(10) \quad \bar{k} = n + a/(1 - \phi)$$

Using relation (5) we obtain the asymptotic rate of growth of per capita income:

$$(11) \quad \bar{y} = a/(1 - \phi)$$

Realizing that the rate of growth of (K/N) is equal to $(k-n)$, it follows that both the asymptotic rates of growth of per capita income and per capita capital stock are identical.

As could have been expected, if the function f contains a region where the marginal productivity of capital becomes zero—that is, $\phi=0$ —any actual growth path must lead eventually to such a region (note that capital cannot grow indefinitely at a rate lower than or equal to n), and income per capita eventually will grow at a rate of technological progress a . A less self-evident result is that in the case where the marginal productivity of labor is zero, i.e., $\phi=1$, any actual growth path must eventually lead to an infinitely high rate of growth of per capita income and of the capital stock. Both outcomes are the immediate consequence of the convergence of actual towards asymptotic rates explained above.

One or the other of these extreme results must also be reached asymptotically if F is a “constant-elasticity-of-substitution function” excepting the Cobb-Douglas function, depending on whether the elasticity of substitution for capital is greater or lower than unity. In the former case the rate of growth of per capita income will asymptotically approach the rate of technological change a , and in the latter it will approach infinity.

The Cobb-Douglas function characterized by a unit elasticity of substitution and constant income shares ($\phi=\text{constant}$) yields the neatest results. We will elaborate on that case a little to clarify further the implications of our growth theory. First, it will be observed that, with constant income shares, both asymptotic rates \bar{k} and \bar{y} will be constant, not changing with the capital-labor ratio. Observing then that \bar{k} must be larger than n , we obtain an asymptotic growth path such as that illustrated by the line bb in Figure 1. The slope of that line is given by \bar{k} and n , and thus the path can be fully determined once we know one of its points. The intersection of bb with Y_0 ($t=0$) is such a point; it can be established as follows.

For a Cobb-Douglas function, f becomes $(K/N)^\phi$. Substituting that expression into relation (8) and writing (in that equation) $N_0 e^{nt}$ for N and $K_0 e^{kt}$ for K , and moreover, substituting for k the asymptotic rate \bar{k} as given in relation (10), we get

$$(12) \quad \bar{k} = n + a/(1 - \phi) = s_0 A (N_0/K_0)^{(1-\phi)} e^{t[a - s_0 + n(1-\phi) - n(1-\phi)]}$$

The exponent of e being zero, \bar{k} effectively is independent of time, and the asymptotic growth path consistent with that rate (i.e., $n + a/(1 - \phi)$), with a prescribed s_0 and A , is that for (N_0/K_0) satisfying relation (12). It is immediately apparent that changes in s_0 will not affect the slope of the asymptotic line such as bb in Figure 1, but will change its position. For higher saving rates we will have paths closer to the K -axis. If the initial K_0 and N_0 are such as to satisfy relation (12), the economy will expand indefinitely at constant rates $n + a/(1 - \phi)$, n , and $a/(1 - \phi)$ for capital, labor, and per capita income respectively. With any other initial

conditions the actual growth path will converge indefinitely towards that asymptote, as indicated for paths I, II, III, and IV in Figure 1.

For all other functions F of the class considered here ϕ will be changing with the capital-labor ratio and thus, with $a \neq 0$, the asymptotic path will not be a straight line (that is, \bar{k} and \bar{y} will not be constant). However, it still is defined (recall our discussion of points such as d earlier in this section), and can be expected to reach a linear form (such as bb) as time goes on, for very high capital-labor ratios. This is so because it is reasonable to expect that, for such ratios, ϕ will reach some stable value within the range (zero to one). But even if ϕ kept wiggling indefinitely (say, along a sine-curve) as K/N goes to infinity, relations (10) and (11) would still give us the corresponding values for the asymptotic rates of growth of capital and per capita income.

Of course, when $a=0$, that is, when technological progress is absent, the asymptotic path must be linear for any function F . This is the situation studied by Solow in his paper referred to in the introduction, and the corresponding rates are, from relations (10) and (11), $\bar{k}=n$ and $\bar{y}=0$.

When \bar{k} and \bar{y} are not constant, the problem of asymptotic convergence becomes a good deal more complex. Actually, as we will see presently, in such situations the term "asymptotic" must be given a broader interpretation than that implicit either in the Solow model or in the case of a Cobb-Douglas function discussed above. In what follows, we will concentrate primarily on the analysis of k and \bar{k} , recalling that corresponding results for y and \bar{y} can be derived from relation (5).

Suppose that \bar{k} is not constant (as it was for the Cobb-Douglas function) but rather a function of the capital-labor ratio, that is, $\bar{k} = \bar{k}(K/N)$. Three typical patterns for \bar{k} must now be envisaged; they are illustrated by the solid lines in Figures 3a, b, and c. All these figures reflect the same space as that of Figure 2, (K/N) being measured along the horizontal, and various rates of growth along the vertical axis.

The \bar{k} -contours divide the plane of these figures into two regions. Any actually realized rates (k) above these contours must be declining, and any actually realized rates below these contours must be increasing. This is what we have previously termed the "broader interpretation" of the asymptotic rates. Note that this interpretation does not require the decline in the difference between k and \bar{k} to an arbitrarily low level as time and K/N goes to infinity. It will also be noted that the contours \bar{k} , for functions F considered here, can never fall below the line corresponding to the rate $n+a$. This is immediately apparent from relation (10). Moreover, it follows that with \bar{k} always remaining at or above $n+a$, the actual rate of growth of capital k cannot indefinitely remain below $n+a$.

Six alternative actual paths for k , corresponding to initial conditions I through VI, reflect the asymptotic properties (broadly interpreted)

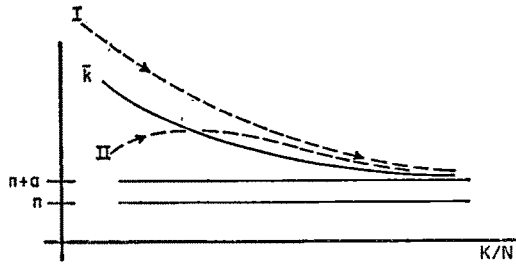


FIGURE 3a

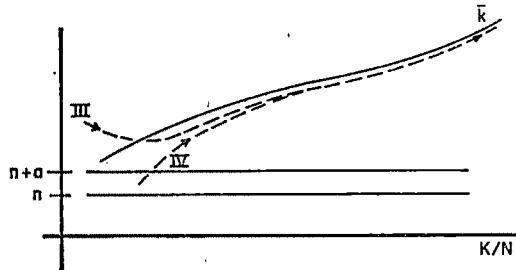


FIGURE 3b

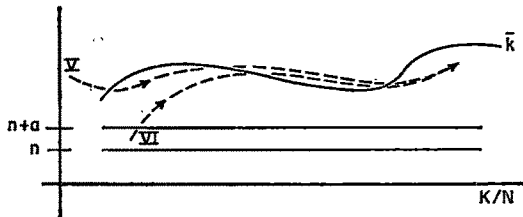


FIGURE 3c

specified above. For example, corresponding to initial conditions I, we find a continuous decline of k with continuously declining \bar{k} as time and K/N increase. As is illustrated, for example, by the path corresponding to initial conditions V, with \bar{k} not constant, k and \bar{k} can be equal (the k - and \bar{k} -paths will intersect) only when k passes through a maximum or a minimum. This is an immediate consequence of the fact that above \bar{k} , k must be declining, and below \bar{k} , k must be growing, as K/N (necessarily) increases.

From the fact that for $k = \bar{k}$ the actual rate k must be stationary, it follows that, whenever \bar{k} increases or declines over time, and k moves in the same direction as \bar{k} , the two rates cannot ever be equal. With \bar{k} increasing or declining over a prolonged period, such as in Figures 3a and 3b, k actually will converge to an asymptote (customarily, or

"narrowly" interpreted), which must remain at a certain minimum distance from \bar{k} . That minimum distance will vary, among other things, with the slope of the \bar{k} -contour.

In the real world, where ϕ (the competitive share of capital) generally changes only very slowly, if at all, and never comes too close to one, the slope of \bar{k} could be only very small. Under such conditions, it could be expected that k would come very near \bar{k} after a prolonged period of adjustment.

An interesting property of the \bar{k} -path is that that path, together with the prescribed rates of technological progress a and of population growth n , makes it possible to construct rather easily an actual k -path (such as the six broken lines in Figures 3a, b and c) once the initial point (such as points I through VI in Figures 3a, b and c) reflecting the initial capital-labor ratio and the initial rate of growth of capital is given. And thus, once the initial level of output or the initial supply of one of the two productive factors is also specified, the entire growth pattern can be fully described.

Moreover, knowledge of the \bar{k} -path and of a makes it possible in some situations—to be specified presently—to derive the true, or narrowly interpreted asymptote of k .

To show these results, let us return to equation (9). Its righthand side, as will be recalled, is the rate of change, with respect to time, of the rate of growth of the capital stock. Thus, making use of relation (8), we can write

$$(13) \quad \dot{k} = dk/dt = k[a - (k - n)(1 - \phi)].$$

Whence, dividing by k and making use of relation (10), we get

$$(14) \quad \dot{k}/k = a \left(1 - \frac{k - n}{\bar{k} - n} \right).$$

or, alternatively,

$$(15) \quad \dot{k}/k = a \frac{\bar{k} - k}{\bar{k} - n}.$$

In words, the rate of acceleration of capital, \dot{k}/k , is the rate of technical change, a , times the ratio of the excess of the asymptotic rate \bar{k} over the actual rate of growth of capital, and the asymptotic rate of growth of capital per laborer ($\bar{k} - n$). Thus, when k equals n , as at point D in Figure 2, \dot{k}/k is exactly equal to a ; and, as required by the definition of \bar{k} , when k equals \bar{k} , \dot{k}/k is zero. Moreover, as indicated by the broken contours in Figures 3a, b and c, above \bar{k} k must be declining, and below \bar{k} k must be increasing.

The convergence of an actual path of k towards a true (narrowly interpreted) asymptote—to be denoted k' —can now be further illustrated. Suppose that \bar{k} grows at the (positive) rate b , so that we can write

$$(16) \quad \bar{k} = \bar{k}_0 e^{bt}.$$

As both \bar{k} and k become very large after a prolonged period, the right-hand side of relation (15) will be approaching $a(1 - k/\bar{k})$, and a path

$$(17) \quad k' = k'_0 e^{bt}$$

where k'_0 is determined from

$$(18) \quad b = a \left(1 - \frac{k'_0 e^{bt}}{\bar{k}_0 e^{bt}} \right)$$

must then come arbitrarily near to the actual path k , after a sufficiently long period. This is so because whenever the actual k is smaller (greater) than $k'_0 e^{bt}$ in a given period the actual rate \dot{k}/k must be greater (smaller) than b .

Using relation (18) we get the value of k'_0 , namely

$$(19) \quad k'_0 = \bar{k}_0 \left(1 - \frac{b}{a} \right).$$

Thus in the special case where \bar{k} grows at a constant positive rate b over time, the true asymptote k' will be a line remaining at a distance $\bar{k}(b/a)$ below \bar{k} .

As was noted already, \bar{k} can never fall below the level $n+a$. Consequently, any continuing decline of \bar{k} must eventually lead to that level, and, as pointed out previously, capital will then grow at a rate k approaching the constant rate $n+a$. Such a situation is illustrated by Figure 3a.

Let us now consider briefly the actual rate of growth of per capita income y , and its relation to the asymptotic rates. We have already pointed out that y is given through relation (5) once a , k , n , and ϕ are known; consequently, once the variation of k is established, that of y can readily be obtained.

However, there is a more direct relationship between the rate of growth of per capita income and the actual and asymptotic rates of growth of capital just studied. This relationship, to which we now turn, will be useful to us in the following section. Using relation (5), the difference between the actual per capita rates of growth of income and capital is

$$(20) \quad y - (k - n) = a - (k - n)(1 - \phi).$$

As is apparent from relation (13), the righthand side of relation (20) is nothing but the rate of acceleration (the rate of growth of the rate of growth) of the capital stock, \dot{k}/k . Thus we can write

$$(21) \quad y = (k - n) + \dot{k}/k$$

and, using relation (15),

$$(22) \quad y = (k - n) + a \frac{\bar{k} - k}{\bar{k} - n}.$$

The already established fact that $\bar{y} = \bar{k} - n$ is now immediately apparent. Moreover, relation (22) tells us that whenever the actual rate of growth of capital falls short of the asymptotic rate, per capita income must grow faster than per capita capital stock, and vice versa for $k > \bar{k}$. More exactly, the difference between the (actual) per capita rates of growth of income and capital is equal to the difference between \bar{k} and k , that difference being measured in units of $(\bar{k} - n)/a$.

IV. *Some Reflections on the Theory in the Light of Observed Growth Patterns*

Before concluding our analysis, it may be of interest to confront the theory explored in the foregoing sections with growth patterns observable in the real world. Clearly, we cannot bring together all empirical data which would be relevant; instead, we limit ourselves here to a rather randomly selected and not always overly rigorous set of observations. Also, it must be kept in mind that any such observations can never provide a proof of the validity of the theory. They can only, to the extent that patterns consistent with the theory are observed, strengthen the hypothesis that the theory is an adequate one, and, to the extent that inconsistent patterns are observed, refute the theory's validity, or, more constructively, suggest possible improvements or alterations.

First, unlike a number of existing theories of growth, the theory presented here yields an explanation of a lasting state of expansion where capital must grow faster than population (or the labor force) and where, as a consequence (see relation [5]), income per capita must grow. Such trends are generally encountered in advanced economies.

Moreover, for economies in early stages of development, with a low capital-labor ratio, and a significant increase in the saving rate, we often observe a very high rate of growth of income accompanied by a gradual deceleration of that rate. Such a situation is illustrated by a growth path corresponding to initial conditions IV in Figure 1.

We can now turn to the U.S. economy, for which probably the most comprehensive statistics are available and where, during a prolonged period of growth characterized by a comparatively stable saving rate,

the dynamic forces studied in the earlier sections could have become satisfactorily operative. We can organize our remarks under two distinct headings: 1. Equilibrium dynamics, and 2. Disequilibrium dynamics.

1. Suppose that our theory is applicable, and that during a certain period of expansion in the American economy the rate of technological advance was reasonably stable; suppose, moreover, that during that period the growth path remained at or near the asymptotic growth path. If these conditions are fulfilled, then an estimate of the rate of growth of technology, a , can be obtained from relation (11) as $a = \gamma(1 - \phi)$.

Now we know from our previous discussion that when the actual growth rate of income is equal to the asymptotic rate, then the rates of growth of per capita income and capital must be equal. (Note that the asymptotic rates of growth of per capita income and capital are equal, and that, by definition, equality of actual and asymptotic rates of growth of income implies equality of actual and asymptotic rates of growth of capital.) Such conditions are quite satisfactorily fulfilled for the American economy, using Solow's tabulations, [7, p. 315] for the period 1909–1930, when income per man-hour grew at an average rate of $\gamma = 0.016$, only slightly less fast than employed capital per man-hour. Using 0.28⁶ as a reasonable estimate of the net competitive share of capital ϕ for the period, instead of Solow's approximately 0.34⁷ reckoned on a gross basis, we obtain $a = 0.0115$. Solow's estimate for this [the corresponding] period is $a = 0.0009$, but it is obtained using gross rather than net capital income share, and would have come to just about our 1.15 per cent if net rather than gross income share were used in Solow's calculation.⁸

A more recent and presumably more reliable set of data fitting our theory is found in Kendrick's study of productivity trends in the United States [5]. As with Solow's period 1909–1930, the portions of Kendrick's series covering the period 1889–1919 reveal only a small difference between the rates of growth of capital and income per unit of labor input [5, pp. 327–30], the latter rate being 0.016. The average income share of capital for the period, extrapolating Kendrick's data [5, p. 121], is about 29.5 per cent, there being an insignificant (about one-tenth of a percentage point per annum) decline in that share over

⁶ As we will see presently from John W. Kendrick [5], this share seems to be a good approximation of the average net share for the period.

⁷ There is no discernible trend in the yearly data on which this average is based, and consequently it can be postulated that the asymptotic rates for the period—of course, assuming a constant a —are invariant.

⁸ Actually, the question can be asked whether Solow's use of the gross share (of course, forced on him for lack of other data) did not lead to a set of downward biased estimates, and whether the figure 0.0115 obtained here is not closer to the true growth parameter for the period.

the thirty-year period. Using these estimates of γ and ϕ , we obtain an estimate of $a=0.011$. Kendrick's calculations of the rate of growth of total factor productivity—conceptually comparable to our parameter a —lead to an estimate of 0.010 for the same period [5, p. 67].

2. Now if calculations similar to those just explained are performed for the more recent portions of Solow's and Kendrick's series—covering 1930–1949 and 1919–1953 respectively—estimates of a significantly below those of the two authors are obtained using our method. But this is, as we will see, perfectly consistent with the theory of growth presented here. Because the arguments for the two cases are similar, and because Kendrick's data are more complete and probably more reliable, we will restrict ourselves in what follows only to the analysis of Kendrick's statistics.

Kendrick's estimate of the rate of growth of total factor productivity for 1919–1953 is 0.021 or 0.022, depending on the method used in fitting a regression [5, p. 67]. In either case, his calculations show that the rate of technological progress has more than doubled between the first and second periods. Using our "equilibrium" method of estimation of a for the second period (i.e., $a=\gamma(1-\phi)$), we obtain $a=1.5$.

While Kendrick's estimate of the average share of capital income for the first period (1889–1919) is somewhere near 30 per cent, that for the second is somewhere near 24 per cent, or even lower; moreover, during the second period a marked decline is observable in the share (about three-tenths of a percentage point per annum). Remembering that even for the first period there was some decline in our estimator of ϕ , the asymptotic paths of \bar{k} corresponding to Kendrick's information can be characterized, using concepts developed in this paper, in the following way: Both paths of \bar{k} for the first and second periods are declining, comparable to the solid line in Figure 3a, but the path for the second period is about twice as high as that for the first (see relation [10]) because of the more than doubling of a and some (offsetting) decline in ϕ . Now if during and toward the end of the first period the actual growth path were near the asymptotic path (as we have argued in the introduction), and a comparatively abrupt upward shift in the asymptotic path of \bar{k} took place in the second period, a dynamic disequilibrium situation must have arisen wherein the actual path of k suddenly found itself considerably below the new asymptotic path. The period of transition (around 1920) can thus be characterized by the situation described by point II in Figure 3a.

From relation (22) it follows that in such a situation the actual rate of growth of income must be considerably above the actual rate of growth of capital. Such were, as is well known, the historical trends; the real product-capital ratio, according to Kendrick [5, pp. 327–330], increased by about 50 per cent over the second period (1919–1953). More-

over, the rate of growth of real income (product) per unit of labor input should have increased in the second period; this is easily established from Kendrick's chart [5, p. 66].

Finally, as indicated by the pattern described in Figure 3a by initial conditions II and the segment of the k -path immediately to the right of point II, the actual rates of growth of capital and income should have been gradually approaching the asymptotic rates in the course of the second period. If such a "rapprochement" were fully effected, the actual rates of growth of capital and income should have come again to equality for rates of growth of capital k in the vicinity of the asymptotic rate \bar{k} . Such a situation is in fact observable from Kendrick's data [5, p. 330] for the period following World War II.

But if the asymptote were again at least approximately approached in the most recent segment of the second period, then our "equilibrium" formula for estimating a ($a = y(1 - \phi)$) should again be applicable. Because the period is very short, and because there is a good deal of year-to-year variation, the corresponding estimate may not be very reliable. However, using Kendrick's estimates of (national) output per unit of labor input for the period 1947-1957, we get an estimate of $y = 0.026$. Coupling this with the average share of income of capital for the period of about 21 per cent, also taken from Kendrick's work [5, p. 121], we obtain exactly Kendrick's estimate for the second period, namely $a = 0.021$.

The strong downward bias of the estimate of a ($= 1.5$ per cent) resulting from the application of the "equilibrium" formula to the entire period 1919-1953 can now be easily explained. Because through most if not all of the second period k was below \bar{k} , as we have argued above, y must also have been less than \bar{y} ; but it was y that was used in obtaining the estimate $a = 0.015$. Were considerably higher values of \bar{y} used *correctly* in the "equilibrium" formula, an estimate much closer to Kendrick's 0.021 would have been obtained.

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ON THE THEORY OF OPTIMUM EXTERNALITY

By F. TRENERY DOLBEAR, JR.*

Modern welfare economics has established the Pareto optimal properties of a competitive economic system which satisfies such customary requirements as convexity and the absence of externalities. A frequent problem of the welfare economist is: Given all of the *other* requirements for Pareto optimality, how can a situation characterized by the presence of an externality be altered so as to produce a Pareto optimum? In the discussion which follows we will, for convenience, presume an external diseconomy.

Most of the formal analysis on externalities has been concerned with relationships between firms;¹ in this paper the actors will be consumers. As a result our analysis will be derived from the theory of consumer behavior instead of the theory of the firm. Although space limitations prohibit a detailed comparison, some of the results will be sensitive to this distinction since the usual welfare objective of maximizing joint profits subject to price parameters cannot be carried over to consumers without interpersonal comparisons of utility.

The occurrence of an external diseconomy will, in general, lead to a discrepancy between marginal *social* net benefit and cost. It is often presumed that a single device such as a per unit tax or individual negotiations can be used to correct the inequality.² While one of these prescriptions may improve the efficiency of the economic system, it will be argued here that it need not, in general, result in a Pareto optimum even if used in the best way.

It is sometimes presumed that the parcelling of legal responsibility³

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¹ For example, Davis and Whinston [4], Wellisz [16], Coase [2], Kneese [8], and Kamien, Schwartz and Dolbear [7].

² For some qualifications on the use of these devices see Coase [2], Buchanan and Stubblebine [1], Davis and Whinston [4, 5], Turvey [15], Wellisz [16], and Mishan [9].

³ It is not for an economist *qua* economist to assess legal responsibility for an externality. For example, suppose my neighbor has a coal furnace and I find the smoke from his chimney noxious. Most people (I know) would hold my neighbor responsible. Now, suppose my neighbor paints his house gray and I find gray a depressing color. Here most people would hesitate to blame my neighbor. Determination of responsibility for externalities seems to have more to do with private property rights than with economics—which makes it a matter for the courts or for a legislative body. Of course, the law might permit a certain amount of externality as the "status quo" (starting point); i.e., the law does not always stipulate a polar solution (see Davis

does not affect the solution that market-bargaining forces will produce in the presence of an externality.⁴ But we will show that where consumers are involved the amount of externality may be very much affected by legal matters.⁵ Since changes in legal responsibility will alter the distribution of real income, there may be an "income effect" which will be of some consequence vis-à-vis the amount of externality. Of course, given a specific assessment of legal responsibility there may be a further income effect if the parties involved have opportunities to negotiate along the contract curve.

To illustrate these points we will resort to a highly simplified and somewhat artificial example involving two individuals X and Y . (The results will be extended to a world with m X -type individuals and n Y -type individuals in Section V .) It is hoped that the example will illustrate some difficulties which have been encountered in the analysis of real world problems.

I. A *Triangular Edgeworth Box*

Presume two consumers, X and Y , who form a "community" in a large economic society. Both have simple consumption patterns. X lives by bread and heat; Y lives by bread alone. However, Y is so situated that he receives air pollution in the form of smoke produced by X 's fire. We presume Y cannot—or at least that it is uneconomical to—move to a location which would escape X 's smoke output. Further, X can only reduce the amount of smoke by decreasing heat.⁶

Our analysis will use indifference curves and budget lines. First, X has indifference curves for bread and heat which meet the usual requirements in the theory of consumer choice (Figure 1). On the other hand, Y has indifference curves for varying quantities of bread and smoke. They are also shown in Figure 1 and have the standard characteristic that the more bread and smoke Y has, the more bread he will trade for a given amount of smoke abatement. By construction we establish Y 's smoke scale so that it corresponds precisely with X 's heat scale.⁷ For conveni-

and Whinston [5]). Pigou [10], who is most frequently footnoted as the precursor of externality discussions, did not explicitly recognize this indeterminacy. To my knowledge the point was articulated first by Coase [2].

⁴ Coase [2] and Kneese [8] obtain this result for externalities among firms.

⁵ This point has been made (but not demonstrated) by Buchanan and Stubblebine [1].

⁶ Buchanan and Stubblebine [1] analyze a comparable problem (for a fence and a numeraire good). Their construction, using "marginal evaluation curves," is similar in content to the one presented here; however, income effects cannot be analyzed directly because costs (for the fence) are handled by modification of the marginal evaluation curves. Also their example is restricted to two individuals.

⁷ We have no conventional measure for units of smoke (nor of heat). For our purposes it is sufficient that units of smoke be a multiplicative function of units of heat.

ence in developing the argument below, Y 's bread axis reads from top to bottom.

Given an X - Y community income endowment, the opportunity locus or budget line is shown as FF in Figure 2. The intercepts of budget line FF give the maximum amounts of bread or heat (smoke) which the community can purchase, and the slope of FF represents the ratio at which

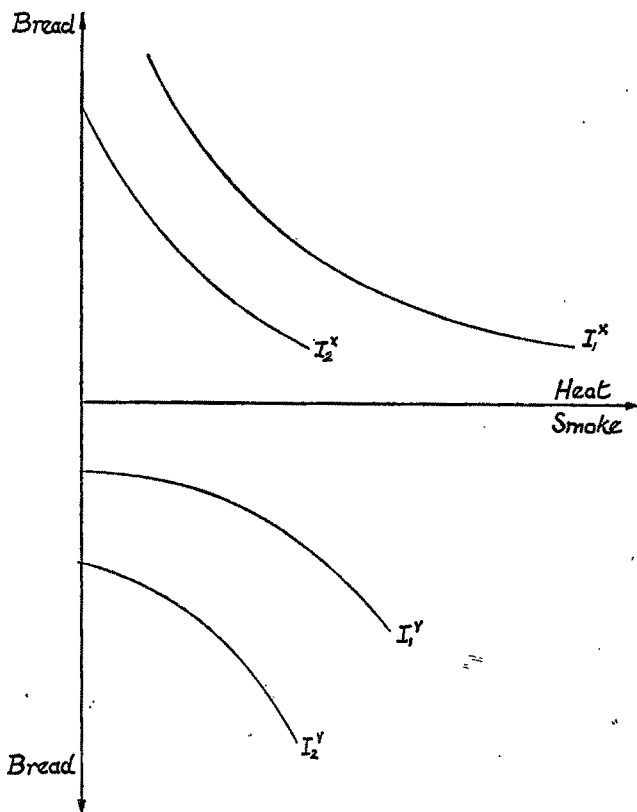


FIGURE 1

heat can be traded for bread. This price ratio between heat and bread is presumed to reflect society's opportunity cost in terms of bread of producing one unit more of heat (the marginal rate of transformation). We presume that variation in the amounts of bread and heat purchased by X and Y will not affect this rate of transformation.

The triangle formed in Figure 2 by the two axes and budget line FF may be thought of as a type of Edgeworth box. The standard two-commodity, two-individual, rectangular Edgeworth box has several distinctive features. Any point in the box unambiguously defines quan-

ties of each commodity for both individuals. Thus indifference curves for each individual can be transcribed into the box. The tangencies of these indifference curves provide the locus of efficient allocations, in the Pareto sense, between the two individuals (the contract curve). However, if the total quantities of the two commodities are variable, according to the marginal rate of transformation, the welfare implications of the contract curve do not hold.

In this example, where there is a one-to-one relationship between heat

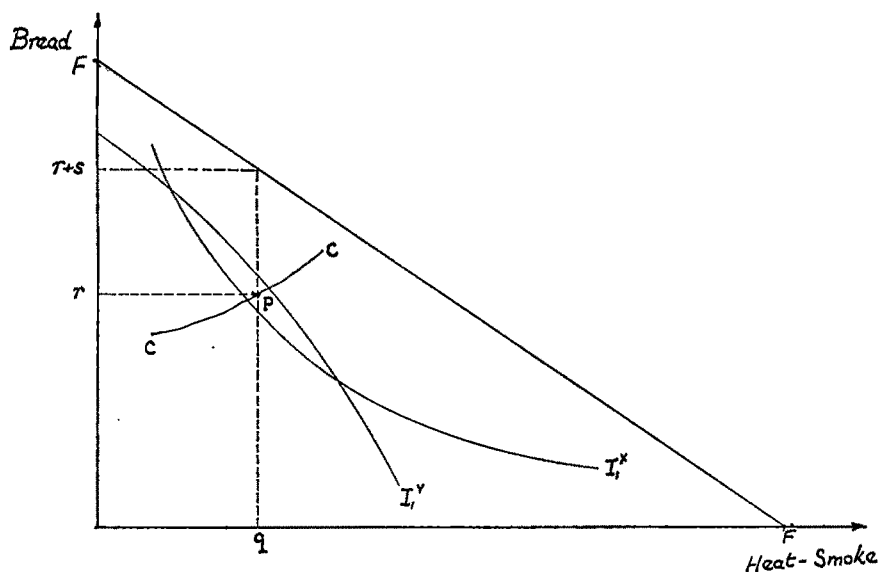


FIGURE 2

for X and smoke for Y , a construct similar to the Edgeworth box will be developed which in addition incorporates changes in the quantities of the commodities. (A decrease in heat—and thus smoke—means the community will buy more bread. The community's total bread consumption will increase according to the slope of FF .) Since tradeoffs between bread and heat are included, the Pareto welfare implications of the contract curve will be appropriate.

Suppose the consumption of the community is represented by (any) point P in Figure 2. Units of heat and bread for X are determined in the usual manner; thus X receives q units of heat and r units of bread. We require that X and Y exhaust all of their income—community consumption must lie on budget line FF . Y , then, receives q units of smoke and s units of bread. In other words, if we know the amount of bread and heat for X (represented by P), we immediately know the amount

of smoke for Y (by reading off the heat-smoke axis) and also the (residual) amount of bread for Y (the vertical distance from point P to the community budget line FF .)

Since we have indicated how to read bread and heat (smoke) consumption for any point P in the triangular Edgeworth box, it is now possible to construct indifference curves in Figure 2 from those shown in Figure 1. First, we transfer X 's indifference curves directly into the triangular Edgeworth box of Figure 2. Then, in effect we place Y 's bread axis of Figure 1 on the bread axis of Figure 2—the origin is at the intersection of community budget line FF and the bread axis. We “fold-in” the smoke axis of Figure 1 so that it lies on the community budget line FF (again with the origin at the intersection of FF and the bread axis) and “stretch” the smoke axis so that it will correspond to the horizontal scale for heat and smoke for Figure 2. This will, of course, “fold-in” and “stretch” Y 's indifference curves also, but they will retain the same general curvature as in Figure 1.

Y 's Figure 2 indifference curves will be always steeper than FF since Y requires an increase in bread to compensate for an increase in smoke. Further, since the more smoke Y has, the more bread Y will trade for a given amount of smoke abatement, the indifference curves must become steeper as Y gets more smoke. Y 's indifference curve which has been transferred to Figure 2 may be seen to meet these requirements.

The reader should note that Y 's Figure 2 “indifference curves” do not possess the usual property that the slope is the marginal rate of substitution. The algebraic slope of a Y indifference curve in Figure 1 is the negative of the marginal rate of substitution of smoke for bread at a given level of smoke and bread. A Figure 1 indifference curve with zero slope would have a slope in Figure 2 equal to the slope of the FF line, the marginal rate of transformation (MRT). This reflects the requirement that an increase in heat must be offset by a decrease in total community bread consumption. Consequently, the slope of an indifference curve for Y in Figure 2 in general will be $-MRS_{SB}^Y$ plus MRT .

Since X 's indifference curves are convex to the origin and Y 's indifference curves are concave to the origin (in Figure 2), tangencies of X and Y indifference curves will be Pareto optimal as in the standard Edgeworth box. Suppose, for example, that at point P , X is indifferent to trades at a rate of 5 breads for 1 heat; and Y is indifferent to trades at a rate of 2 breads and 1 smoke (heat). If the marginal rate of transformation is 3 breads for 1 heat, the indifference curves of X and Y are tangent since:

$$(1) \quad \begin{aligned} MRS_{HB}^X &= -MRS_{SB}^Y + MRT \\ -5 &= -(+2) - 3. \end{aligned}$$

It can be seen that no reallocation in consumption or production can make one consumer better off without making the other worse off. Thus P would be a Pareto optimum point.⁸ In the triangular Edgeworth box of Figure 2, the contract curve (locus of Pareto optimal points) is labelled as cc .⁹

II. Possible Solutions

Now that we have indicated how to interpret the curves in Figure 2, we are ready to impose a distribution of money income between X and

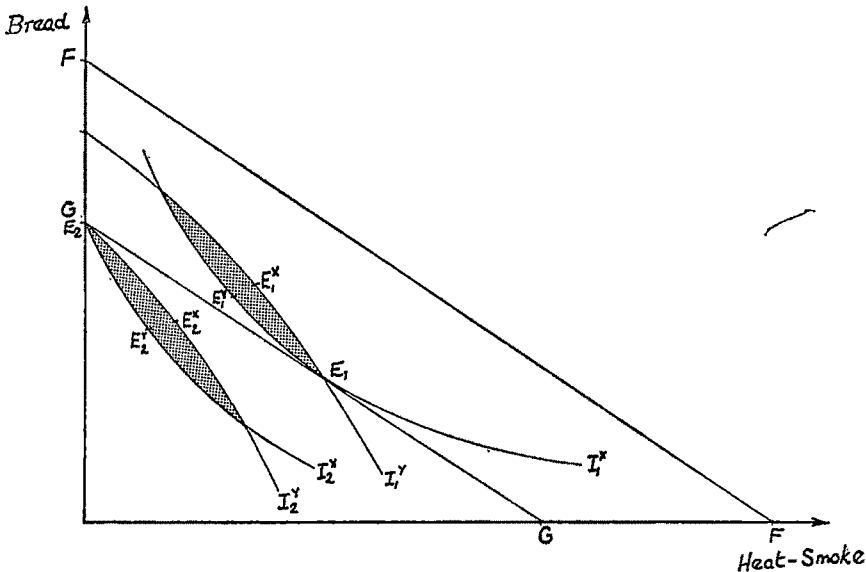


FIGURE 3

Y and consider several legal status quo or starting points which might obtain prior to any "negotiations" to ameliorate the effects of the externality. We will also consider the possible results of such negotiations. (In the next section we will examine government tax schemes designed to ameliorate the external effects.) In Figure 3 budget line GG reveals

⁸ Readers who are familiar with Samuelson [11, 12] will recognize the condition expressed in (1). Samuelson suggested that Pareto optimality for a public good requires the sum (over consumers) of the marginal rates of substitution of the public good for the private good to equal the marginal rate of transformation. The same condition is used for this example—which could be called a public "bad" (social evil) for consumers other than X .

⁹ We assume that heat (for X), smoke abatement (for Y) and bread (for both) are not inferior goods. Consequently, the contract curve must have a slope which is greater algebraically than that of the budget line. When Y moves to a lower indifference level along the contract curve, he must obtain more smoke and less bread. X , who will be moving to a higher indifference curve, must receive more heat, but since the total amount of bread decreases, we cannot be sure he will receive more bread.

the distribution of money income between X and Y . X 's money income will buy any combination of heat and bread represented by points on the GG line. Y 's money income will buy a quantity of bread represented by the vertical distance between FF and GG .

The examples we will consider differ in several ways. First, the legal system might permit X to generate as much smoke as he wishes; at the other extreme X might be prohibited from generating smoke without Y 's permission. Second, given a legal starting point, all of the gains from amelioration of external effects could accrue either to X or to Y . We will consider only "polar" cases; in so doing we will bracket the results which might obtain from intermediate cases.

Initially, we suppose a world in which X has the legal right to generate as much smoke as he wishes. In this case X , who independently maximizes his satisfaction, will be at E_1 on I_1^X (Figure 3). Y will be at E_1 on I_1^Y where he spends all of his money income on bread—the vertical distance between (parallel) budget lines FF and GG —and receives gratuitously smoke from X 's fire. Since I_1^X will be tangent to GG while I_1^Y must be everywhere steeper than GG , the two indifference curves cannot be tangent. Thus E_1 will not be Pareto optimal.

The shaded area between I_1^X and I_1^Y is, for the E_1 starting point, the "area of possibilities"; it includes all solutions which will not decrease the satisfaction of either consumer. In this area the contract curve is bounded by E_1^X and E_1^Y where all the gains from amelioration of the externality accrue to X and Y respectively.

Now suppose a world which prohibits X from generating smoke without an agreement with Y . Here X and Y will, in the absence of such an agreement, start on indifference curves I_2^X and I_2^Y at the intersection of budget line GG and the bread axis (E_2). This starting point reflects the prohibition of heat (smoke) production; both X and Y are spending all of their incomes on bread. If I_2^X lies above I_2^Y everywhere but at E_2 , then E_2 is a Pareto optimal corner solution. But if I_2^X is below I_2^Y in the neighborhood of E_2 , E_2 will not be Pareto optimal.

For this starting point, the shaded area between I_2^X and I_2^Y is the area of possibilities. The contract curve in this region is bounded by E_2^X and E_2^Y where all the gains from amelioration of the externality accrue to X and Y respectively.¹⁰

We have not determined what point in the triangular Edgeworth box will actually prevail. This is a (largely unresolved) problem in bargaining theory. However, when the legal starting point was altered from E_1 to E_2 , nonintersecting areas of possibilities were obtained. Thus legal

¹⁰ The possible legal starting points need not be restricted to E_1 and E_2 . If restrictions on smoke output could be varied continuously from no restriction to total restriction, the area of possibilities could include all points lying between I_1^Y and I_2^X .

interpretation may be expected to affect the final allocation of resources; only as a special case would the amount of externality (smoke) be unaltered. Of course if the marginal rates of substitution of heat (or smoke) for bread were independent of the quantity of bread (i.e., indifference curves parallel vertically), then the contract curve would be vertical and the amount of smoke would be independent of the legal starting point.

III. Taxes—1 Y and 1 X

In this section we will sketch some techniques frequently advanced for "solving" externality problems and briefly examine the properties of these techniques. In the two-person example described above, negotiations (bilateral bargaining) would seem to be the most appropriate method for ameliorating the effects of an externality.¹¹ However, since economic principles do not determine the outcome of such bargaining, this technique will not be discussed here.

Instead we will consider, in anticipation of our extension to m X -type individuals and n Y -type individuals, the solutions which would result from various tax schemes. We will presume that the individuals do not use their "monopoly power." For example, X does not recognize the effects of his actions on tax rates. He just (blindly) maximizes at the margin. Such behavior will be easier to justify when we think of a community composed of a large number of X 's; so that the isolated behavior of one individual has a negligible effect on the total quantity of smoke.¹²

In the real world we frequently encounter externality with the economy in effect operating at E_1 in Figure 4. (Smoke pollution is a possible example.) Suppose a welfare economist suggests that X should, if he wants to continue to produce smoke, be required to pay Y for the damages inflicted. That is, X 's bread compensation should be sufficient to keep Y on I_2^Y (Figure 4).¹³ One technique for achieving this goal, which enjoys the role of the standard solution, is a tax (in bread) on X per unit of smoke. The tax revenue would be given to Y .¹⁴

This per unit tax has the effect of rotating X 's budget line GG around starting point E_2 (The higher the tax rate, the steeper the after-tax budget line.) Then the effective bread price of heat is the market price

¹¹ Most of the recent literature has discussed the possibilities for negotiations, e.g., Coase [2], Buchanan and Stubblebine [1], Turvey [15], Davis and Whinston [5].

¹² For some suggestions as to how to use taxes when many individuals are involved in the example of water pollution, see Kneese [8] and Day, Dolbear and Kamien [3].

¹³ The following analysis could be carried out for other sets of assumptions. For example, X might be permitted to produce at E_1 , and Y required to compensate X for any reduction in smoke output. Such analysis is not included here since it would be mostly repetitious.

¹⁴ It is important that the tax revenue be given to Y ; the argument that Y is on indifference curve I_2^Y depends upon this transfer.

for heat plus the per unit tax on smoke. The appropriate solution will be obtained if an after-tax budget line is selected which goes through the intersection of I_2^Y and X 's price-consumption curve, pp (the locus of tangencies of X 's indifference curves and after-tax budget lines originating at E_2 .) In Figure 4, X and Y will be on I_3^X and I_2^Y respectively. However, this cannot be Pareto optimal (unless a corner solution at E_2 is appropriate) for I_2^Y will intersect the optimum budget ray and I_3^X from above, and thus not be tangent to I_3^X .¹⁵

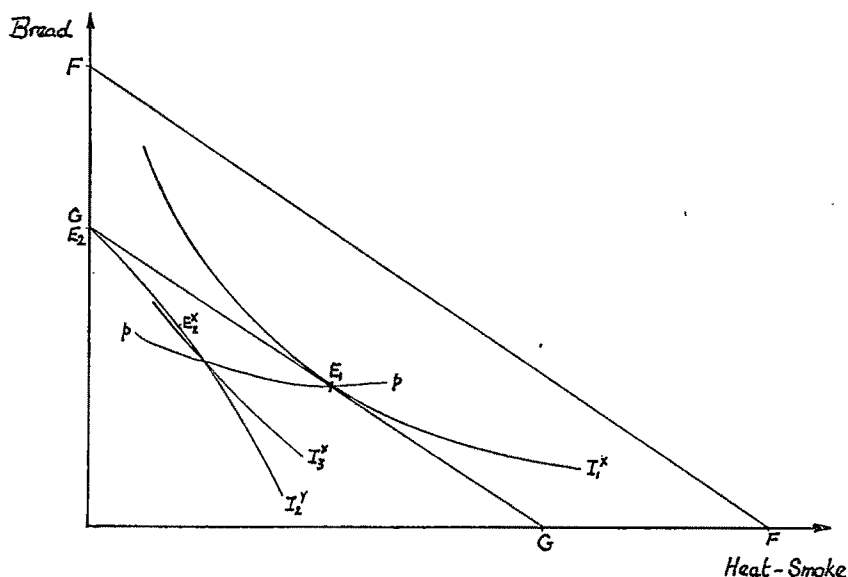


FIGURE 4

This analysis suggests that a per unit tax on X which has the twin objectives of keeping Y on his starting point indifference curve and attaining a Pareto optimum is not, in the general case possible.¹⁶ (The tax rate which leads to a solution at the intersection of the price-con-

¹⁵ Another solution which might be considered is a tax achieving a lump sum bread transfer from X to Y for damages. In this case X will move from E_1 to a tangency with a lower "after-tax" budget line parallel to GG ; the appropriate transfer can be determined from the intersection of I_2^X and X 's income-consumption curve (the locus of tangencies between X indifference curves and budget lines parallel to GG .) The vertical distance from the lower budget line to GG represents the lump sum bread transfer. Since I_2^Y must be everywhere steeper than the after-tax budget lines, a tangency solution will not result and the lump sum tax will not be Pareto optimal.

¹⁶ I have been unable to find any welfare economists who recommend a complete set of goals and techniques for internalizing an externality. However, per unit taxes, compensation, and Pareto optimality are not uncommon classroom presumptions and they can be found individually, if not *in toto*, in the modern literature.

sumption curve pp with the contract curve would be Pareto-optimal, but at this tax rate Y would be more than compensated for smoke damages caused by X .) However, a combination of a lump sum *subsidy* and a per unit tax could be used to achieve the Pareto optimal point E_2^X on indifference curve I_2^Y . The appropriate per unit tax could be derived from the slope of an after-tax budget line tangent to I_2^Y at E_2^X ; this per unit tax will be higher than the per unit tax derived above. The vertical distance from budget line GG to the intersection of the after-tax budget line with the bread axis represents the lump sum subsidy. Intuitively, since the shape of Y 's indifference curves in Figure 2 implies that his smoke damages increase as he encounters more smoke, the appropriate per unit tax is more than enough to pay smoke damages on the inframarginal units of smoke. The steeper tax rate discourages smoke production appropriately at the margin while the subsidy holds down revenues so that Y is not overcompensated.

IV. A Digression on Information

In the tax example above it was implicitly presumed that an omniscient government or Y had full knowledge of X 's indifference curves as well as of I_2^Y . Thus the appropriate tax could be immediately determined. However, in the real world such knowledge is usually unavailable. To circumvent this information problem the government might use an iterative procedure (similar to a *tatonnement*).¹⁷ In our example the government wishes to permit X to create smoke so long as Y is adequately compensated—kept on I_2^Y . Suppose the government levies a per unit tax in terms of bread on X 's creation of heat (smoke). X responds by selecting that quantity of heat and bread which maximizes his utility. Then if the resulting quantity of smoke and tax revenue (in bread) puts Y to the east (west) of I_2^Y , the per unit tax should be increased (decreased). The final solution will, of course, be at the intersection of X 's price-consumption curve and I_2^Y .

Suppose, instead, the government seeks to insure the satisfaction of the marginal conditions necessary for Pareto optimality. It can again assume that X will be on an indifference curve tangent to his after-tax budget line. If X 's selection puts Y on an indifference curve which cuts the after-tax budget line from above (below), the tax rate should be increased (decreased). A Pareto solution will be achieved at the intersection of the contract curve and X 's price-consumption curve.

All of this can be done without knowledge of X 's indifference curves (providing X maximizes blindly). However, if Y and not the government is administering this scheme, a Pareto optimum is unlikely. Y can

¹⁷ Such a procedure was suggested by Davis and Whinston [5].

maximize his utility by selecting the tax rate where X 's price-consumption line is tangent to a Y indifference curve; this point will not be Pareto optimal. Consequently this *tatonnement* procedure would seem more applicable in situations where Y is not a self-seeking individual but represents a government authority which desires that marginal conditions for Pareto optimality be satisfied. Income redistribution can be achieved (ideally) through some other means such as lump sum transfers.

The above discussion displays techniques for improvements (in the Pareto sense) over the starting point solution when X 's indifference curves are not known. However, if this lack of information means the government (or Y) is also ignorant of the starting point, improvements may not be possible. This situation is examined in the remainder of this section.

In some applications of economic theory to externalities, it is presumed that one of the agents takes the initiative in achieving an optimum amount of externality. This (magnanimous) agent presents to the other agent a schedule of damages or benefits which will accrue to him from changes (from the starting point) in the amount of externality. The other agent then is invited to choose that amount of externality (and pay or be paid damages) which maximizes his satisfaction without reducing his "benefactor's" satisfaction below the level achieved at the starting point.¹⁸

But this "benefactor" technique is not always successful even theoretically. If Y is the benefactor and there are no legal restrictions on smoke production (starting point E_1), a difficulty may result if Y does not know X 's indifference curves. Generally, the "magnanimous" agent can derive the starting point from the law and his own preferences.¹⁹ But in this case the starting point E_1 depends on the behavior of X in the absence of any restrictions on smoke production. Perhaps Y will know this behavior historically. But if there is no history or if X 's behavior changes over time (because of changes in income for example), Y is faced with a dilemma. If X knows that Y will permit him to move along Y 's starting point indifference curve, X can maximize his satisfaction by claiming that his starting point would be an expenditure of *all* of his income on heat (smoke). This would of course put Y on an indifference level below the level of E_1 . Y will be understandably reluctant to have his generosity result in such a sacrifice. But Y has no obvious

¹⁸ For example, such a system is implicit in Kneese [8]. A (magnanimous) water authority is the counterpart of Y and a polluting firm is the counterpart of X .

¹⁹ If the law indicates prohibition, Y or X (as a benefactor) can determine his starting point indifference curve. And if the law indicates no smoke restrictions, X can determine what he (as a benefactor) would do, i.e., he can locate E_1 and indifference curve I_1^X .

way out of his dilemma. If, on the one hand, he does nothing, the non-Pareto optimal E_1 will obtain. But, on the other hand, if Y takes the initiative in achieving a Pareto optimum, he may be forced to a lower indifference level.²⁰

V. Taxes— m X 's and n Y 's

For simplicity and ease of exposition our analysis has thus far been carried out in terms of two individuals X and Y . But the exercise is much more appropriate for applications where there are many individuals such as X and many individuals such as Y . Although the resulting model is still artificial, it is an improvement in an important dimension over the bilateral case.

Suppose there are m bread and heat (smoke-producing) individuals and n individuals who live by bread alone (each must breathe the smoke produced by the m smoke-producing individuals). We need not assume equal incomes or identical tastes for either the X -types or the Y -types. Assuming that a unit of heat for any X -type consumer generates an equal amount of smoke for every Y -type consumer, the condition for a Pareto optimum is:

$$(2) \quad MRS_{HB}^{X_1} = \dots = MRS_{HB}^{X_m} = - \sum_{i=1}^n MRS_{SB}^{Y_i} + MRT.$$

Following our example of Section III, we consider the starting point where no X is permitted to produce smoke. In the absence of an agreement with the Y community, all of the X 's will spend all of their income on bread. Given the income of each X , it is a simple matter to determine the total quantity of bread which would be bought by the X community. If we also know the price ratio between heat and bread, a budget line corresponding to GG can be constructed.

The Y community will spend all of its income on bread also. Given the total income of the Y 's, their total bread consumption can be determined. This leads to a budget line corresponding to FF . For each of the Y -type individuals, expenditure of all of his income on bread would put him on a particular starting point indifference curve. A Scitovsky community indifference curve [14] (or a Samuelson minimum-total-requirements contour [13]) corresponding to I_2^Y can be obtained by summing over all Y 's the quantity of bread which would be required to keep

²⁰ A similar problem is analyzed in a paper by Kamien, Schwartz, and Dolbear [7]. In that article a profit-maximizing firm (X) finds it advantageous to create "uneconomic" pollution in order to convince the (magnanimous) water authority (Y) of a false starting point which works to the advantage of the firm in terms of income distribution. But since the increase in the profits of the firm is less than the decrease of profits of the authority (increase and decrease are measured as deviations from the result which would obtain using the "true" starting point), the solution is not Pareto optimal.

each Y on his starting point indifference curve at various levels of smoke. The resulting community indifference curve will have the same general shape as I_2^Y (i.e., downward-sloping, concave to the origin); however, it will be steeper.

The intersection of this Y community indifference curve with the X community's aggregate price-consumption curve²¹ yields the after-tax budget line which will keep the Y community at their starting point indifference levels.⁶ This solution, as in the two-person example, will not be Pareto optimal since all members of the X community will be on indifference curves which are tangent to the after-tax budget line, and the Y community indifference curve will intersect this after-tax budget line from above. Equation (2) will not be satisfied.

Although most of the results obtained in the 1 X and 1 Y example can be carried over to m X 's and n Y 's, one result cannot. It is well known that the shape of a community indifference curve depends upon the distribution of income.²² Thus a locus of tangencies between Y community indifference curves and X community indifference curves (the contract curve) cannot be obtained in the absence of information as to how the gains from amelioration of the external effects will be distributed. To be sure, Pareto optimum points exist but the problem of dealing with externality is confounded since the techniques used above for locating such points will not be effective here.

VI. Conclusions

Although the preceding analysis has been based on a highly simplified model, the following implications for real-world problems involving consumers have been suggested:

1. The amount of externality that will tend to emerge depends on the extent of legal responsibility. The distribution of the "gains from trade" should also have an effect on amount of externality. (Section II).

2. It is not simple to regulate externality with government tax schemes. Some of the standard tax proposals will not generate results that always satisfy the requirements for Pareto optimality. It is not in general possible to impose a per unit tax which will simultaneously compensate (exactly) for damages and achieve a Pareto optimum. (Sections III and V).

²¹ This price-consumption curve is the sum of the price-consumption curves of all of the X -type individuals. Each after-tax budget line leads to a point on each individual's price-consumption curve. Quantities of heat and bread are summed and these numbers generate a point on the aggregate price-consumption curve. The aggregate curve will have the same general curvature as pp in Figure 4.

²² For an expanded discussion of community indifference curves, see J. de V. Graaff [6, pp. 45-58].

3. With information deficiencies and no legal restriction on pollution (smoke), a government authority set up to offer "bribes" for the reduction of pollution may be unable to make improvements in the Pareto sense. (Section IV).

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URBAN POVERTY AND LABOR FORCE PARTICIPATION

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Research on the determinants of the supply of labor, long a barren area in economics, has recently made major advances as a result of a number of interesting and significant studies [2] [3] [5] [7] [8] [11] [12] [13] [14]. Concurrently, policy makers have moved somewhat from their preoccupation with macro problems, which can be said to have dominated discussion in the early postwar years, to a re-emphasis on the "old" issue of income distribution—appearing now under the mantle of the "war on poverty." The purpose of this paper is to bring together these two streams of intellectual activity by analyzing the significance of patterns of labor force participation for the poverty problem.

More specifically, this paper is concerned primarily with estimating the direction and magnitude of the relation between the overall state of the economy (as approximated by the unemployment rate) and the labor force participation rates of the urban poor. Special attention will be given to the variations in sensitivity of participation rates of married women (with husband present), to differences in non-white and white labor force behavior, and to a comparison of sensitivity (to differences in economic conditions) of participation rates among the poor and among the population at large. A profile of urban poverty in the 52 largest Standard Metropolitan Statistical Areas (SMSA's), based primarily on a racial-age breakdown, will also be presented.

Why should a study of labor force participation among the urban poor be undertaken at the present time? There are several possible reasons. Any short-run model of the economy which attempts to estimate changes in the size of the poverty class as a function of changes in GNP and/or changes in the unemployment rate [1] [4] should include estimates of the sensitivity (to changing economic conditions) of labor force participation rates among the poor. In making projections of the size of the poverty population over the next few years, it is necessary to estimate not only how many presently unemployed poor

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people will become employed but also how many who are not in the labor force at the moment will be drawn into the labor force when the unemployment rate falls.

Along similar lines, as the unemployment rate falls to low levels (3½ per cent or lower), it is important to know how the poor, one readily available source of labor, react in terms of labor force participation to improved economic circumstances. A highly negative relationship between the labor force participation rates of the poor and the overall unemployment rate would imply a large expansion of the labor force and a possible postponement of the emergence of widespread manpower shortages, as the economy moves to lower levels of unemployment. This does not mean that poor people will move into skilled positions. Those who are drawn into the labor force when the unemployment rate falls will undoubtedly take unskilled jobs, which will permit other workers to be upgraded and thus relieve some of the pressures on shortage occupations.

At a more theoretical level, this study of the relation between unemployment and labor force participation of the poor provides an indirect test of two important alternative hypotheses of labor force behavior; the "additional worker" hypothesis and the "discouraged worker" hypothesis.¹ Directly related to this issue of the responsiveness of the secondary labor force to changes in job opportunities is another substantive issue of considerable importance. An argument can be made that one of the most important routes by which an American family moves out of poverty is the acquisition of a job by a secondary worker in the family, i.e., a married woman whose husband is the primary wage earner. One might hypothesize that, as the unemployment rate falls and jobs become more plentiful, married women in poor households headed by a male are drawn into the labor force. Consequently,

¹ The "additional worker" hypothesis postulates that, as the unemployment rate rises, overall labor force participation will rise. Thus, if this hypothesis is true, the withdrawal from the labor force of an unemployed head of household is offset by the entry into the labor force of secondary earners (wife, teenage son, etc.) who are now working in order to supplement the household income. Conversely, the "discouraged worker" hypothesis postulates that labor force participation will fall as the unemployment rate rises for prolonged unemployment causes large-scale withdrawal from the labor force and thus a consequent decline in the overall labor force participation rate. Those who argue that the "additional worker" hypothesis best describes labor force behavior contend that *on balance* the "additional worker" effect offsets the "discouraged worker" effect as the unemployment rate rises. Potentially, the best data for a test of these two hypotheses are gross labor force change data, derived from the monthly *Current Population Survey*. Unfortunately, because of a variety of reasons (e.g., non-offsetting response errors) it is very difficult to establish reliable inferences about net changes from the gross data. The civilian labor force participation rate of a particular subgroup of the population is computed by dividing the total labor force of that subgroup (which equals all those employed plus those unemployed) by the total civilian population of that subgroup.

the probability that this now multiple earner household will remain in poverty declines rapidly.² Conversely, it has been argued that, when the unemployment rate rises in a particular area and the male head of a poor household is laid off, his wife will enter the labor force to acquire a job in order to supplement the family income. The latter argument implies a positive relationship between the labor force participation rates of females in poor households and the unemployment rate. In the light of this last series of observations, this paper will devote a good deal of attention to females, especially married women with husband present.

I. *The Framework of This Study*

This study consists of a cross-section analysis of labor force participation rates in the poverty areas of the 52 largest SMSA's. Presently, there exist no systematic studies of the sensitivity of labor force participation rates among the poor to changing overall economic conditions. The reason, no doubt, is that the requisite data are very hard to obtain. The monthly Current Population Survey data are not classified in such a way as to permit this kind of analysis; nor are the decennial Census results for SMSA's or for states well suited for this purpose. The best sources of data are the *Census Tract* reports of the 1960 Decennial Census which provide a wealth of socio-economic data for carefully defined geographic areas within each SMSA. Information on median income of families and unrelated individuals, age, sex, race, marital status, educational attainment, and labor force status is provided. Unfortunately, the tabular form in which much of the data are presented in the tracts precluded their use in this study. Cross-tabulations of the tract data, appropriate for a study of this kind, are simply not available.

The data on labor force status apply to all males and females (14 years of age and over) and to married women with husband present. Thus, there is no information in the tracts on labor force status by sex and race for various age categories. However, I subdivided the poverty tracts according to the percentage of non-whites who resided in a particular tract and thus reasonably accurate tests could be performed on the basis of a racial breakdown. We computed the labor force participation rates for the respective population groups within the slums of each metropolitan area by summing the civilian labor force for the

²In 1963, among all families headed by a male in which there were two earners, the per cent who were poor was 7.9. For all families headed by a male in which there was just one earner, the per cent who were poor according to the Social Security Administration's index of poverty was 13.4. These proportions were calculated from data presented by Orshansky in [9].

specified poverty tracts within each SMSA and dividing it by the appropriate civilian population figure. The magnitude of this task can be illustrated by the fact that there are 1,400 poverty tracts in the 52 SMSA's analyzed in this study. New York City alone has 167 poverty tracts according to the poverty definition which was used.

How were the urban poverty areas, ghettos if you will, identified? A somewhat arbitrary but defensible method was used. Those census tracts which had a median family income less than two-thirds of the median family income for the entire SMSA were regarded as poverty areas. The advantage in using a certain percentage of the SMSA's median family income as the poverty cut-off level is that one largely avoids the problems associated with differences in costs-of-living among the various metropolitan areas which are inherent if a particular absolute income level is used as the cut-off point. At one point I used a cut-off income for each tract equal to $\frac{1}{2}$ of the SMSA's median family income. When this lower income level was used as the dividing line, there was a large number of metropolitan areas with few or even no poverty tracts. Indeed, even when the two-thirds level was used, many of the smaller metropolitan areas had very few tracts which could be called poverty areas according to the definition. I largely eliminated this disadvantage of the definition by restricting the final analysis to just those SMSA's (52 in number) which had a total population of 500,000 or more in 1960.³ Although some urban low-income areas were excluded by the application of our definition, there is every reason to believe that the vast majority of urban slum areas are included in this study.

Within each metropolitan area, the poverty tracts were classified in the following manner. (1) All poverty tracts in which the non-white population was greater than 50 per cent of the tract's total population were grouped together and hereafter are referred to as the *N tracts*. (2) All poverty tracts in which the percentage of non-whites was less than 50 per cent of the tract's total population but greater than the percentage of non-whites in the entire SMSA constituted another grouping and hereafter are referred to as *N* tracts*. (3) The remaining poverty tracts, the residual after (1) and (2) are deducted from the total, are the white poverty areas and are referred to as *W tracts*.

The N and W tracts permit us to examine the labor force sensitivity of the urban poor on the basis of a racial breakdown. Although the N tracts do contain whites and the W tracts non-whites, the distortion which is introduced is a small one. Indeed, the vast majority of the N tracts had a non-white population which was 75 per cent or more of

³ Honolulu, on account of the special nature of its labor force and its location, was not included in our study.

the tract's total population. The N* tracts permit us to analyze the sensitivity of labor force participation in racially mixed neighborhoods.

II. *The Results*

A. *A Profile of Urban Poverty*

The first "output" of this study is a breakdown of the poverty areas in the 52 SMSA's along racial lines.⁴ Since these poverty areas, which provided the raw material for the regression analyses in this study, are of some independent interest, they are briefly summarized here. Fourteen hundred poverty tracts were analyzed and included in this study. Of the 1400 tracts, 842 had a population which was over 50 per cent non-white. There were only 248 W tracts. The remainder (310) constituted the N* tracts. The number of poverty tracts in which the non-white population was 50 per cent or more of the population was more than 50 per cent of all poverty tracts in 31 of the 52 SMSA's. Those SMSA's which had a relatively large number of poverty tracts in which the non-white population was greater than 50 per cent of the tract's total population were mainly those metropolitan areas which had a large non-white population (Baltimore, Birmingham, Chicago, Cincinnati, Cleveland, Memphis, New Orleans, St. Louis, and Washington, D.C.). Milwaukee with a relatively small non-white population (5.3 per cent of the total) experienced an enormous increase in its non-white population between 1950 and 1960 (185.5 per cent).

The areas that had a relatively small number of poverty tracts in which the non-white population was more than 50 per cent of the tract's total population are largely in the Far West or the South West (San Antonio, Phoenix, San Diego, San Jose, Sacramento, Seattle, etc.). Here, the poverty problem in ethnic terms is largely a Mexican-American problem. Mexicans are listed as whites in the Census.

When we "plowed" through the tracts, we did record the median age of the male and female population in each tract. Forty-five of the fifty-two SMSA's had white poverty tracts. The median age of the median ages for the poverty tracts in 21 of these 45 SMSA's was over 40. Thus, the urban poor population in America's largest cities consists mainly of non-whites, Mexican-Americans, and elderly whites.

The mean labor force participation rates of all males, all females, and married women (husband present) in the poverty tracts followed a definite pattern. The mean labor force participation rates of each of

⁴Mimeographed tables, which list the number of poverty tracts for each SMSA, a breakdown of the poverty tracts by race, the range of cutoff median incomes, the percentage of non-whites in the poverty tracts in each SMSA, and a variety of other information are available from the author, Industrial Relations Section, Princeton University, Princeton, New Jersey, 08540.

the non-white groups (N Tracts) were substantially higher than those of the white groups (W Tracts). For example, in the N tracts the overall mean labor force participation rates of all males and all females were 72.8 and 41.1 respectively. In the W tracts, the rates for all males and all females were 67.3 and 35.2 respectively. The higher labor force participation rates of poor non-white females relative to poor white females is not surprising and parallels the national situation. What is surprising is the fact that poor non-white males have a higher labor force participation rate than poor white males. This differential is explained in part by the fact that many urban white poor are over 65 and

TABLE 1—RESULTS OF SIMPLE REGRESSIONS FOR THE MALE POVERTY POPULATION IN ALL SMSA'S OVER 500,000 IN POPULATION

| Type of Poverty Tract ^a | All Males 14+ | | |
|------------------------------------|------------------------------------|-----------|-------------------|
| | X_1 = The SMSA Unemployment Rate | | |
| | Regression Coefficient (b) | "p" Value | (R ²) |
| N Tracts (n=45) | -1.27 ^b | 2.48 | .13 |
| N* Tracts (n=49) | - .72 | .67 | .01 |
| W Tracts (n=45) | -2.12 | 1.75 | .07 |
| All Poverty Tracts (n=52) | -1.47 ^b | 2.25 | .09 |

^a Explained in text above.

^b Significant at the .05 level.

thus out of the labor force. Other factors which are subsequently discussed may also explain this differential.

B. Sensitivity of Male Participation Rates

Because males between the ages of 25 and 54 form such a large fraction of the total male labor force and because this group tends to be in the labor force come good times or bad, it would be surprising if the variable (the SMSA unemployment rate)⁵ used in this study explained very much of the inter-SMSA differences in the labor force participation rate of males (14 years of age and older). Table 1 presents the results for all males living in the poverty areas in the 52 SMSA's with

⁵ This is the unemployment rate in the SMSA during the census month in 1960. The SMSA unemployment rate is used because it is the "best" single indicator of economic conditions in a particular area.

500,000 or more in population. The form of the simple regression was:

$$(1) \quad Y_i = a + bX_1$$

Y_i = the labor force participation of the i th group
(e.g., all males (14+) living in N tracts)
 X_1 = the SMSA unemployment rate.

A weak but statistically significant negative relationship appears for all males living in N tracts. This same pattern is reflected in the results for all poverty tracts (= the sum of the N, N*, and W tracts).

TABLE 2—RESULTS OF SIMPLE REGRESSIONS FOR THE FEMALE POVERTY POPULATION IN ALL SMSA'S OVER 500,000 IN POPULATION

| Type of Poverty Tract ^a | All Females (14+) | | | Married Women (Husband Present) | | |
|------------------------------------|----------------------------------|--------------|-------------------|------------------------------------|--------------|-------------------|
| | Regression Coefficient (b) | "t" Value | (R ²) | Regression Coefficient (b) | "t" Value | (R ²) |
| N Tracts (n=45) | -3.61 ^b | 4.6 | .33 | -4.52 ^b | 5.7 | .43 |
| N* Tracts (n=49) | -2.55 ^a | 2.63 | .12 | -1.84 ^a | 2.39 | .11 |
| W Tracts (n=45) | -1.54 | 1.65 | .06 | -1.59 | 1.75 | .07 |
| All Poverty Tracts (n=52) | -3.14 ^b | 4.81 | .32 | -3.09 ^b | 4.3 | .27 |

^a Explained above in text.

^b Significant at the .01 level.

^c Significant at the .05 level.

Simple regressions in the same form were also run for *all males* by race living in the same 52 SMSA's. Although once again the signs of the regression coefficients were negative, none of them was significant at the .05 level—not an unexpected result. One tentative conclusion based on these findings is that the sensitivity to changing economic conditions of the labor force participation rate of low income urban male workers tends to be greater than that of the average urban male worker.

C. Sensitivity of Female Participation Rates

In this section, the sensitivity of the labor force participation rates of all poor females (14+) and subgroup, married women with husband present, will be analyzed. The results presented below in Table 2 are those obtained from simple regressions of the form:

- (2) $Y_i = a + bX_i$
 Y_i = the labor force participation rate of the i th group
 (e.g., all females (14 +) living in N tracts)
 X_i = the SMSA unemployment rate.

An examination of the results in Table 2 reveals an interesting pattern. The most significant relationship exists for non-white married women with husband present. A highly significant relationship also exists for all non-white females, a group which includes, of course, non-white married women (with husband present) as a subset. A regression coefficient of -4.52 implies that an increase of one percentage point in the overall unemployment rate of the SMSA leads to a decline of $4\frac{1}{2}$ percentage points in the labor force participation rate of non-white married women (with husband present). This result can be construed not merely as inconsistent with the "additional worker" hypothesis but as strong support for the "discouraged worker" hypothesis, at least with reference to non-white married women (with husband present). In other words, although there may be some non-white females who enter the labor force when the unemployment rate rises, on balance they are more than offset by the discouraged females who withdraw from the labor force when the unemployment rate rises.

An examination of the mixed tracts and the white poverty tracts reveals that the sizes of the regression coefficients and the R^2 terms decline sharply. On the basis of a comparison between the mixed tracts and the white tracts, one possible conclusion is that the reduced significance of the relationship for the mixed tracts is a result of the increase in size of urban white poor within this classification.⁶ This latter observation suggests the following question: Why are the labor force participation rates of urban white poor females and married

⁶ Another way of depicting the influence of the size of the non-white poor population on the sensitivity of the labor force participation rate of all poor females is by means of an interactive model. One such model is the following:

$$Y = a + b \left(c + d \frac{N}{P} \right) X.$$

Y = labor force participation rate of all poor females in each SMSA; N/P = per cent of the total poor population in each SMSA who are non-white; X = SMSA unemployment rate. When this model was tested, the result was:

$$Y = .53 - 3.46 X + .90 \left(\frac{N}{P} X \right). \quad R^2 = .36$$

(.66) (.45)

Thus, the change in the labor force participation rate of all poor females with respect to a change in the SMSA unemployment rate is a positive function of the per cent of the poor population who are non-white. This checks with the results presented above and provides strong support for them.

women less sensitive to changes in economic circumstances than those of non-white females? At least two reasons are possible.

1. The poor white females are much older on the average than their non-white counterparts. A significant number of all urban white adult poor are over 65 and of this age group the vast majority are females.⁷ Presumably, many of this latter group have permanently withdrawn from the labor force and thus their labor force status is virtually insensitive to changes in economic circumstances.⁸

2. Another explanation pertains to a possible but speculative characterization of urban white poor. Unlike the non-white population, the white population in this country has not been subjected to over 300 years of continual discrimination, injustice, and denial of equal opportunity. The question naturally arises—why was the white population in this study unable to take advantage of the opportunities afforded them? Some obviously were the victims of temporary economic setbacks—for example, laid off auto workers in Detroit, or laid off defense workers in San Diego. The poverty of this group is quite likely of short duration and it does not really belong in this study. This is not meant to minimize the hardships associated with this type of economic calamity. Poverty and lack of employment among other urban whites, however, may have been caused by a more complex set of circumstances. Low I.Q., physical disabilities, mental illness, alcoholism, and narcotics addiction are a few of the debilitating factors which come to mind. Of course, non-whites are not strangers to these sundry disabilities but their incidence may be higher among poor urban

⁷ A special tabulation of the poverty population, based on the March 1965 *Current Population Survey*, discloses that (i) of the total number of non-farm white poor over 22 years of age, 37 per cent were over 65, (ii) of the total number of unrelated individuals who were white, poor, and over 65, almost 80 per cent were females (1.9 million out of a total of 2.4 million persons). These data were provided by the Office of Research, Program Planning, and Evaluation in the Office of Economic Opportunity.

⁸ The search for a suitable age variable for the regression analysis in this study was unsuccessful. The only age measure which is available from the tracts in a usable form is median age broken down by race and sex. The non-white median ages fall within a narrow range, largely within the 22-30 age bracket, and thus would be of little value in attempting to explain inter-SMSA differences in labor force participation of non-whites in N tracts. An aggregation problem affected the N* tracts. The N* tracts, consisting of younger non-whites and older whites often in roughly equal numbers, likewise did not present a great deal of dispersion in their overall median ages. Since the N* tracts do consist of whites and non-whites, it would not be legitimate to use either the white or non-white median age alone to test for differences in labor force participation. For white females in the W tracts, the range of median ages was very broad (18.2 in San Antonio to 69.9 in Tampa-St. Petersburg). However the relationship between age and white female labor force participation is bimodal in nature. The labor force participation rate for all white females peaks at the age of 20 or thereabouts, declines until the mid-30's, and then rises to another peak around age 50. The upshot of all this is that a linear regression with the white female labor force participation rate as the dependent variable and median age as an independent variable would be of little interest and very difficult to interpret.

whites. If a population subgroup were subject to one or a combination of these factors, the insensitivity of their labor force participation rates to changing economic conditions is explicable. The prospect of a job is not sufficient to draw them back into the labor force. Indeed, a job alone is not the major solution for their poverty-related problems.

This admittedly speculative explanation is indirectly borne out by Locke Anderson's study [1, p. 523] which analyzes changes in median family income of different family types as a function of changes in aggregate income. Anderson finds that the median income of non-white, non-farm families headed by a male < 65 has a high elasticity with respect to aggregate income, whereas the same elasticity for white non-farm families headed by a male < 65 is much lower. The insensitivity of the labor force participation rates of white urban poor females to changing economic conditions lends support to Anderson's findings.

In the results presented below in Table 3, the analysis has been broadened to include a demand variable.⁹ This demand variable based on the distribution of female employment among various industries, is designed to take into account the differences among local labor markets in their demand for female labor. A local labor market such as Pittsburgh with a heavy industry base has a rather low demand for female labor (the demand variable for Pittsburgh equals .293) whereas Washington, D.C. with its government employment base has a much higher demand for female labor (the demand variable for Washington, D.C. equals .382). One long-run effect of a situation such as Pittsburgh's should be noted. If the local industries hire only males and thus create a shortage of jobs for women, the surplus pool of female labor may tend to attract industries with complementary labor demands. The classic examples of this phenomenon are the silk mills in Pennsylvania coal towns and shoe and textile industries in the New England port towns [15, p. 42].

Table 3 presents the results of the multiple regressions for the pov-

⁹ For a complete explanation of the construction of this demand variable, see W. G. Bowen and T. A. Finegan [2, pp. 157-58]. In their own words—"We calculated the ratio of female employment to total employment in each of some 35 two- and three-digit industries for the United States as a whole. Taken together, these industries contained all the employed persons in the civilian labor force during the census week. Second, for each individual metropolitan area, we multiplied the total employment (of both sexes) in each industry group by the national female-employment ratio for that group as calculated in step one. Finally, the products obtained by step two for each city were summed and divided by the total civilian employment in the city. Each quotient is simply a weighted average of the national female employment ratios for the industries in each city. What it shows is the female employment ratio we would predict each city to have in a given year if this ratio depended only on the city's industry mix. The actual ratio, of course, will be affected by many other factors." Professors Bowen and Finegan were kind enough to put this index at my disposal.

TABLE 3—RESULTS OF MULTIPLE REGRESSIONS FOR THE FEMALE POVERTY POPULATION IN ALL SMSA'S OVER 500,000 IN POPULATION

| Type of Poverty Tract ^a | All Females (14+) | | | | |
|------------------------------------|---|--------------|---|--------------|-------------------|
| | X_1 =SMSA Unemployment Rate | | X_2 =The Demand Variable | | |
| | Net Partial Regression Coefficient (b) | "p" Value | Net Partial Regression Coefficient (c) | "p" Value | (R ²) |
| N Tracts (n=45) | -2.26 ^b | 2.76 | +1.32 ^b | 3.3 | .47 |
| N* Tracts (n=49) | -2.57 ^c | 2.21 | +.01 | .02 | .13 |
| W Tracts (n=45) | -2.08 | 1.92 | -.60 | .99 | .08 |
| All Poverty Tracts (n=52) | -2.67 ^b | 3.57 | +.47 | 1.24 | .34 |
| | Married Women (Husband Present) | | | | |
| | | | | | |
| | | | | | |
| N Tracts (n=45) | -3.63 ^b | 4.1 | +.87 ^c | 2.0 | .48 |
| N* Tracts (n=49) | -1.39 | 1.51 | +.42 | .93 | .12 |
| W Tracts (n=45) | -2.36 ^c | 2.3 | -.85 | 1.46 | .11 |
| All Poverty Tracts (n=52) | -2.93 ^b | 3.5 | +.16 | .38 | .27 |

^a Explained above in text.^b Significant at the .01 level.^c Significant at the .05 level.

erty population in the 52 largest SMSA's. The regressions were of the following form:

$$(3) \quad Y_i = a + bX_1 + cX_2$$

Y_i = the labor force participation rate of the i th group
(e.g., all females (14 +) living in N tracts)
 X_1 = SMSA unemployment rate
 X_2 = the demand variable.

The addition of the demand variable significantly improves the simple relationship in two cases—all females (14+) in N tracts and married women (with husband present) in N tracts. The R^2 for all females in N tracts increases from .33 to .47 and for married women (husband present) from .43 to .48. The pattern which was evident in the simple regressions reappears. Although the addition of the demand variable has reduced somewhat the size of the negative regression coefficient of the SMSA unemployment rate for females (14+) and married women

(husband present) in N tracts, in both cases the coefficient is still significant (at the .01 level) and quite high. The demand variable for these latter two groups is also significant at the .01 level and .05 level respectively.

In the N* tracts and the W tracts, the amount of variance explained by our regressions, as indicated by the R^2 terms, is very small. Although the regression coefficients of the unemployment rate are significant at the .05 level in a couple of cases—all females living in N* tracts and married women (husband present) in W tracts, the demand variable is negative for both groups in the W tracts and the R^2 's insignificant at the .05 level. The fact that the coefficients of the demand variables for poor white females and married women in W tracts have negative signs is consistent with the view presented in this paper that many of them are for one reason or another (age, debilitating personal attributes, etc.) largely bypassed by economic progress. The contrast between the results for the N tracts and the result for the N* and W tracts is simply striking.

In order to provide a reference point for comparison purposes, exactly the same regressions were run for all females (14+), non-white females (14+), white females (14+), and married women (husband present), all non-white married women (husband present), and all white married women (husband present) in the 52 largest SMSA's. These groups naturally include the poverty populations analyzed above. The results are presented in Table 4. The fit is consistently better for these groups than for comparable groups within the poverty population. The regression coefficients of the unemployment rate are always negative and significant at the .01 or .05 level in every case, save all white females (14+). There can be little doubt that the discouraged worker hypothesis also describes the labor force behavior pattern of the general populations as well. Note, however, that the negative regression coefficients of the unemployment rate for the poverty populations, especially the non-white groups, are larger in almost every case than the same coefficients for the population as a whole. It seems clear that the low income worker is more likely to be a discouraged worker than an additional worker especially if "she" is a non-white married woman with husband present.

III. *Conclusions*

Before drawing any conclusions from the results presented above, at least a few caveats should be posted. There has been no attempt to introduce into our analysis a control variable such as the number of teenagers (by sex and race) enrolled in school. Exactly 1,400 poverty tracts would have to be analyzed and the computation of these variables simply lies beyond the ambitions of this study. Furthermore, no account has been taken of the role of migration in affecting labor force

TABLE 4—RESULTS OF MULTIPLE REGRESSIONS FOR THE ENTIRE FEMALE POPULATION IN ALL SMSA'S OVER 500,000 IN POPULATION^a

| Population Groups | X_1 =SMSA Unemployment Rate | | X_2 =The Demand Variable | | |
|---|---|--------------|---|--------------|-------------------|
| | Net Partial Regression Coefficient (b) | "t" Value | Net Partial Regression Coefficient (c) | "t" Value | (R ²) |
| Non-white females 14+ | -1.61 ^c | 2.54 | +1.40 ^b | 4.43 | .51 |
| White females 14+ | -1.12 | 1.84 | +.35 | 1.16 | .16 |
| All females 14+ | -1.45 ^b | 4.15 | +.41 ^c | 2.35 | .47 |
| Non-white married women (husband present) | -2.1 ^b | 2.87 | +1.1 ^b | 3.0 | .46 |
| White married women (husband present) | -1.85 ^b | 3.37 | +.003 | .01 | .27 |
| All married women (husband present) | -1.83 ^b | 3.6 | +.14 | .6 | .30 |

^a The number of observations in the regressions for non-white married women and white married women (both with husbands present) was 47. For the other four regressions, the number was 52.

^b Significant at the .01 level.

^c Significant at the .05 level.

Sources: The population and labor force data used in these regressions were obtained from U.S. *Census of Population 1960*, "General Social and Economic Characteristics by State," PC (1) 6 (c) and U.S. *Census of Population 1960*, "Detailed Characteristics by State," PC (1) 32 (D).

participation rates [8, pp. 80-81]. Clearly, migration to areas of low unemployment rates is an alternative to withdrawal from the labor force in areas of high unemployment rates. In the light of this, it is quite probable that our estimates of the sensitivity of labor force participation rates are too high. It would be desirable to include in our analysis an appropriate migration variable. However, the task of constructing appropriate net and/or gross migration rates for specific groups by age, sex, race, and marital status, which would be applicable to the poverty areas in each of 52 SMSA's, would be a major undertaking in itself. Let it suffice to say that, if labor market theory has anything valid to say about population movements, then the regression coefficients for the unemployment rates are biased upwards somewhat.

An additional qualification should be noted. The cross-sectional analyses in this study probably represent the accumulation of labor force adjustments to long-range differences in economic opportunity

and social factors in addition to short-run cyclical variations. Thus it is not entirely legitimate to use the estimates of labor force sensitivity presented in this study to project short-run changes in labor force behavior over the cycle. Nevertheless, there is a certain amount of support in the time series information for the results presented herein.¹⁰ For example, from 1961 to 1965, the annual average total unemployment rate fell from 6.7 per cent to 4.6 per cent. Over the same period, the labor force participation rates of all non-white females (ages 20-64) rose from 54.3 to 56.2. The labor force behavior of non-white females (ages 20-34) provides more compelling support for the results presented herein. From 1961 to 1965, the labor force participation rate of non-white females (ages 20-34) rose from 50.0 to 54.4. On the other hand, if one searches the time series data carefully over a longer period, one can find some evidence of counter-cyclical labor force behavior of non-white females.

With these qualifications in mind, the basic conclusion which can be drawn from the above findings is that the average non-white family with both husband and wife present attempts to lift itself out of poverty during prosperous times by becoming a multiple-earner family. The high sensitivity of the labor force participation rate of non-white married women with husband present means that, as the unemployment rate falls, they enter the labor market to secure a job—either part-time or full-time. By our estimates, their labor force participation rate rises by approximately three percentage points with a one percentage point fall in the unemployment rate. During the day, young children presumably are left with relatives or at day care centers, while the mother goes to work. If their husbands are presently working, their wages rise somewhat during the course of an upswing in economic activity. Thus, the combination of rising wages on the part of the employed husband and increased labor force participation on the part of the wife act to lift the non-white urban family above something called a "poverty line."

For all non-white females (those living in N tracts), the negative

¹⁰ One recent example of a sophisticated time series study was done by Dernburg and Strand [12, pp. 71-96]. Using an employment-population ratio and an unemployment compensation exhaustion ratio as independent variables, Dernburg and Strand estimate the elasticities of labor force-population ratios of particular groups with respect to the overall employment-population ratio. They estimate a full-employment labor force based on a "conservative" definition of full employment equal to a 4 per cent unemployment rate. Using their full-employment labor force estimate, they are able to estimate hidden unemployment equal to the estimated full-employment labor force minus actual unemployment. The finding of the Dernburg and Strand study which bears directly on this study is the higher elasticities or higher sensitivities of the adult female labor force with respect to total employment relative to the adult males. However, Dernburg and Strand do not classify the labor force according to color or income level, both of which are of direct concern to this study.

regression coefficient of the SMSA unemployment rate is less than the same regression coefficient for non-white married women (in the N tracts) in both the simple and multiple regressions. The "discouraged worker" effect is clearly much stronger for non-white married women (with husband present) than it is for all non-white females. One explanation for this result may lie in the fact that a large number of non-white females over 14 years of age who are not married women with husband present are female heads of families and many of this latter group are on the public assistance rolls. Under the existing means tests for recipients of public assistance, the amount of assistance is an estimate of minimal needs minus any earnings which the family makes. The upshot of this means test is that it taxes earnings at 100 per cent. Thus, public assistance contains a strong built-in disincentive to work. Consequently, it is not surprising that a tightening labor market does not necessarily induce them to enter the labor market.

The "backwash" thesis concerning poverty in America asserts that the economy may prosper and grow as in the past but the "other America," a hard core of poor beyond the reach of a growing economy, will be stranded behind and thus bypassed. On the basis of the findings in this study, the "backwash" thesis may have some relevance as far as poor urban white families are concerned but it does not apply to poor urban non-white families with both parents present. A tighter labor market not only means better jobs, higher wages, and longer hours for workers, but it also draws workers into the labor force, especially non-white females who were not in the labor force when prospects for finding a job were much dimmer.

Finally, it has been said that real improvement in the economic status of Negro men and in the stability of Negro families would probably lead to a reduction in labor force participation by Negro women [16, p. 882]. A "real improvement," say a 5- to 10-year period with the non-white male unemployment rate 4 per cent or lower, would probably lead to a reduction in the very high labor force participation rates of non-white women. Over the range of unemployment rates encompassed by this cross-sectional study (largely between 8 per cent and 3.5 per cent) there is no evidence, however, that Negro women withdraw from the labor force as general business conditions improve. We will simply have to wait until that state of true full employment is reached and *maintained* to test for the other possibility.

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MIXED PUBLIC AND PRIVATE FINANCING OF EDUCATION: EFFICIENCY AND FEASIBILITY

By MARK V. PAULY*

Governmental support for basic education is rarely questioned; this consensus has long been institutionalized in the American public school system which, ideally, provides each child with access to *equal* publicly supported facilities. Questions have been raised concerning the efficiency of organization, and public operation has been criticized. No one has demonstrated, however, that there are inefficiencies inherent in *equal* provision of public support to each student, whether facilities be publicly or privately operated. This paper will show that, if efficiency considerations are controlling, ethical canons of distribution being neglected, *unequal* public support is necessary under most plausible circumstances. The argument does not depend on differentiation of children according to ability to learn; in the analysis all children are assumed equally capable of benefiting from education. The argument depends critically on the distinction between marginal and inframarginal externalities.

I

In the modern theory of public finance, a pure public good is defined to be a good every unit of which produced is equally available for consumption by all [11]. A necessary condition for optimality in the production of such a good is:

$$(1) \quad \sum_{i=1}^m u_p^i / u_r^i = F_p / F_r$$

where the term on the lefthand side of the equality represents the summation (over all m persons in the community) of the ratios of each person's marginal utility from public good X_p to his marginal utility from some private (numeraire) good, and the term on the righthand side represents the ratio of the marginal cost of the public good to that of the numeraire good. That is, condition (1) states that the marginal rates of substitution summed over all persons must equal the marginal rate of transformation, or, more crudely, that the summed marginal evaluations must equal the marginal cost.

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It is clear that such services as education do not fall under the strict Samuelson definition of a public good. Full exclusion is possible; a desk occupied by one child cannot be occupied by another. Education is provided or financed publicly, therefore, not because the technical considerations of production make it difficult to exclude others from the consumption of it, but rather because persons other than the direct consumers of education (the child, or, more generally, his family unit) derive benefit from that child's consumption of educational services.

This idea is usually expressed by saying that some kinds of education—citizenship or literacy-oriented education—generate spillover or neighborhood effects which benefit persons other than the direct recipients of the education.¹ While “everybody stands to gain from living in a more educated community,” the child or his family clearly benefits more from his consumption of educational services than does anyone else.² The basic reason for public provision of education is not that it is impossible to exclude anyone from the consumption of educational services, but that consumption of such services by any child confers benefits on everyone else; the external economies are those arising from consumption per se.

This type of externality in consumption requires a utility function (assumed, for simplicity, to be attached to families) in which there enter as arguments not only the consumption of education and all other goods by the family itself, but also the consumption of education by *all other* families in the community. If X_{pi} represents the amount of educational services consumed by the i th family, a typical utility function for the i th family consuming n goods plus education in a community of m persons is as follows:

$$(2) \quad U^i = U^i(X_{1i}, X_{2i}, \dots, X_{ni}; X_{p1}, \dots, X_{pi}, \dots, X_{pm}).$$

Note that the consumption of education by each one of the other families constitutes a separate “public good” in family i 's utility function.³

Each family thus has a potentially positive marginal rate of substitution between another family's consumption of education and the goods it consumes itself. The optimality condition (similar to condition (1)) is that these marginal rates of substitution summed over all families must

¹ See the discussion by Milton Friedman [5] [6, pp. 85–98] and others [10] [12]. B. A. Weisbrod also discusses spillover effects [13], but he is mainly concerned with those of a geographical nature.

² R. A. Musgrave [9, p. 13] considers education to be a “merit want” that involves substantial elements of social wants, but it is difficult to be sure just exactly what he means by a merit want. See also Head's development [7].

³ This procedure allows all “impure” public goods and even purely private goods to be analyzed as public goods. It has been developed by James M. Buchanan in his manuscript [1].

equal the marginal rate of transformation. That is, for the good X_{pj} , the education of the j th child, optimality requires that the following condition holds:

$$(3) \quad \sum_{i=1}^m u_{pi}^i / u_r^i = F_{pi} / F_r.$$

Generally, family j will have the highest marginal rate of substitution between child j 's education and the numeraire good (that is, the highest marginal evaluation of the education of child j), but other families as well may have a positive marginal evaluation of child j 's education. A positive marginal evaluation for other families indicates that, if provision of child j 's education is left solely to family j , marginal external benefits to other families may exist which could be obtained by some community provision of education to child j .

II

The procedure for conceptually determining the optimum amount of community payments for the education of any child would appear simple. Given the amount of education provided by the parents, if the rest of the community evaluates positively an additional unit of education to a child, the community should provide a subsidy for education, an addition to the parents' expenditure, until the education of that child is extended up to the point at which condition (3) above is satisfied.

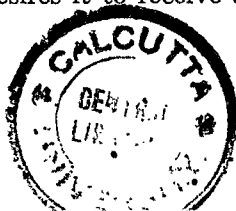
Satisfying this optimality condition is far from simple, however, because the externality generated by consumption per se of education is what has been termed a "reciprocal externality" [2]. That is, the amount of benefit an additional unit of education for child j would confer on the rest of the community depends on how much education family j has already provided for its child on its own, and at the same time family j 's decision whether to make expenditures for additional education for its child depends on how much the community has spent. The presence of this sort of externality raises the possibility of the non-existence of equilibrium [4], and of the nonexistence of a uniquely optimal result. The outcome depends on the bargaining skill, the adroitness in concealing preferences, on the parts of family j and of the community.

This problem can be illustrated as follows: Suppose the community initially provides zero educational services. The family units will allocate their budgets among education and all other goods in such a way that they maximize their utility. Education will be provided for each child by his own family up to the point at which $u_{pj}^j / u_r^j = F_{pj} / F_r$. Assume that education is a normal good, and that all parents of equal income levels wish to provide equal amounts of education for their children

(that is, identity of tastes as regards education), so that families with low incomes will be consuming small amounts of education, and families with large incomes large amounts. If the community now "wakes up" to the possibility of obtaining benefits from the provision of additional education, the benefit to the community (that is, the marginal evaluations summed over all families except family j) for expenditures on additional education to be provided to child j will be greater the smaller the amount of educational expenditure made by child j 's parents, so long as the community's demand or marginal evaluation curve is negatively sloped. It is possible that some parents with large incomes will have made on their own large enough expenditures on education so that the community evaluates additional education for their child at zero. In such a case, though provision of education to a child confers benefit on the community, it does so only inframarginally. However, for those parents with low incomes who provided only small amounts of education (possibly zero) for their children, the public supplement will be large, and will diminish as the parents' income increases. In such a case, the externalities are marginal. But this is an equilibrium situation only if the parents continue to provide for their children the amount of education they would have provided when community expenditures were zero, which is unlikely—the education provided by the community will doubtless substitute for some of the education provided by the parents.

If, however, we suppose that all parents initially provide no education for their children, then the amount the community will provide in maximizing its utility will be the same for every child. The value to the community of one literate and patriotic citizen is about equal to the value of any other. This equal provision will be optimal only if the parents cannot or will not provide education on their own. If parents will provide supplements to the state financed amounts, to allow such payments is clearly Pareto-optimal, since only those parents making the expenditures need bear the incremental costs. If institutional arrangements are such as to make such supplementary financing extremely difficult, inefficiency clearly characterizes the result. Such a situation approximately corresponds to that of the present public school system, in which socialization of the schools (and consequent public aid only to public schools) has resulted in such high cost in terms of public aid lost to parents who would wish to supplement their child's education (and who can do so only by sending him to a nonpublic school) that most do not do so.

The inference here might seem to be that optimality could be introduced by allowing each parent who desires it to receive a voucher of



value equal to the per-pupil expenditure within the public school system, which voucher would be of the same size to all parents.⁴ If such vouchers were given, each family would extend its own expenditures up to the point at which its own marginal evaluation of an additional unit of education for its child (given the total amount provided by it and by the community) equalled the marginal cost. But since the community's marginal evaluation, though lower than it was when family expenditures were zero, may very well still be positive, this position may still be nonoptimal

$$\left(\text{that is, } \sum_{i=1}^m ME > MC \right).$$

Moreover, the community's evaluation of an additional unit of education to any family will have fallen more the more expenditure for education that family had made on its own, and the community will readjust its own payments accordingly.

Whether we begin with level of parental expenditure when community expenditure is restricted to zero, or with level of community expenditure when parental expenditure is restricted to zero, removal of the restriction, and consequent increase in community expenditure in the first case, and in parental expenditure in the second, will lead to a reduction in expenditure by the other party. Hence, we can conclude that simple removal of the restriction on community or parental expenditure (by a voucher scheme, for instance) will not lead to a Pareto optimum, but will lead to a further process of adjustment and readjustment on the parts of community and parents. It is impossible to specify the optimal amount of community expenditure for any child's education without knowing the community's and the parents' choice functions, and the types of bargains struck in getting to optimality.

III

By making some assumptions about these choice functions, and by assuming away strategic behavior, we can indicate the nature of an optimal solution. The crucial assumption here will be that the community's demand curve for education is (almost) perfectly inelastic in the relevant range, and that there are zero income effects on the community's choice function from different levels of parental provision. This is equivalent to assuming that families, in their capacity as members of the community, have a minimum or "target" amount of education they

⁴ See, for instance, André Danière's discussion [3]. A voucher system with the vouchers of equal value to all families has actually been used in the State of Virginia for several years, and is described in [15].

wish to see provided to each child—an amount which is considered (a) the minimum necessary for good citizenship and (b) the maximum amount the community would wish to provide for citizenship-oriented education at any positive price.

Refer to Figure 1 below. The solid lines are the result of the following conceptual experiment: Families at different income levels are confronted with varying amounts of community expenditure for their

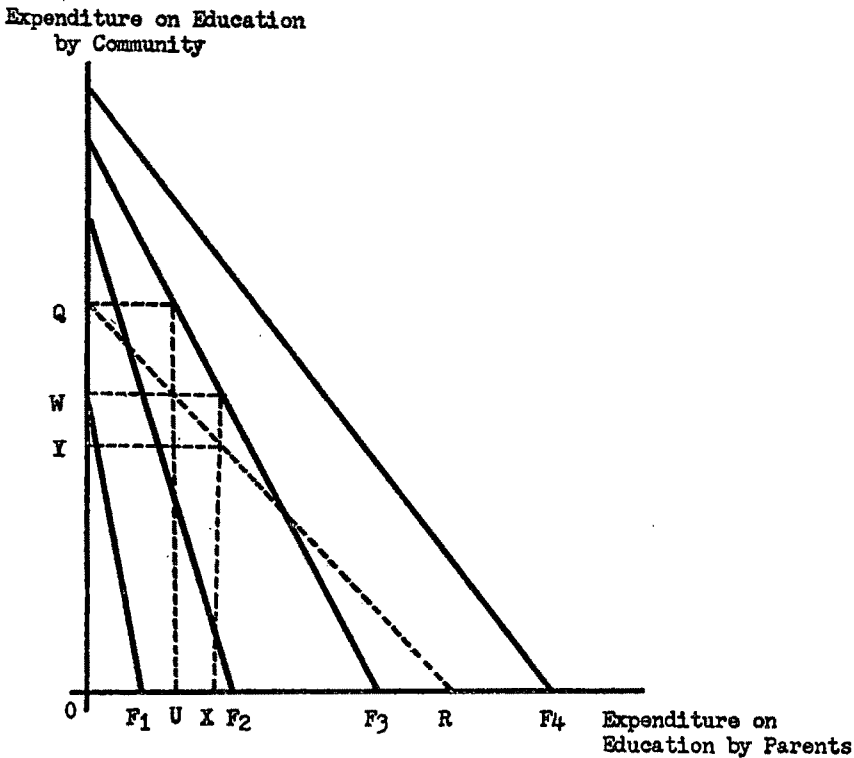


FIGURE 1

children's education (given as lump sums), and are allowed to adjust their own expenditure to maximize their utility. It is assumed that no family is taxed to provide education for its child, that the amount of community expenditure represents the contributions of persons other than the child's parents. The assumption that education is a normal good and that all families have the same tastes for their children's education means that families 1 through 4 are arranged in ascending order of income. These choice lines (drawn linear, though they need not be) must have an absolute slope greater than unity, since each family will

doubtless wish more education when it is provided to it "free" by the community than when it must pay the full marginal cost of it, so long as education is a normal good.

The dashed line QR represents the various outlays on education that will be made by the community for various outlays made by the parents. This line has an absolute slope of one, based on the a priori assumption made above of a "target" amount of education. Thus if the parents spend nothing on their own (the assumption underlying the public school system), the community will spend OQ for each child. If the community spends OQ , but the parents are allowed to provide additional amounts (the voucher system), family 3, for instance, will spend OU . But if family 3 spends OU , then the community should optimally spend only OW ; if the community does in fact spend OW , family 3 will spend OX , and then the community will spend OF , and so forth. A series of adjustments and readjustments by the community and each family occurs, but it can be seen that the process is converging to an equilibrium.

The equilibrium payments by and for each family are given by the intersections of QR with the choice lines for each family. (For family 1 the community would pay the full cost of education; family 4 would pay the full cost on its own.) It can be seen that the community payments will vary inversely with income. These equilibria are also optimal. At all quantities of education less than at the intersections, the marginal evaluations of the community and of the family exceed the marginal cost, so that expansion in quantity is indicated. For quantities of education above the equilibrium amounts the marginal evaluation of the community falls to zero, and that of the family is less than the marginal cost, so that optimality requires a reduction in quantity. Only at the intersections, where the marginal evaluation of the community is zero, and that of the family is equal to marginal cost, is condition (3) above satisfied. For families with incomes high enough that they provide all of the minimum basic education on their own, the community should optimally make zero expenditures.⁵ For families with incomes so low that they make no expenditures on education, no matter what the community does, the optimum payment is the minimum basic amount. For families with incomes in between, the optimum community payment

⁵ E. G. West [14] seems to make the same assumption that was made here—that there is a minimum amount of education in which the community is interested, and that it has zero demand for education for any child beyond that point. He further claims that, in fact, almost all families are in the position of family 4, having incomes sufficiently large that they would provide the minimum basic amount of education if there were no community provision of education to them and if their current taxes were reduced by the amount they now pay for their child's education. He does not recognize, however, that for those who receive community payments, these payments should vary inversely with income. See also Mill's comments [8, pp. 190–91].

should vary inversely with the fraction of the minimum basic amount provided by the parents' expenditure. That is, it should vary inversely with income.

This solution, it must be admitted, arises only because of the peculiar form given to the community's choice function. But since it seems a fair conjecture that the community's demand curve for citizenship-oriented education is very elastic, if not perfectly so, and that income effects of parental expenditures are negligible, this result is interesting.

IV

Let us consider the more general case in which the community's demand curve for education, though downward sloping and probably inelastic, is assumed to depart somewhat from perfect inelasticity, and income effects are positive. A diagram could be drawn for this case in which QR would have an absolute slope of less than unity, and it would again show a series of independent-adjustment equilibria. Again, the equilibrium amount of community provision would vary inversely with income.

But now these equilibria are not optimal. The community and the parents adjust independently until *each* of their marginal evaluations equals the marginal cost. Hence, the sum of these two evaluations

$$\left(\sum_{i=1}^m u_{pm}^i / u_r^i \right)$$

exceeds the marginal cost. What is necessary to get optimality is an agreement between the community and each family to share the cost of providing additional education for each child. Unless the cost of reaching such agreements is prohibitive, this is the typical public good case in which the sharing scheme at optimality is, strictly speaking, indeterminate.

Intuitively one would suppose that, since in independent-adjustment equilibrium (which can always be reattained by the community) payment by the community varies inversely with income, if the same "kinds" of bargains are struck at all income levels, a similar pattern will have to be exhibited at the optimal point.⁶ One possible sharing scheme which does give this result is based on the assumption that the community's share is to be kept as small as possible. This assumption is

⁶ If the parental demand curve for education were very elastic at high-income levels, it is conceivable that, under some sharing schemes, the community might be led to pay subsidies varying directly with income, since a small subsidy from the community could trigger such a large increase in parental outlay that the community would wish to increase its payments. But since all parental demand curves for education are likely to be inelastic, this situation seems implausible.

made for three reasons: (1) Since the community can adjust more freely than can individual families, it can get a bargain more in its favor. (2) The process of providing a service through the fisc is likely to entail high tax collection costs. (3) Most importantly, since lump-sum taxes are not likely to be practical, almost any method of public provision through tax and expenditure involves some excess burden; any feasible tax will cause some distortion. Hence, any reduction in the extent of public provision will reduce this burden [2].

Refer to Figure 2; the ΣD curves are the vertical summations of the

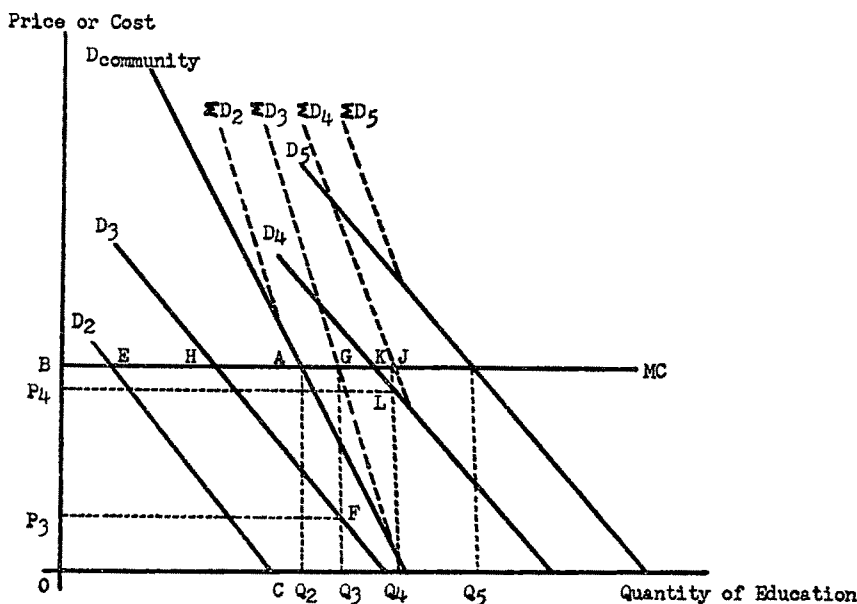


FIGURE 2

community's demand curve with the demand curves for each family. Income effects are ignored here. The optimum quantities of education are OQ_2 for families 1 and 2, OQ_3 for family 3, etc. (Family 1 is so poor it has no perceptible demand for education at any positive price.) Since the community can, in effect, vary the price per unit to the families, it can get them to make payments equal to the area under their demand curve below its intersection with the cost line (e.g., $OCEB$ for family 2). Then the amount the community will have to pay to get the provision of education extended up to the point at which condition (3) is satisfied is the whole rectangle OQ_2AB for family 1, area CQ_2AE for family 2, triangle FGH for family 3, triangle LJK for family 4, and zero for family 5. Note that the size of these areas varies inversely with income.

The amounts that the community should pay in an optimal situation are, of course, the result of a bargaining process, and it is conceivable, given only condition (3), that the community could optimally pay the same amount to all. But this would require a unique sort of bargaining behavior which is unlikely to occur, if the community were free to adjust. Equal community payments to all would involve more favorable bargains for those with high incomes, and less favorable bargains for those with low incomes. It is unlikely that the community would permit such bargains to be struck.

Actually attaining the optimal result may be difficult, since lump-sum payments like those in Figure 2 are only optimal if there is an explicit agreement on how much each family at each income level is to spend on education. But a scheme in which the community agrees to pay some fraction of the cost of *each unit* of education purchased by the parents could lead to optimality in the absence of explicit agreement with each family. The optimal structure of these payments is not, however, one in which the community pays the same fraction of the per unit cost at all income levels, but rather it is one in which the fraction paid by the community varies inversely with income. In Figure 2, the community would pay fraction P_3B of the cost of each unit of education family 3 purchases, and a smaller fraction P_4B of the cost of each unit bought by family 4. It would pay none of the cost for "rich" family 5, and would pay the full cost for families 1 and 2.

A matching scheme such as this, with the community's share of the per unit costs varying inversely with income, can lead to that optimality in the provision of citizenship-oriented education which cannot be obtained simply by allowing parents to supplement equal-value state tuition certificates. If, on the other hand, the assumption that the community desires every child to attain some fixed minimum level of education is accepted, then efficiency requires unequal lump-sum public payments for different families. In either case the optimum community outlay varies inversely with income.

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THE CHARACTERISTICS OF FOREIGNERS IN THE U.S. ECONOMICS PROFESSION

By HERBERT G. GRUBEL AND ANTHONY D. SCOTT*

While the theoretical aspects of the "brain drain," the migration of highly skilled people from the rest of the world to the United States, have been analyzed recently [2] [4] [5], very few reliable statistics on the actual magnitude of the problem are available. The unavailability of the empirical information is unfortunate because as a result we cannot answer the question whether the public concern over the brain drain reflects a new trend or merely a new awareness of a flow having a long history, nor derive insights into the drain from the less developed countries of the world, nor discover anything about the "quality" of the migrants, more specifically whether the "best" in some sense are drawn to the United States.

In this paper we have tried to provide quantitative information relevant to some of these issues by analyzing the characteristics of foreigners in the U.S. economics profession. The study has become feasible through the 1964 National Science Foundation project of registering the U.S. stock of scientific and technical personnel, which recorded the life and professional histories of American natural and social scientists through surveys conducted in cooperation with professional organizations.¹

Our analysis of the economics profession is useful even in the context of the broader question of the "brain drain" involving many other professions because economics is a science in which the transfer of foreign skills is probably about "average." On the one hand, foreign training in economics is not as readily transferable and useful in the United States as is the training of theoretical physicists. On the other hand, the tools of economic analysis are probably more uniform be-

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¹The technical details of how the survey was conducted, the response rate, etc., are set forth in [1], to which the interested reader is referred.

tween nations than is the specific knowledge of lawyers or historians. Also, economics is not a licensed profession, which makes it easier for economists to practice in a foreign country than, for instance, medical doctors and lawyers.²

Before turning to the presentation and analysis of our findings it is necessary to discuss two characteristics of the data. First, as is common experience in survey data, some respondents failed to answer some of the questions. Consequently, the sum of all observations in each of the categories discussed often fails to be as large as the total sample. Under all income classification schemes, for example, somewhere between 5 and 10 per cent of the members in each class failed to report income statistics or had no income at all. We have simply disregarded these observations in our compilations of median incomes and deciles, a procedure biasing the computed averages in an unknown direction. The unknown bias does not, however, impair the usefulness of our statistics, since we are primarily interested in comparisons between U.S.-born and foreign-born economists and there is no reason to believe that the share of nonrespondents or zero-income earners is greater in one group than in the other.

Second, we had separate information on professional salaries and on gross professional income, but decided to use only gross professional income in our analysis because the use of both would have unduly complicated the presentation of the information without adding substantially to the insights attainable by the statistic we used. Gross professional income was considered to be a more relevant measure of professional success than basic salary since the earnings from consulting, royalties, and other honorariums included in the former are an important part of many economists' incomes.

I. Characteristics of Foreign- and U.S.-born Economists

Table 1 was compiled from the total sample and compares the characteristics of the foreign-born and the U.S.-born. As can be seen (from columns 1 and 3 giving the absolute number, column 5 the percentage), nearly 12 per cent of U.S. economists were born abroad. However, this percentage differs for the various age-groups (see column 5). The foreign-born are relatively most numerous among the oldest age-groups, are the smallest proportions among the very young and those who are now between the ages of 50 and 60, while their share in the age-group of 30 to 45 is in between these two extremes.

² A study concerned with professional migration to and from Canada found that according to the 1961 census the foreign-born postwar immigrants represent 25.2 per cent of engineers, 18.5 per cent of economists, 12.6 per cent of health professions and only 3 per cent of the law profession now working in Canada. These data are from [3, Table 21, p. 63].

TABLE 1—CHARACTERISTICS OF U.S.- AND FOREIGN-BORN ECONOMISTS^a

| | Foreign | Per Cent | U.S. Born | Per Cent | Foreign Born as Per Cent of Total |
|-----------------------------------|---------|--------------------|------------------|----------|--|
| | (1) | (2) | (3) | (4) | (5) |
| A. Place and Date of Birth | | | | | |
| 1. Born before 1890 | 19 | 1.4 | 110 | 1.0 | 14.7 |
| 1891 1895 | 21 | 1.6 | 140 | 1.3 | 13.0 |
| 96 1900 | 67 | 5.0 | 342 | 3.2 | 16.4 |
| 1901 05 | 89 | 6.7 | 504 | 4.7 | 15.0 |
| 06 10 | 93 | 7.0 | 794 | 7.4 | 10.5 |
| 11 15 | 131 | 9.9 | 1,224 | 11.3 | 9.7 |
| 16 20 | 147 | 11.1 | 1,596 | 14.8 | 8.9 |
| 21 25 | 254 | 19.1 | 1,834 | 17.1 | 12.2 |
| 26 30 | 231 | 17.4 | 1,780 | 16.6 | 11.5 |
| 31 35 | 191 | 14.4 | 1,520 | 14.1 | 11.1 |
| 36 40 | 83 | 6.2 | 870 | 8.1 | 8.7 |
| 41 45 | 3 | .2 | 41 | .4 | .1 |
| Total | 1,329 | 100.0 ^c | 10,755 | 100.0 | 11.9 |
| B. Degree Information | | | | | |
| 1. High school | | | | | |
| Foreign diploma | 990 | 75.3 | 43 | 1.0 | 95.7 |
| U.S. diploma | 324 | 24.7 | 10,764 | 99.0 | 2.9 |
| Total | 1,314 | 100.0 | 10,807 | 100.0 | |
| 2. Highest degree | | | | | |
| B.A. | 123 | 9.3 | 2,578 | 24.0 | 4.6 |
| M.A. | 384 | 29.0 | 3,727 | 34.8 | 9.3 |
| Ph.D. | 766 | 57.9 | 4,323 | 40.3 | 15.1 |
| Other ^b | 50 | 3.8 | 93 | .9 | 35.0 |
| Total | 1,323 | 100.0 | 10,721 | 100.0 | |
| 3. Source of highest degree | Foreign | | United States | | Foreign as Per Cent of Total |
| Foreign born B.A. | 28 | | 95 | | 22.8 |
| M.A. | 60 | | 324 | | 15.6 |
| Ph.D. | 184 | | 582 | | 24.0 |
| Other | 45 | | 2 | | 95.8 |
| Total | 317 | | 1,003 | | 24.0 |
| 3. Source of highest degree | | | | | |
| U.S. born B.A. | 4 | | 2,572 | | .1 |
| M.A. | 14 | | 3,708 | | .4 |
| Ph.D. | 46 | | 4,277 | | 1.1 |
| Other | 9 | | 80 | | 10.1 |
| Total | 73 | | 10,637 | | .7 |

^a Subtotals differ from grand total due to information missing.^b This category includes economists who hold no college degrees.^c Percentages may not total 100.0 due to rounding.

| | Foreign | Per Cent | U.S. Born | Per Cent | Foreign Born as Per Cent of Total (5) |
|---------------------------|---------|----------|--------------|----------|---|
| | (1) | (2) | (3) | (4) | |
| C. Employment and Income | | | | | |
| 1. Type of employer | | | | | |
| Industry | 258 | 21.0 | 3,556 | 34.9 | 6.8 |
| Academic | 705 | 57.3 | 4,304 | 42.3 | 14.1 |
| Federal government | 94 | 7.6 | 1,180 | 11.6 | 7.4 |
| State government | 21 | 1.7 | 141 | 1.4 | 13.0 |
| Other government | 12 | 1.0 | 73 | .7 | 14.1 |
| Consulting | 9 | .7 | 72 | .7 | 11.1 |
| Self employed | 18 | 1.5 | 174 | 1.7 | 9.5 |
| Nonprofit organization | 54 | 4.4 | 347 | 3.4 | 13.5 |
| Other | 59 | 4.8 | 333 | 3.3 | 15.0 |
| Total | 1,230 | 100.0 | 10,180 | 100.0 | |
| 2. Academic rank | | | | | |
| Dean | 4 | .6 | 18 | .5 | 18.2 |
| Full professor | 192 | 31.1 | 1,319 | 35.1 | 12.7 |
| Associate professor | 155 | 25.1 | 842 | 22.4 | 15.5 |
| Assistant professor | 168 | 27.2 | 907 | 24.1 | 15.6 |
| Instructor | 44 | 7.1 | 319 | 8.5 | 12.1 |
| Lecturer | 23 | 3.7 | 202 | 5.4 | 10.2 |
| Research asst. and assoc. | 11 | 1.8 | 93 | 2.5 | 10.6 |
| Other | 21 | 3.4 | 57 | 1.5 | 26.9 |
| Total | 618 | 100.0 | 3,757 | 100.0 | |

| Foreign | | United States | | Foreign-born Income as Per Cent of U.S.-born Income | |
|--|--------|---------------|-------|---|-----|
| 3. Gross professional income (\$ thousand) | | | | | |
| Deciles: | below | 6.0 | below | 6.9 | 87 |
| | | 7.7 | | 8.7 | 87 |
| | | 9.1 | | 9.8 | 93 |
| | | 10.0 | | 11.2 | 89 |
| | median | 12.5 | | 12.5 | 100 |
| | | 14.1 | | 14.2 | 99 |
| | | 15.9 | | 15.9 | 100 |
| | | 18.5 | | 18.8 | 98 |
| | | 22.5 | | 23.7 | 95 |
| | above | 22.6 | above | 23.8 | 95 |

D. Characteristics of Economists with Second-Highest Degree Foreign

| Classes | Birth | High School | Highest Degree | Number |
|---------|-----------|----------------|-------------------|--------|
| 1 | F | US | US | 3 |
| 2 | F | F | US | 198 |
| 3 | F | F | F | 134 |
| 4 | F | US | F | 2 |
| 5 | US | F | US | 3 |
| 6 | US | US | F | 21 |
| 7 | US | US | US | 39 |
| 8 | All Other | | | 9 |

An interesting feature of these statistics is that the lowest share of foreigners occurs in the age group of people who were born during or shortly after World War I, and who were likely to have been attending institutions of higher learning during the 1930s. During this period, on the one hand, Europe experienced the tyranny of the Hitler regime which encouraged emigration of older professionals, but the United States, on the other hand, was in the middle of the Great Depression which reduced incentives for immigration, perhaps more for students than for established professionals. One interpretation of the data is

TABLE 2—FOREIGN-BORN AS A PROPORTION OF TOTAL POPULATION AND OF ECONOMISTS

| Years of Birth | Foreign-Born as Per Cent of Total Population ^a (1) | Foreign-Born Economists as Per Cent of all Economists ^b (2) | General Population = 100 Index of Economists (3) |
|----------------|--|---|---|
| 1896-1905 | 13.7 | 15.6 | 114 |
| 1906-1915 | 7.2 | 10.0 | 139 |
| 16- 20 | 4.3 | 8.9 | 207 |
| 21- 25 | 4.0 | 12.2 | 305 |
| 26- 30 | 3.6 | 11.5 | 319 |
| 31- 35 | 2.8 | 11.1 | 396 |
| 36- 40 | 1.5 | 8.7 | 580 |
| 41- 50 | 1.5 | .6 | 40 |
| Total | 6.1 | 11.9 | 195 |

^a U.S. Census of 1960, Final Report PC(2)-2D.

^b Table 1.

that for people of university age at the time the latter factor seems to have been predominant over the former. Another explanation is that more foreigners than Americans of the relevant age groups were killed in World War II.

The secular decline in the proportion of foreign-born in the U.S. economics profession evidenced by Table 1 is put into perspective by the data assembled in Table 2. Throughout this period the share of foreign-born in the total population has fallen even more significantly (column 1) than that of foreign-born in the economics profession. Taking the proportion found in the total population as a basis (= 100) we have computed an index measuring the magnitude by which the role of foreign-born in the economics profession exceeds that in the total population. This measure shows an impressive and steady upward trend for the entire period except for the last age-group, which contains an insufficient number of observations for a meaningful interpretation. It suggests that the U.S. immigration has been selective, favoring the highly skilled or at least people capable of attaining high levels of skill.

Part B of Table 1 contains information about the level and source of scholastic degrees earned by U.S. economists. The most interesting feature of these data is that 75 per cent of the foreign-born received their high school education abroad, which means that probably no more than 25 per cent of them immigrated to the United States before the age of 18 or 19.

Section B.2 of Table 1 groups the economists according to the highest professional degree they have earned and indicates that the foreign born represent a greater share of the total the higher the degree. The next section of Table 1 reveals that fully 75 per cent of the foreign-born economists arrived in the United States before completion of their professional training abroad and enrolled in U.S. institutions of higher learning. Incorporating the information contained in Part D of Table 1 the following conclusion emerges about the extent to which the foreign born also received their education abroad: Of the 1330 foreign-born, 990 have foreign high school diplomas, 332 have a foreign second-highest degree, 317 have foreign highest degree.

The information about degrees also reveals that a very small proportion of U.S.-born economists received some part of their training abroad. The foreign highest degree most frequently obtained by U.S.-born economists is the Ph.D., of which there are in our data 46, representing 1 per cent of all Ph.D.'s held by U.S.-born economists. Part D of Table 1 shows the extent to which U.S. economists have obtained foreign degrees other than their highest. Interesting is Class 7 which shows that 39 Americans hold a foreign degree supplementing their otherwise complete U.S. education. Of these 39 about 72 per cent, 28, count the Ph.D. as their highest degree.

Section C.1 of Table 1 reveals that the foreign born are employed in relatively greater numbers by universities, state and local government, and nonprofit organizations and are relatively under-represented in industry, self-employment, consulting and the federal government. This pattern of concentration is not surprising if we remember that academia and sometimes also the civil service have been the traditional roads for advancement of members of minority groups in many societies, since in these fields objectively measurable output and examination performance are most important for success, and language and citizenship barriers are often the least restrictive.

The interpretation of income statistics in part C.3 of Table 1 must be handled with care because the two groups under examination differ with respect to many characteristics such as employment and age, all of which tend to influence professional incomes. We turn to a closer analysis of income data in Section III below.

TABLE 3—COUNTRIES OF BIRTH—U.S. ECONOMISTS

| Country or Continent | Number | Per Cent of Continent | Per Cent of Total Foreign Born | Country or Continent | Number | Per Cent of Continent | Per Cent of Total Foreign Born |
|----------------------|--------|-----------------------|--------------------------------|----------------------|--------|-----------------------|--------------------------------|
| N=1287 | | | | | | | |
| Africa | 31 | 100 | 2 | West Europe | 576 | 100 | 45 |
| U.A.R. | 20 | 65 | 1 | Unclassifiable | 1 | 0 | — |
| N. Africa | 1 | 3 | 1 | Scandinavia | 28 | 5 | 2 |
| East Central | 1 | 3 | — | Great Britain | 97 | 17 | 8 |
| West Central | 2 | 6 | — | Benelux | 35 | 6 | 3 |
| East South | | | | Germany | 210 | 36 | 16 |
| Central | 1 | 3 | — | France | 26 | 5 | 2 |
| Southern | 6 | 19 | — | Austria | 102 | 18 | 8 |
| Asia | 196 | 100 | 15 | Iberian Peninsula | 6 | 1 | — |
| Unclassifiable | 1 | 1 | — | Greece, | | | |
| China | 82 | 42 | 6 | Yugoslavia | 50 | 9 | 4 |
| S.E. Asia | 6 | 3 | — | Italy | 21 | 4 | 2 |
| Indian Subcont. | 44 | 22 | 3 | East Europe | 258 | 100 | 20 |
| Japan | 17 | 9 | 1 | Russia | 66 | 26 | 5 |
| Korea | 23 | 12 | 2 | 3 Baltic | | | |
| Formosa | 4 | 2 | — | Countries | 21 | 8 | 2 |
| Philippines | 12 | 6 | 1 | Poland | 63 | 24 | 5 |
| Indonesia | 7 | 3 | 1 | Czechoslovakia | 40 | 16 | 3 |
| Australia | 19 | 100 | 1 | Rumania | 13 | 5 | 1 |
| Australia | 16 | 84 | 1 | Bulgaria | 5 | 2 | — |
| New Zealand | 3 | 16 | — | Hungary | 50 | 20 | 4 |
| South America | 18 | 100 | 1 | North America | 189 | 100 | 15 |
| Argentina and | | | | Canada | 160 | 85 | 12 |
| Paraguay | 7 | 39 | 1 | Central America | 6 | 3 | — |
| Bolivia | 5 | 28 | — | Mexico | 4 | 2 | — |
| Colombia | 3 | 17 | — | West Indies | 6 | 3 | — |
| Ecuador, Peru | 1 | 6 | — | Bahamas | 1 | 1 | — |
| Guiana | 1 | 6 | — | Cuba, Dominican | | | |
| Venezuela | 1 | 6 | — | Rep. | 12 | 6 | 1 |

Note: "—" means less than .5 per cent.

II. Countries of Foreign Birth and Training

In Table 3 we present an analysis of the shares of foreign born supplied by individual countries and continents. Especially noticeable, though not surprising, are the large numbers of American economists born in Western Europe, and in Germany, Austria, and the United Kingdom especially. There are more German-born economists in the United States than there are foreigners born in any of the continents other than Europe.

However, these absolute figures are not particularly meaningful and

comparisons of the importance of individual countries as well as estimates of losses from the "brain drain" require adjustments of the raw data to some common base. One adjustment factor widely used in studies of this nature, population abroad, does not appear to be useful in our context, since the level of industrialization, historic tradition in economics and other matters are the more important determinants of demand for and supply of economists. The computation of an index of adjustment incorporating these demand and supply factors requires data not now available and could not be handled in the framework of our study.

In Table 4 we have concentrated our analysis on the characteristics of economists who are both foreign born and foreign trained, focusing on holders of the Ph.D. from the major countries of Western Europe and Canada. The greatest proportion of Germans earned the Ph.D. degree in the decade of the 1920s, implying that they left Germany as young professionals during the Hitler years and that they have been expelled rather than "drained." There is a relatively large number of British, Benelux, German, Austrian, and Canadian economists who earned their degrees in the 1950s and now work in the United States. While these data could be interpreted as evidence of a "brain drain," it is quite possible that many of these men are in the United States for a period of postdoctoral training, planning to return home ultimately.

III. *Ph.D.'s in Academic Employment*

Of the 4,865 Ph. D.'s in economics for whom we have information about their place of birth and present employment, 70.5 per cent hold academic positions. We have chosen this group of most highly trained economists, all in the same type of employment, to investigate more closely the relationship between income and the country of professional training.

Such an analysis can serve two purposes. First, it serves to reveal whether the United States is attracting the "best" people, who we assume to be those who receive the highest pay for their professional services. If the United State really attracts the best foreign economists, then their incomes should be above the average of all U.S. economists since, assuming abilities to be distributed equally throughout the world, the foreigners are competitive in terms of ability only with the best of the American group. Two objections can be raised against the validity of this empirical proposition. First, foreigners may often be attracted to the United States by salaries which appear high in comparison with their domestic ones but are actually low given the U.S. scale and their ranking in ability. Second, foreigners may be very able but have training inferior to that of their U.S. colleagues. While both of

TABLE 4—FOREIGN-BORN AND FOREIGN-TRAINED U. S. ECONOMISTS

| Country of Highest Degree: | Highest Degree: Ph.D. | | | | | | | | Highest Degree: Other than Ph.D. | |
|--|-----------------------|---------|---------|---------|-------|---------|--------|-----------|-------------------------------------|------|
| | U. K. | Benelux | Germany | Austria | Italy | Hungary | Canada | All Other | | |
| <i>Number of observations</i> | 18 | 11 | 58 | 37 | 7 | 13 | 7 | 33 | All Foreign Countries | 13.2 |
| Gross professional income (\$ thousands) | | | | | | | | | | |
| Median | 16.5 | 17.5 | 15.0 | 13.5 | 17.7 | 10.8 | 14.5 | 14.0 | | 12.0 |
| Upper Quintile | 25.0 | 23.0 | 20.0 | 18.5 | 37.5 | 17.0 | 20.0 | 22.0 | | 19.5 |
| Type of employer (number) | | | | | | | | | | |
| Private industry | 1 | 3 | 8 | 4 | 1 | 2 | 1 | 4 | | 5.0 |
| University or college | 14 | 5 | 35 | 23 | 3 | 8 | 6 | 20 | | 4.9 |
| Federal government | — | — | 4 | 3 | 2 | 1 | — | 2 | | .7 |
| Nonprofit Organizations | 1 | 1 | 1 | — | — | — | — | 2 | | .7 |
| Other employers | 1 | 1 | 4 | 7 | 1 | 1 | — | 10 | | 1.0 |
| Academic rank | | | | | | | | | | |
| Full professor | 5 | 2 | 21 | 7 | — | 5 | 3 | 7 | | 1.4 |
| Associate professor | 6 | 1 | 5 | 8 | 2 | 2 | — | 3 | | .3 |
| Assistant professor | 2 | 2 | 3 | 2 | — | — | 1 | 2 | | .8 |
| Instructor or lecturer | — | — | 1 | 2 | — | 1 | — | — | | .8 |
| Other | — | — | 1 | — | — | — | — | 1 | | .6 |
| Decade of birth | | | | | | | | | | |
| Before 1890 | — | — | 3 | — | — | — | — | 2 | | — |
| 1891-1900 | 2 | — | 16 | 5 | — | 1 | — | 3 | | 1.2 |
| 1901-1910 | 2 | 3 | 19 | 10 | 4 | 4 | 3 | 7 | | 3.1 |
| 1911-1920 | 7 | 2 | 12 | 11 | 3 | 6 | 2 | 11 | | 2.1 |
| 1921-1930 | 6 | 5 | 7 | 10 | — | 2 | 2 | 8 | | 3.0 |
| 1931-1940 | 1 | 1 | 1 | 1 | — | — | — | 2 | | 4.3 |

these objections may be considered valid for the period immediately following the individual's first arrival, they lose their validity later on, as the forces of the market tend to equalize pay for equal productivity and the foreigners make up for their deficient training through study and experience.

For our empirical test we have separated our data of 3,443 observations into four groups according to countries of birth and Ph.D.: (1) U.S.-born and U.S. Ph.D. (2,870); (2) U.S.-born and foreign Ph.D. (45); (3) foreign-born and U.S. Ph.D. (416); (4) foreign-born and foreign Ph.D. (112), and computed median incomes for each decade of birth for each of the four groups. The resultant earnings-age profiles have been plotted in Figure 1. For the middle decades a rather clear picture emerges: the U.S.-born and U.S. Ph.D. group's earnings are higher than those of U.S.-born and foreign Ph.D. group, which in turn exceed those of the foreign-born and foreign Ph.D. holders. The fourth earnings profile, that of the foreign-born with a U.S. Ph.D., behaves rather differently, exceeding the U.S.-born and U.S. Ph.D. in the oldest age groups but falling behind consistently in the following decades.³

Albert Rees has suggested to us the use of an index which, by giving proper weights to the number of observations in each age-income cohort, allows comparison of the four categories in a more direct fashion than does the graph. The index was computed by expressing the income of each age cohort as a per cent of the U.S. income at the same age, multiplying it by the number of observations in the cohort of the respective group, adding these numbers and dividing by the total number of observations in the respective group.⁴ According to this measure, the incomes of the four categories are as follows:

| | |
|--------------------------------|--------|
| U.S.-born and U.S. Ph.D. | = 100 |
| U.S.-born and foreign Ph.D. | = 95.5 |
| Foreign-born and U.S. Ph.D. | = 94.9 |
| Foreign-born and foreign Ph.D. | = 91.5 |

³ We need to point out that the numbers of observations in the 1885 and 1935 birth-group figures are rather small, suggesting that the results are meaningful only for the middle decades. We did not employ tests of statistical significance here since we are dealing with a full population, not a sample. Median incomes are as we report them, additional observations do not exist. Measurement error is the only source of random variation and we have no way of determining the variance due to this source.

⁴ The formula for the computation is:

$$I = \sum_{i=1}^k \left(\frac{Y_i}{YUS_i} \right) \cdot n_i \cdot 100 / \sum_{i=1}^k n_i$$

where I is the index, YUS_i is the income of U.S. born—U.S. Ph.D. cohort, Y_i is the income in the age cohort of the group for which the index is computed, n_i is the number of observations in the cohort and k is the number of cohorts in the group.

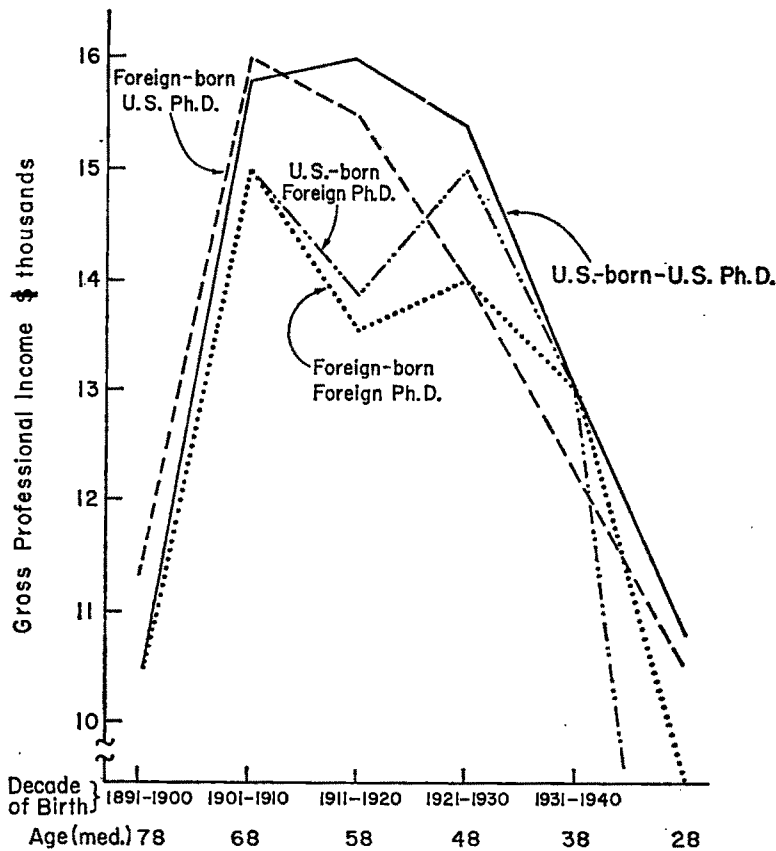


FIGURE 1. ECONOMISTS WITH PH.D. IN ACADEMIC EMPLOYMENT: INCOME-AGE PROFILES OF GROUPS DIFFERENTIATED BY COUNTRIES OF BIRTH AND TRAINING

On the basis of these findings it does not appear that the United States is attracting primarily the "best" economists from foreign countries. Also, the great melting-pot hypothesis according to which a person's place of birth has no influence on his earnings after he has been in the United States for a certain length of time does not seem to be supported by these data.

Several explanations of the income differences can be advanced, none of which, however, were we able to test any further. First, even though there is a world market for the best and the United States pays the highest salaries, other countries have nonpecuniary methods of compensating and holding their best, such as giving them prestige, power, and admiration through asking for counsel in public affairs, making them directors of institutes and bestowing official honors upon them. Second, many of the immigrants to the United States were not

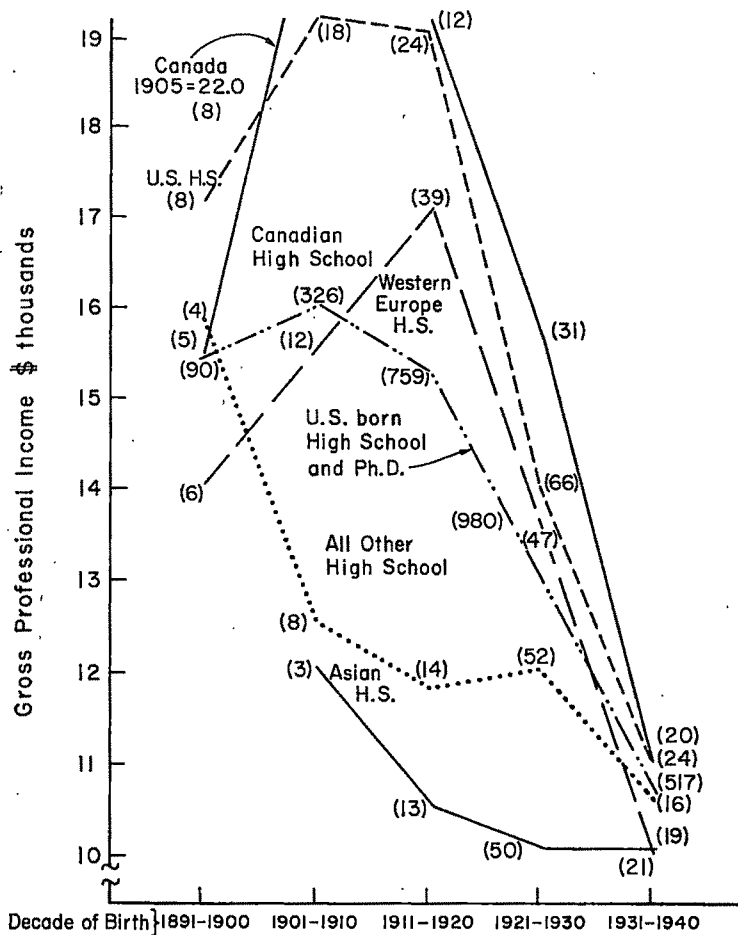
primarily attracted by higher salaries, but fled undesirable social and political conditions in their native countries. These motives tend to work on all people of a professional group, regardless of their abilities. Third, there may be an element of discrimination in the hiring and promoting of foreigners even by employers such as universities and colleges, which are alleged to have very cosmopolitan views on such matters. Fourth, it is possible that economists with a worldwide reputation are primarily theorists, statisticians, and mathematical economists, while those of great capacity in applied work are both less well known abroad and (by reason of their vested interest in knowledge of local institutions and situations) less mobile. If this is true, market forces would tend to draw mainly theorists and mathematicians, etc., to the United States. This situation, in turn, would explain the lower gross incomes of foreign-born economists only if it is true that "applied" economists tend to receive higher gross incomes than academic theorists. Because *gross* incomes include consultation fees, research fees, etc. from government and industry, it is not implausible that the "market" *has* drawn the best foreign economists, but only those with a theoretical specialization.

We produced an income-index like the one just discussed for groups of economists classified by the countries which granted the Ph.D.'s. The results are based on relatively few observations (see line 1 of Table 4), and should be interpreted with caution. However, they do present an interesting ranking of the value of Ph.D.'s granted by some countries.

| <i>Birth</i> | <i>Ph.D.</i> | <i>Index of Income</i> |
|---------------|----------------|------------------------|
| United States | United States | 100 |
| Foreign | United Kingdom | 102.1 |
| United States | United Kingdom | 99.7 |
| Foreign | Canada | 95.8 |
| Foreign | Austria | 93.8 |
| Foreign | Germany | 89.9 |
| Foreign | Hungary | 68.1 |

Second, the analysis of Ph.D.'s in academic employment is relevant to the question whether the United States retains the "best" of the foreign students who have come to this country for training.

This proposition can be tested in the same way as was the previous one: The average incomes of the foreign-born with foreign secondary education but a U.S. Ph.D. should exceed the average incomes of those with a U.S. background throughout. We have grouped the foreign-born into the following four geographical areas of their high school: Canada, Western Europe, Asia, and All Other (in which Eastern Europe



Note: Numbers in parenthesis represent number of observations.

FIGURE 2. ECONOMISTS WITH PH.D. IN ACADEMIC EMPLOYMENT: INCOME-AGE PROFILES OF FOREIGN BORN U.S. PH.D., GROUPED BY COUNTRY OF HIGH SCHOOL, PLUS ALL U.S. BACKGROUND

predominates). For comparison we have also included the group of foreign-born with a U.S. high school education and a U.S. Ph.D. As can be seen from Figure 2, the income-age profiles of the Asian and "All Other" high school groups is considerably below the profile for the U.S. born and trained. On the other hand, people of all ages with a Canadian high school and those foreign born with a U.S. high school have incomes above their American contemporaries, and West European high school graduates have higher incomes than Americans during two of the decades. The numbers of observations in practically all

of the categories are quite large so that the figures shown can be treated with some confidence.

The computed index provides the following summary results:

| <i>Birth</i> | <i>High School</i> | <i>Ph.D.</i> | <i>Index</i> |
|---------------|--------------------|---------------|--------------|
| United States | United States | United States | 100 |
| Foreign | United States | United States | 111.8 |
| Foreign | Western Europe | United States | 102.8 |
| Foreign | Canadian | United States | 117.1 |
| Foreign | All Other | United States | 90.0 |
| Foreign | Asian | United States | 79.3 |

These computations suggest that the "quality" of Ph.D. holders of foreign birth and with a foreign high school degree is above that for U.S.-born and U.S. high school individuals for two geographical areas having a great cultural affinity to the United States and for which discrimination is likely to be smallest. The characteristics of the foreign-born with U.S. high school may be the result of well-known sociological and psychological factors common to minority groups. As for the low incomes of high school graduates from Asia and "All Other" (mostly Eastern Europe), we would suggest that they reflect discrimination rather than lower "quality," though we have no way of proving this proposition.

IV. *Summary and Conclusions*

Our analysis of the American economics profession in 1964 has revealed that the proportion of foreign-born is 12 per cent, which is about twice as high as that for the U.S. population in general. One quarter of the foreign-born arrived in the United States before high school completion, 75 per cent before acquisition of their professional degree.

Analysis by age-groups reveals that the foreign-born are a smaller proportion of the total the younger the group. But the proportion of foreign born in the economics profession is 1.1 times that for the population as a whole for the oldest age-group and is larger the younger the group. Breakdown of the foreign born by countries of origin shows that an overwhelmingly large proportion has come from Western Europe and other developed parts of the world, and only 18 per cent from Asia and Africa. Before being able to draw any conclusions about the "brain drain" problem from less developed countries in general, these data have to be put in relation to supply statistics; moreover, the economics profession may reflect only inadequately the developments in other sciences and professions.

According to the data, foreign-educated Ph.D.'s in academic em-

ployment have below-average incomes. One interpretation of this finding is that the United States does not attract the "best" economists from foreign countries, though discrimination against foreigners can also explain at least part of the income differences. The data reveal that economists with Western European and Canadian high school training who obtain U.S. Ph.D.'s and stay on at U.S. universities earn a higher income on the average than economists who were born and trained in the United States.

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ON THE QUESTION OF OPERATIONALISM

*A Review Article*¹

By BEN B. SELIGMAN*

I

It is now a cliché, and no longer an accurate one at that, to describe Paul A. Samuelson as the *wunderkind* of American economics. It is not accurate because Samuelson is now past the half-century mark; yet he has been producing professional papers and articles as well as popular columns for sundry newspapers here and abroad (not to speak of the frequent revisions of his introductory text) in such profusion that we are apt to think of him as an elder statesman. But when the time sequence is placed into proper focus one must concede that the Samuelson corpus represents an extraordinarily sustained performance. Although Samuelson does not discount "late bloomers" in economics (*vide* his article on Abba Lerner as a case in point [13]), Samuelson himself illustrates the Schumpeterian dictum—a preanalytic vision of the economic process developed early in life and then carried through in an endless relation of give-and-take between empirical and theoretical work. For Samuelson this has meant the creation of a "grand neoclassical synthesis" combining modern national income analysis with the classical verities enunciated by the founding fathers.

This central view pervades the present collection of papers, whose 1,813 pages of text span the years 1937 to 1964. Its two large volumes, containing 129 articles, comments, lectures, and statements, include virtually all of Samuelson's contribution to modern economic theory, except, of course, the *Foundations of Economic Analysis* and *Linear Programming and Economic Analysis* (the latter authored jointly with Robert Dorfman and Robert Solow). One finds here the well-known articles on revealed preference, the transfer problem, factor price equalization, the pure theory of public expenditure, and the essays on Ricardian and Marxian economics. Included also are some fifteen pieces on purely mathematical topics, as well as the famous discussions of the multiplier, acceleration principle, and income determination. Some articles are published for the first time, known up to now to only an inner circle who were privileged to read them in manuscript; others have been resurrected from generally inaccessible books of essays, dedicated to professors on the verge of retirement, and from other obscure places. The latter include items on the constancy of the marginal utility of income; market mechanisms and max-

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¹*The Collected Scientific Papers of Paul A. Samuelson*. Edited by Joseph E. Stiglitz. Cambridge, Mass. and London: The M.I.T. Press, 1966. 2 vols., pp. xi, 771; xi, 1041. \$12.50 each.

imization; the evaluation of real national income; probability and attempts to measure utility; a neoclassical reformulation of modern fiscal policy rules and principles; substitutability in open Leontief models; linear programming and economic theory; recent monetary controversies; intertemporal price equilibrium; the structure of a minimum equilibrium system; the 1961 Stamp Memorial lecture; a memorandum for the Canadian Royal Commission on Banking and Finance; and the 1962 Wicksell lectures. It is all quite a heady feast.

The task of putting the papers together was undertaken by Joseph Stiglitz, a graduate student, who has done a commendable job of organization. Several of the articles have postscripts redefining Samuelson's present position on the question at issue. Others are essentially predecessors of the *Foundations* or *Linear Programming*. (Curiously, two articles from *Econometrica*, published in 1941 and 1942, which were later incorporated in the *Foundations*, are also reprinted. One might ask whether the editor did not really gild the lily at this point!) The arrangement, which is topical rather than chronological, provides for five "books": (1) pure theory: consumer's behavior and capital theory (including the papers on Ricardo and Marx); (2) mathematical economics, linear programming, comparative statics and dynamics, and equilibrium systems; (3) trade, welfare economics, and income determination; (4) the theory of public expenditures, fiscal and monetary policy, and political philosophy; and (5) doctrinal history and methodology. Merely listing the categories is sufficient to underscore the broad scope and depth of the Samuelson *opera*. These in many ways represent the essence of contemporary economics; their significance stems not only from the mirror image they present of what has gone on in economics from 1937 to 1964, but also from the enormous impact that Samuelson has had on the way in which economists now think.

II

It would be only fair to indicate my own biases, which are those of an institutionalist who, contrary to popular opinion on institutionalists, happens to have a rather high regard for theory (after Keynes, who would dare denigrate theory?). But as I have indicated elsewhere, model building as a mode of theorizing needs to provide insights into economics as a *social* science, thereby providing materials for a more general theory of human behavior. I assume that goods and services are not only commodities and performances, but are significantly related to the human actors who control them and that the relationships between such actors are also economic. Yet such relationships may be infused and even dominated by other, noneconomic considerations, so that rationality and self-interest are not always the prime movers in human behavior. In fact, the noneconomic may provide parameters for the economic; insofar as this may be the case, many of the models of the economist need rather broad amplification.

I further feel strongly that a theory needs to be viewed not only in its own terms, but functionally as well, that it is the responsibility of a commentator to uncover the meaning of a theory for the particular time to which it relates. This is perhaps the most difficult task of all, and particularly so for an approach that has been developed during one's own lifetime. Nevertheless, such

criteria, I think, must be applied to Samuelson's total contribution, which is doubtless an important milestone in the intellectual history of economics. Lasting judgments at such close range may be impossible to make, but since I feel that his work will be pondered by students of economics long after most of us are gone, it seems useful to try. Let us then start at the beginning.

The notion of revealed preference was an early Samuelson contribution that is finally being enshrined in the textbooks. (When this occurs danger signals ought to be raised, for often the original idea is then emptied of all subtlety, a fate suffered by many a major economist.) An evaluation of revealed preference at this juncture, however, requires an estimation of Samuelson's methodological approach, a task we shall attempt below. Obviously, any theory of consumer behavior is intended to provide foundations for the law of demand, and in addition, the more sophisticated versions, such as Samuelson's, seek to establish underpinnings for the logic of index numbers. In older formulations the specified utility function, with price given, defined utility as a function of the quantities purchased, so that a continuous, smooth curve was derived. From this one could construct indifference curves and explicate the nature of substitution and income effects. All this is now well known from the work of such writers as Slutsky and Hicks [8] [9] [14]. Stated as simply as possible, Slutsky, for example, held that the lower the price of a good relative to another, the more attractive it would be to a consumer. The ensuing substitution effect would lead to a negative slope in the demand curve of the affected good. But with a lower price, purchasing power was enhanced, leading to an income effect which could cause a shift in the choice of commodities. That is, substitution and income effects were interrelated.

The roots of this rather refined approach may be traced to the 19th century marginalists, Jules Dupuit and Heinrich Gossen [16]. Further contributions were made by Edgeworth while others drew refinements from the work of D. Bernoulli as well as the psychologists Weber and Fechner who thought that a unit of sensation (utility?) was proportional to the original stimulus. So far as the economists were concerned, all this was an attempt to define the shape of the consumer's utility curve. By Slutsky's time, at about World War I, it was evident that Occam's Razor was to be ruthlessly applied: a theory of consumer's behavior was to be evolved that would be free and independent of psychological lumber. The thrust was toward a general theory, but one that would presumably accord with reality.

Yet the ensuing explanation of consumer behavior remained troublesome. While generality might be supplied by the notions of continuity and smoothness (i.e., infinitely large numbers of combinations of goods were posited along the utility curve with continuous first and second order partial derivatives) the argument nevertheless failed to establish a congruence with reality. It was at this point that Samuelson's revealed preference hypothesis seemed to come to the rescue, for if the observed behavior of the consumer fulfilled the basic Samuelson premises then it would still be possible to derive indifference curves and a demand function. The postulates were that a consumer would buy more of a good if such an action would not affect the amount of other goods he could obtain, and that his choices were consistent as well as transitive. In es-

sence, Samuelson introduced the notion of finite differences so that the observing economist was not required to work with instantaneous rates of change.

The theory seemed persuasive enough, except that it assumed a constancy of buying experience that did not appear to accord with the notion of "congruence with reality." Moreover, if the "observed" consumer were an ideal type of representative man, then the extremes of behavior to be found in such an implicit distribution of actors might very well by their eccentricities upset the search for generality. Consequently, the rationality suggested by revealed preference can be quite restricted. And since the information the economist seeks can be gleaned only after preference is revealed, it could be difficult to locate a counterpart of the theory's logic in the hurly-burly of an actual market place. True enough, many economists accept Hicks' dictum that there is no need to consider whether or not the real consumer acts like the representative man. But if such considerations are indeed to be set to one side, the economist runs the risk of constructing "black boxes" that in the final analysis are otiose. For if demand theory is really to describe consumer behavior, then the request for congruence to reality is a valid one.

From another standpoint, it would seem reasonable that the observing economist would welcome a way of independently determining the order of preferences in advance of the choices that a consumer makes. Surely this would provide a framework for empirically establishing consistency [3]. Revealed preference, with a logic internal to itself, cannot supply such information, and, as a consequence, it becomes virtually impossible to obtain independent verification of the rules of consumer behavior. For in fact consumers may really believe that price alone makes a good desirable and make their choices accordingly, or the range of choices may be actually intransitive so that the ensuing inconsistencies may indeed obliterate the postulates of revealed preference. Economists may consider such behavior pathological, but then so much of human behavior is precisely pathological.

Furthermore, since revealed preference deals essentially with points, repeated observations over time are required in order to arrive at sensible statements regarding consumer behavior. This presumes that experience has no impact on behavior, a concept rather difficult to digest. Yet given enough observations of what the consumer does, it is argued, we can derive his indifference map: the question that arises is—do we not come right back to the continuity and smoothness assumptions? There are problems then in utility theory that remain unresolved: one is uncertain that a concern for the minutiae of consumer behavior—under what conditions is tea a substitute for coffee *vis-à-vis* a third good—really adds much to our understanding of what in fact does occur in the market. Perhaps more important, one is not really convinced that the sophisticated mathematics of modern theory really cuts away from psychological roots, for the underlying details of consumer behavior are psychological and sociological as well as economic.

Samuelson does acknowledge some of these difficulties, especially in his 1950 article on integrability (p. 75 ff.): here he speaks of irreversible lag effects, inconsistent preferences for small changes, and the influence of collective or family preferences as important sources of divergent behavior. The

theorist, it would seem, is confronted by a dilemma. Once such difficulties are recognized, the empirical verification that would validate a *behavioral* theory becomes moot. Behavior patterns that might supply evidence to confirm or disconfirm a theory must necessarily stem from a kind of metaeconomics. (The term is used here in a somewhat different sense than is found in Samuelson's Part VII where, for example, the famous "correspondence principle" is described as metaeconomic. Why the mutual relationships of static and dynamic systems form a transcending principle of analysis is not clear.) The appeal to metaeconomics supplies what G.P.E. Clarkson has called interpretive rules that establish consistency between consumer action and theoretical models [3, p. 96 ff.].

Such rules—transcending principles or "going beyond"—it would seem, offer the most promising techniques for evolving criteria of verification. An investigation of the *decision* to buy may throw more light on preferences than the mere observation of the *act* of buying, which is where revealed preference begins. Consequently, the latter's behavioral terms may or may not be indicated. Have not all the external conditioning elements in choice-making been brushed aside in revealed preference? Does it really tell us how choice is made? Such questions seemingly raise issues of no small interest. For not all goods are subjected to identical evaluation procedures by the consumer, and his expectations regarding price movements do exert some influence upon his behavior. As Jan Tinbergen once remarked in quite another context, many more parameters of conduct are necessary, "most of which have not yet been measured" [2, p. 9n]. Revealed preference appears to suffer, as do most other variants of the theory of consumer behavior, from such limitations. This entire branch of economics may be questioned on the ground that it avoids what Galbraith has called the diminishing urgency of consumption in an affluent society [4, p. 150]. The reduction of consumer desires to terms of preference excludes the possibility that present needs might be fully satisfied in the calculable future.

On the other hand, I do not imply by any means that practical statistical research is in any better condition. Interviewing techniques to probe the mind of the consumer are filled with snares and traps galore, while time series and cross sectional studies may either overlook significant variables or deal with so many elements that the whole apparatus collapses. Yet we have gleaned enough knowledge of consumer behavior from the work of Kuznets, Brady, Duesenberry, and others that it may be more fruitful to pursue their line of investigation. Schumpeter once remarked that a theory ought to be a program for research. Insofar as consumer behavior is concerned, it seems dubious that theories such as revealed preference will perform that function. Perhaps economic analysis, as Samuelson said in his 1961 A.E.A. presidential address (p. 1499 ff.), does not really have to trouble itself with the state of the real world. This may explain why so many of the models that theorists construct are virtuous for their own sake; yet their often unmanageable mathematical framework must be subjected to a metaeconomics if we are to have a useful social science.

III

Why has economics taken this turn? Here one must examine some methodological questions, an area to which Samuelson has made important contributions. A fundamental issue involves the uses of mathematics in economics. In a paper given at the 1951 meetings of the American Economic Association (p. 1751), Samuelson argued that no distinction should be drawn between mathematical and nonmathematical modes of discourse. While the latter employs literary forms, said Samuelson, it may be argued that mathematics is also a language. In fact, he insisted, mathematics is language.

Now, that would seem to be fudging the methodological issue with a bit of poetic license. Mathematics is mathematics, with its own operational rules and logic. It may have had its roots in language and practical usage, but as it flowered, mathematics necessarily had to transcend the limits of direct experience. That is as it should have been, otherwise we would not now possess so powerful a tool. We may teach mathematics by using language, but what other pedagogic device do we have? Those who think that mathematics can be taught with symbols alone, or those who think that economic discourse should be restricted to symbols, might try learning Swahili by the crash Berlitz method.

If economists assume the task of describing, summarizing, and explaining empirical reality, as Samuelson says they must, then the transformation of mathematics in economics from a tool into a fetish will not help. Mathematics may be faster and more convenient in handling deductive inferences than ordinary logic in literary form (p. 1759), but not all practitioners are as good as Samuelson. He may very well have to paraphrase Marx—"Je ne suis économiste mathématique!" Indeed, mathematics may have swept the field like an epidemic of measles, as Samuelson says elsewhere, but it can become quite a serious ailment: all too often the very qualitative elements that underpin differences in variables are ignored [5, p. 119]. Having made so strong a case for the mathematical mode, he must take some responsibility for all those lesser lights who now believe that a symbol is the best entrée into the inner circle of expertise. An illustration of the latter situation is a model offered some years ago by a young economist in which national product was described as a function of capital stock services, the rate of use of natural resources, employment, society's fund of knowledge, and the cultural milieu. Stated in literary form, the expressions made sense, but translated into symbols, the cultural milieu became U , presumably capable of being subjected to all the mathematical operations of the calculus. Surely this is carrying an analytical simile to ludicrous extremes. And it goes on all the time.

One supposes that an ability to manipulate symbols expertly gives an economist a feeling of psychological advantage over his confrères who are less expert (on occasion Samuelson is not above such ploys). But that does not mean that they can predict better than anyone else the direction in which the economy will move. One can quote no better authority on this point than Samuelson, who recently noted that as far back as 1961 some of our best model builders had proved beyond a scintilla of doubt that the international pay-

ments problem would be solved. Yet the high aggregate demand in 1966 did spill over into international markets, upsetting the expectations for a sufficiently large current-account surplus that would keep us in overall balance.

True enough, mathematical economists have suddenly discovered a welcome mat in the middle and upper echelons of corporations and in the corridors of Washington bureaus, but they still have to speak English. A case in point is Samuelson's task force paper on economic policy prepared for the incoming Kennedy Administration in 1960 (p. 1478). It is an excellent document, well-reasoned and carefully delineating the goals, economic targets, and boundaries within which the new President would have to function. It didn't take much knowledge of mathematics to write that paper: its modern Neo-Keynesian policy formulations are clearly stated in pellucid English. Perhaps this merely illustrates the high skill and adroitness that Samuelson displays in literary as well as mathematical forms of discourse.

In any case, the mathematically oriented have their present triumph, and future historians may accord to Samuelson a fair share of the credit. But the victory is not entirely costless, since the current fashion stems in many ways from the apparent ease that it lends toward achieving full-fledged professional status. Few students are willing to undertake the task of dealing directly with empirical materials: nor do many arise before dawn to read Locke and Hume on futurity as, say, did John R. Commons. Having acquired a facility with algebra and calculus, it seems easier to construct a model and enter upon an academic career. Unfortunately, the issue of relevance is oftentimes cast aside. Let us be clear about the matter: mathematics can be a good tool in economics and a useful one in checking theoretical hunches. It gets closest to economics when it can deal with data, as say in econometrics, operations research, or input-output analysis. And there are areas—labor economics for one (which does deal with the behavior of the largest part of the population)—where mathematically based theory has but limited usefulness, despite recent efforts in the literature to expand its application. For man is what he is—changeable, irrational, and unpredictable.

IV

From a philosophic standpoint, one suspects that Samuelson would justify his reliance on the mathematical method by an appeal to that variant of positivism he calls operationalism. The latter suggests that every science must issue in a deductive system in which all observed events stem from structural laws involving relatively simple relations. Unfortunately, there is little outside the *Foundations of Economic Analysis* that one can draw upon to summarize Samuelson's philosophic views. In that *magnum opus* he contended that economics must be concerned only with operationally meaningful propositions, i.e., with statements capable of being refuted. This suggests that a concept must be defined in terms of its implicit operations, that a meaningful statement is one in which the operators can supply an answer. Yet that is not the sole method for conducting investigations in economics, or in other social sciences for that matter. There may be intuitive extensions into untrodden ter-

ritory that do not immediately yield operational propositions [11]. Indeed, it would appear that a good deal of scientific work began this way. Moreover, a purely operational approach unsupplemented by transcending data can stir up enormous difficulties. To employ a sociological illustration, the operationalist may believe that the causal relationship between poor housing and delinquency necessitates an improvement in housing as *the* way of reducing delinquency. The sad fact is that it does nothing of the kind.

More specifically, the reservations expressed here on operationalism are supported by D. E. Gordon's criticism in his review of Samuelson's *Foundations*. (*Quarterly Journal of Economics*, May 1955.) Samuelson's response is not fully convincing (p. 1767). Gordon had tellingly argued that Samuelson failed to supplement his principles of maximization and dynamic stability with a demonstration that the specified function would not shift unpredictably; that such shifts in the functions were left unexplained; that observed values might be characteristic of a stable function yet not exhibit a stable equilibrium; and that static conclusions do not always require the correspondence principle, that is, may not necessarily be rooted in dynamic conditions. Similarly, Nicholas Georgescu-Roegen [5, p. 59] speaks of qualitative residuals in a preference map stemming from the contrast of alternative choices of two goods and some admixture of such goods. In fact, the latter might be utterly distasteful thereby upsetting the presumption of proportionality involved in preference theory. But how the latter would be disturbed and the direction of its movement can be established only by a factual investigation, by requiring an appeal to a system of metaeconomics. Or what account would an operationalist take of the impact of new commodities on consumer behavior? Again Georgescu-Roegen says, a change in the character of goods does imply a rearrangement of both production patterns and consumer behavior, a factor not easily incorporated in the operational models thus far offered [5, p. 66].

These difficulties issue from the belief that economics has no need to consider questions that might involve value systems or qualitative elements. Many economists believe, neoclassical synthesizers included, that it is entirely possible to create a "science" that abstracts from all human goals. They believe that the semantic significance of knowledge necessarily consigns issues of values or statements about qualitative reactions to the realm of the senseless. A neutral perception of experience is offered, thereby denying that economics might be concerned with matters of values. In effect, there has developed a refusal to concern oneself with any implications other than those specified by a highly restricted model, with the consequence that economists become involved with technique for its own sake. The habit becomes too deeply ingrained to permit any consideration of the metaphysics of purpose.

The central fact in the contemporary world is an evolving, changing industrialism and within that state of immanence the major questions are those that explore man's role in it. We cannot take as given the institutional framework, since it is precisely that which undergoes change and which needs to be examined. Few theoretical economists have been concerned with the process of *becoming* industrialized or with its consequences: such issues are usually set aside by the rule of *ceteris paribus* under which all facts not immediately rel-

evant to theory are either set aside or dismissed. Granted that a theoretical model seeks to introduce order into the bewildering variety of variables and parameters and granted that an effort is sometimes made to relax restrictive assumptions; nevertheless, there remain numerous variables that never enter the particular model's equations. How often is it that a theoretical truth requires modification by unconsidered facts, facts which at first glance may not suit the theorist's pattern? At this point, the economist ought to display the angle of vision which permits him to look at society in the round. He needs to be that economist *cum* sociologist who disproves Schumpeter's *bon mot* that sociology is the proper occupation of a tired economist.

The operationalist approach suggests an imitation of the physical sciences and presumes therefore an ability to produce positive propositions. But why should we always imitate the physical sciences? They function, it would seem, on a different level of discourse and we economists ought not feel so guilt-ridden because we then cannot secure the same payoff. Naturally, economists borrow some of the methodology of the physical sciences and their standards of investigation. One does not depreciate such transactions, of course, but let us not suppose that we can duplicate the precision of their results. Society is a highly complex affair, and its components are not so closely and delicately knit that the slightest disturbance will immediately send convolutions gyrating in explosive fashion throughout the whole structure. A good many of the seams that hold us together are rather loosely sewn: there is much give and play and a change in one sector does not always transmit shock waves to the others. Institutional growth and selection are also loose: perhaps this explains the survival of a good many atavistic traits. The attempted application of the methods of physics to society is a facile escape from the complexities of our social condition. While we need to know what men do, we also need to know the aetiology of their actions, their motivations, their history. Albert Einstein was once asked, "If we have discovered the structure of the atom, why haven't we found a political means to keep the atom from destroying us?" He replied with characteristic wisdom, "It's quite simple. Politics is more difficult than physics." Not all the attributes of the universe can be reduced to space, motion, and mathematics.

It can be argued, therefore, that economic behavior is not quite analogous to physical behavior; to compare one with the other is to commit a serious logical error, for consciousness and the explication of meaning appear in the acts of humans that are not discernible in the movements of models. For the economist this would presuppose a wish to explore *interpretations* of economic actions in a manner that would expose root causes as well as shifts in interrelations. Generally described as the phenomenological approach, this method appears as one well worth the attention of economists.

As a philosophy, phenomenology is by no means an easy one to digest; moreover, the term itself is subject to numerous interpretations. Literally, a theory of phenomena, it may refer to an experienced object which represents some deeper reality that confronts us. Without exploring the roots of the concept in Kant, Hegel, Heidegger or Sartre at this time, we may note that phenomenological philosophers today generally address themselves to "subjec-

tive" descriptions in which human considerations are primary. Actions can be understood by applying a generalized principle of connectibility; it then becomes quite possible to derive general statements about economic or social behavior. Hence, objective propositions are entirely feasible, albeit intimately related to the subjectivity from which they stem. Objects, in other words, may be characterized by experience, and become actuality through phenomena generated by the subjective evaluation of facts and material goods. By no means does this reduce everything to mere experience, for theory can still display an independent persuasiveness. In economics, we do not deal directly with the relationships that contribute to the social fabric, but rather with "ideal" formulations—price levels, consumption, even index numbers—which nevertheless do not seriously violate facts.

Here then it may be possible to achieve what Niels Bohr had called complementarity [1], perhaps the most useful antidote to the self-assurance of operationalism. Bohr had observed that when light interacted with material substances two contrasting results could be observed: on the one hand, there was refraction or similar wave-like behavior; on the other hand, light appeared to travel in packets of energy. It was possible to work out a single set of equations based on the idea of complementarity, which could account for both effects. Reality, therefore, could not be fully described solely in terms of one effect or the other: in fact, conceptions that seemed to be antagonistic were reconcilable at a higher level of analysis as aspects of a single reality. Hence, operationalism is to be replaced, or at least modified, first, by probabilistic considerations, and second, by what has been called the ambiguity of complementarity. That is to say, theoretical science is not the only form of scientific knowledge, as, indeed, Georgescu-Roegen suggests, and economics cannot be limited solely to the realm of the measurable.

Essentially, economic phenomena may not be explained always by pure cause and effect, nor are they always linear; they are rather transactions, a phenomenon well understood by John R. Commons. Moreover, in these transactions there can be no certainty, a principle long established in physics, the science economists wish most to imitate. And, if Bohr is correct, then it may be possible to accept seemingly contradictory explanations of economic phenomena on the ground that the analysis of fact must be supplemented by understanding and comprehension. Those who seek to employ complementarity as a method in economics may wish to use behaviorist description not only to derive operational propositions but also as data to help mark off the margins of discourse. But it is important to note that an operational concept or definition does not insure that absolute clarity has been achieved. Georgescu-Roegen recently demonstrated that there are concepts that violate the principle of contradiction, that there are cases in which "B is both A and Non-A," that indifference may not necessarily occupy the ground between preference and nonpreference [5, p. 22 ff.]. It is in such situations that the idea of complementarity may be most helpful.

Some economists have, perhaps unconsciously, attempted to achieve a measure of complementarity. While it may be too early to judge their success, we may observe that they have sought to weave a web of operational analysis

into the tradition of amelioration established by Mill, Marshall, and Keynes. Samuelson may be classified as a leading practitioner of this difficult approach for, while he has insisted on verifiable theorems, he has conceded that the economist ought to be ready to offer policy proposals, which though reflecting value judgments, do not wander too far from accepted humanitarian standards. Unfortunately, some of the theoretical concepts have skirted close to the edge of emptiness; for example, when consumers' behavior is explained by preferences with the latter defined by observed behavior, the analysis threatens to become circular. The way out seems to be the assumption of a definite commitment by the consumer before a description of his state of mind is possible. This ordering then allows the observing economist to derive welfare propositions from single acts of choice. Yet even as Samuelson has observed, not much of great significance is forthcoming from all this legerdemain. The conclusion that more production of a good might be desirable, or that the same output could be obtained with less input, is admittedly not very striking.

Complementarity also reveals that economics is but one way of examining the totality of human behavior. Traditionally, economics has explored the areas of production and distribution. But goods and services are not merely commodities and performances; they are in a real sense the expressions of human actors who control them and who in turn are controlled by them. Moreover, the relationships which the actors establish between themselves are also economic. And it is the culture arising from these relationships, acting in reciprocal fashion, that determines the movement of society and the fate of man. How the dangers and rewards of existence are to be shared is established in the last analysis by these cultural forms. The material fact of scarcity may be present at the start, but human action makes the material fact a social pattern.

We suggest then that the immanent meaning of economic theory may be grasped by its relations to *praxis*. In this context, verbal meanings and practical goals are often antecedent to logical validity and analytical elegance. Further, if economics is a science of fact rooted in the human condition, it would be necessary to check theory against those events established at the level of connectibility. No simple correspondence should be expected, however, for economic facts are all too often squeezed through the grinder of political policy and social turmoil. But all too often the problem of relevance is ignored, for theory itself is a way of perceiving reality. Unless this perception is achieved, theory cannot perform its primary function—that of guiding empirical inquiry.

V

It is fascinating to watch Samuelson employ his technique, when the occasion demands it, to transcend the limits of his own preconceptions. Here one examines his brilliant dissection of the Friedman formula for model building (p. 1772). Samuelson once wrote to the present writer that it takes an analyst to answer an analyst, and though I am not sure that I would agree, he may understandably offer this piece as the prime exhibit in support of his contention. Professor Friedman, it will be recalled, argued in rather extreme fashion

that direct verification of an hypothesis is not an essential ingredient of a model, for if a prediction is not contradicted by events, then the initial set of assumptions is fully acceptable even though there may be no "congruence with reality." Samuelson's rebuttal is extraordinarily powerful, yet essentially a simple one, employing set theory to demonstrate that consequences are contained in antecedent theorizing, which in turn is implied by certain premises, or as Samuelson would say, minimal premises. By violating this sequence, the Friedman approach improperly admits invalid propositions into analytical discourse.

It is a pity the editor of these papers was unable to include the subsequent debate on this issue [10]. Some sharp words were addressed by Samuelson to his correspondents. (In the author's preface to the *Collected Scientific Papers* Samuelson hopes that he has mellowed over the years. He can be assured that such is not the case. For example, he suggests (p. 1762) that those who are uncomfortable with his sort of theory might prefer doctrinal history or labor economics. Since these happen to be my major areas of interest, I could not help but raise my eyebrows at such a quip; did its maker wish to place boundaries around his specialty, keeping out everyone else as a mere poacher?) In any case, the discussion in the *American Economic Review* drifted from the problem of unrealism in theory to the nature of theory itself, with Samuelson emphasizing a search for empirical regularities as a basis for theoretical foundations in economics. So far as this goes, I must opt for the Samuelson position. One cannot quarrel with his statement that the description of empirical regularities provides a basis for prediction or that explanation is a form of description. Yet in a social science such as economics we may still need a measure of looseness not envisaged in strictly operationalist formulations. Physicists have worked for a long time with concepts that were imprecise by present standards, and it is only recently that a "gene" has been "exactly" defined. There are areas of economic and social science that can be usefully explored by methods and concepts that do not completely partake of the Samuelson purview. To be sure, one must seek to improve and advance conceptual foundations; all one asks is a measure of tolerance from the operationalist, who does not yet, I think, have the last word.

Samuelson's versatility is illustrated by his work in some of the newer, more esoteric areas of economic research—game theory, input-output analysis, and linear programming. His contributions to linear programming are fundamental and now form part of the basic literature. On the other hand, he evidently has been less happy with game theory, founding his reservations on a reluctance to accept the notion that utilities are additive. A cardinal concept such as is implied in game theory is "usable," he has contended, only if there is an external measure proportional to the utilities of the players. The latter assumes that utilities can be specified on some scale of intervals, that is, to make them invariant via linear transformations. Samuelson's hesitation is expressed in the paper, "Probability and the Attempts to Measure Utility" (p. 117) which has an interesting postscript conceding the possibility of employing the utility concept in this manner, followed by a paper on the independence axiom which would allow for additive properties in certain cases (p. 137).

Nevertheless, linear programming appears to be the area that has captured Samuelson's attention, mainly, it would seem, because it does permit a numerical specification of the neoclassical synthesis that he has been advocating over the years. This in no way, of course, minimizes his frequent explorations of the interrelationships that game theory and input-output analysis patently exhibit with linear programming. But it is the latter that has been emphasized, most notably in "Market Mechanisms and Maximization" (p. 425), a paper originally prepared for the RAND Corporation and in "Linear Programming and Economic Theory" (p. 493).

One of the more significant propositions stemming from this work has been labelled the "Turnpike Theorem." It is one of those theoretical formulations that arouse marked interest because it has imbedded in it direct policy implications. A number of writers have offered subtle variants—R. Radner, L. W. McKenzie, M. Morishima [6, p. 104 ff.]. Samuelson's contribution is to be found in the paper on market mechanisms, as well as in one on capital accumulation (p. 287) and one on balanced growth written jointly with R. M. Solow (p. 248). These articles are to be supplemented by the discussion of *Linear Programming and Economic Analysis* in Chapter 12. In these papers—particularly the one co-authored by Solow—one finds mathematical demonstrations of the existence and uniqueness of a balanced growth path, the elements of proportionality of the factors involved, the stability of these proportions, and the suggestion that consumption levels are maintainable under the conditions of "turnpike" growth.

It will be recalled that the turnpike theorem, which theoretically specifies the fastest, most efficient path, has its roots in von Neumann's general equilibrium model of economic growth. The latter described an economy in which the production function exhibited fixed, linear homogeneous properties, where all outputs were inputs, where demand functions of the ordinary kind were excluded, and where all input factors were in limitless supply. The objective was to reveal a path describing a constant equilibrium rate of growth under conditions of perfect competition and zero profits. The equilibrium growth path then required money outlays on inputs to be greater than or at least equal to the value of outputs and the current inputs of any item to be less than or equal to the output of that item in the preceding period. As a consequence, only one such equilibrium rate could satisfy the uniqueness and existence conditions and there was a unique value for the rate of interest and the equilibrium rate of growth. Outputs in excess of those flowing from the equilibrium growth rate were deemed to be free goods. And competition insured that there could be only one such sustainable growth rate.

Consumption, in this original formulation, was not part of the model, so that the von Neumann case appeared as a special one within a more general framework. Essentially, as John R. Hicks has shown, the turnpike theorem is a statement about economic planning [7, p. 201 ff.]. It asks the significant question: what will production plans be, given the initial capital and the future flow of capital, that will enable an economy to reach some future target? Yet, various turnpike models, including Samuelson's, seem to display certain common features that do raise troublesome questions. For one thing, they ab-

stract from the organizational structure necessary to achieve the stated objective (theorists may take this as a "given," but I doubt that this is enough). Secondly, there seems to be no time horizon to which they are addressed, in almost all cases appealing to some vague precept "given enough time." Further, the assumption that surplus goods, i.e., goods in excess of that required for the equilibrium growth path, are noneconomic is much too facile; discarding such goods may place the growth path on a line that varies substantially from the optimum [7, p. 228 ff.].

The most serious problem, however, is the model's concentration on capital endowments. To be sure, Samuelson has been more aware of this problem than other writers, yet in most instances, the theorem in its various guises has explored the techniques that would maximize "final" capital. Hicks has described planning based on such motivations as stemming from a "war mentality." His characterization may be a harsh one, but the planning philosophy stemming from "turnpike" theorizing does suggest a "crash" technique; adopted by underdeveloped nations, it has had some unfortunate consequences. The paradigm, of course, is the debate over the pace of development that took place in the Soviet Union in the early 1920s. On the one hand, there were advocates of a kind of paced development to account for the output and demand patterns in agriculture and consumption; on the other, there were the devotees of a massive investment program that would achieve a rapid expansion of "heavy" capital goods [15]. The extraordinary burdens that the latter strategy imposed are well known. Reverting to the "turnpike" concept itself, it would appear that Hicks' evaluation is much to the point: the "turnpike" is simply one of several optimum paths and one ought to search for a model that allows for per capita consumption growth.

Samuelson, to no one's surprise, has already done so, though it appeared too late for inclusion in the present volumes [12]. His new theorem suggests that, *given enough time*, a balanced turnpike growth path would provide maximum per capita consumption. That indeed is the rub—what is "enough time"? Given "enough time" even the Soviets, with their unbalanced growth pattern, may evolve a high-consumption economy. Would 50 years be enough time? How, in the meantime, would the projected growth path be influenced, say, by technical change? And what influences would uncertainties generated by time lags or wage rate movements exert on the optimum growth path? These would appear to be but a few of the practical issues from which the theory abstracts.

VI

If I have given the impression that the first two "books," as well as parts of the third, are rather rough going, that indeed was my intention. At times the high powered symbols are handled after the fashion of those British mathematicians who blithely say: "It can be shown that . . ." Yet no doubt the feast is rich enough to whet the intellectual appetites of economists for years to come. The last two books—on public policy and doctrinal history—are lighter fare, though no less challenging. The reader will find here perceptive comments on fiscal and monetary policy, several excellent essays on famous economists

(Keynes, Schumpeter, Hotelling, Robertson), and the insightful Wicksell lecture on the state of the American economy. Yet some are not so good: if Beethoven had his *Battle Symphony*, Samuelson may be forgiven his Stamp Memorial Lecture. Aside from a memorable last sentence ("... what do they know of economics, who political economy do not know?"), the lecture rambles painfully and succeeds only in specifying a number of economic issues then current without saying much about them.

On the other hand, if I were to choose the best of the group, the accolade would go to the statement prepared for the Canadian Royal Commission on Banking and Finance in 1962 (p. 1361 ff.). While agreeing that money and credit policies have enormous influence on the course of an economy, Samuelson makes it clear that monetary policy is not the sole mechanism for influencing aggregate behavior. He rejects the notion that adjustments in the supply of money—say 3 per cent a year—would be sufficient to provide the automaticity that some economists believe to be desirable, for "... choices have to be made pragmatically in terms of the goodness or badness of behavior patterns that result from various kinds of discretionary action" (p. 1362). The implication that human action is subject to a decision-making process that is less than completely automatic is well taken. Hence, the attempt to prove a controlling causal proportionality between money supply and aggregate income cannot be convincing. The technical issues involved are so clearly stated that it seems difficult after Samuelson's discussion to defend the quantity theory of money in any of its versions. Says Samuelson tartly, "Optimal stabilization policy must vary with the probability pattern of the system to be stabilized, which makes it rather ridiculous to specify in advance for all times that some particular gadget like 3 per cent money-increase-per-year should be adhered to, in season and out of season" (p. 1376). The clarity of his distinctions between monetary and fiscal policy makes this essay one of the best didactic pieces in the collection.

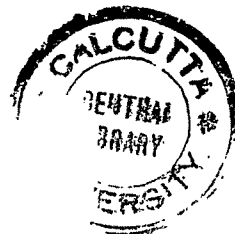
I realize that there is much in these volumes on which I have not commented. To do so would extend an already lengthy review. The essays on Marx and Ricardo are powerful dissections of these systems from the standpoint of modern analysis; the early articles on the multiplier and acceleration remain viable analytical documents; the papers taken from various published symposia on the state of economic thinking are very good reading indeed; and the A.E.A. presidential address stands up surprisingly well, even after a third reading (although, I still do not believe that economists should work solely for each other's applause). What the papers in the last two "books" particularly illustrate, as they move toward greater and greater relevance, is that Samuelson's neoclassical synthesis is heavily weighted on the Keynesian side. They represent an elucidation and extension of the best of economic thinking of the last thirty years. It may be premature to put Samuelson into the pantheon of the greats, but there is no doubt that he is knocking on the door.

One final word and mild protest: the publishers have used a photo offset process to fit all sorts of formats onto a standard $5\frac{1}{2} \times 9$ page. The result is a random sequence of typography ranging from miniscule 4-point type to a font that appears gigantic by contrast. One's eyesight rebels against such

printing barbarities. Nevertheless, the material is there to be read and mulled over on long winter evenings.

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COMMUNICATIONS

Implicit Tax Rate Reductions with Growth, Progressive Taxes, Constant Progressivity, and a Fixed Public Share

The presence of progressive elements in the tax system implies that increases in per capita income will produce more than proportional increases in tax revenue. This well-known aspect of our tax system has been discussed most frequently, until recent years, in terms of contracyclical fiscal policy and "automatic stabilizers." More recently, with the widespread shift of interest to the question of economic growth, the effect of a progressive revenue structure is more likely to be discussed in terms of a persistent contractionary force during periods of rising income and output. The "full employment surplus" concept developed by the Council of Economic Advisers has been used to make this point.

More specifically, let us assume that the share of public sector spending in total spending is to remain constant over a period of sustained growth in total and per capita income. Then a tax system with progressive components will generate ever-rising full employment budget surpluses. If full employment is to be maintained under such circumstances, and if private consumption spending maintains its roughly constant share of disposable income, then private investment must continually rise as a share of total output,¹ tax rates must be periodically reduced, or progressive taxation must be abandoned.² Still more precisely, if planned private full employment saving and investment remain roughly equal over time, the pattern of periodic tax rate reduction must produce tax revenues proportional over time to national income, the required proportion being the desired share of public spending in income and output.

It is the purpose of this paper to state with rigor and precision the pattern of tax rate reductions which will meet these requirements. The question is approached in Part I by means of a theoretical model with highly restrictive assumptions, as indicated below. After the required pattern of tax rate reductions is established for the simple model, the results are generalized sufficiently to talk about the present situation of the United States. In Part II, the 1964-65 personal income tax cut is evaluated in the light of the

¹ This possibility perhaps implies falling interest rates, and/or accelerating technological advance, and an accelerating rate of growth. We shall sidestep all these knotty issues and related questions of liquidity traps, the interest elasticity of investment and the like by concentrating our attention on the tax reduction case. More simply, we may observe that balance-of-payments considerations rule out for the present any possibilities of the easy-money-tight-fiscal-high-investment policy mix.

² We shall sidestep the controversy over progressive taxation by restricting our attention to the case in which progressive taxes are retained. This appears to represent a long-standing consensus in the United States; in fact, political pressures are such that changes in the degree of progressivity are likely to be small in either direction. This observation lends further relevance to our model's concentration on the constant-progressivity (defined below) case.

theoretical analysis of Part I. Part III explores some further implications of the ideas discussed in Parts I and II.

I. *An Aggregate Tax Model*

Assume an economy with the following characteristics: (1) constant population, (2) steadily rising income and output per capita at rate z per year, (3) an unchanged distribution of income over time, (4) no taxes other than a progressive personal income tax, (5) government spending is a constant share of total expenditure, and (6) planned private saving and investment are equal at full employment so that (7) the appropriate behavior of aggregate tax revenues over time is to maintain the share of income given by the desired share of public sector spending in total output. The last assumption implies a continually balanced public budget.

In the model which follows, households (or tax-paying units) are arrayed by income from the lowest to highest; the resulting array may be described by the income function

$$(1) \quad y = f(x)$$

where y refers to an individual household's income and x to its place in the array. The i^{th} household, for example, has income $f(x_i)$. As explained above, the number of households, n , for the moment, is held constant. If we assume a continuous function (as we easily may with n large), aggregate income, Y , is seen to be

$$(2) \quad Y = \int_0^n f(x) dx$$

It also follows that aggregate income is rising at rate z if the income of the household occupying each position in the array is rising through time at rate z , and the distribution of income among households remains unchanged.

The income of the household occupying the i^{th} place in the array is then, at time t ,

$$(3) \quad y_t = e^{zt} f(x_i)$$

and aggregate income at time t is

$$(4) \quad Y_t = e^{zt} \int_0^n f(x) dx$$

Further, it is not strictly required that every household's income rise at rate z ; households may "switch places" as long as the occupant of place i has an income e^z times that of the occupant one period before.

Suppose now that the progressive income tax in question is of the form

$$(5) \quad r = ay^b$$

where r is the tax rate. This function, apparently first fitted to U.S. data in [1], gives a rather good fit to U.S. personal income tax data for 1962, as is

shown in Part II of this paper. This function has a number of useful properties; the tax amount per household, for example, is simply $y \cdot ay^b$ or ay^{b+1} . Aggregate tax revenue, T , is given by

$$(6) \quad T = a \int_0^n [f(x)]^{b+1} dx,$$

A further important property is the following: the elasticity of T with respect to Y (Y changing only via increases in y , distribution unchanged as above) is simply $b+1$.³

The tax is, of course, progressive if $b > 0$, proportional if $b = 0$. With a and b unchanged, T/Y rises through time if Y rises through time by means of rising per capita income.

Now consider the problem of keeping the share of national income paid in taxes constant through time while per capita income rises; and doing so in such a way as to keep the progressivity of the tax system unchanged.⁴ We have already shown that aggregate income is rising at rate z per year, while aggregate tax revenues are rising at rate $z(b+1)$ per year. Our problem, then, is to reduce tax rates steadily in such a way that tax revenues rise only at rate z per year. Referring to (6), we observe that this may be accomplished by steadily lowering the parameter a at rate zb per year.⁵ Each household's income after tax thus rises at rate z , so that the progressivity of the tax structure remains unchanged as required.⁶ This result is consistent with the observation that the parameter b is, after all, the "progressivity" parameter.

Aggregate tax revenues might also be maintained at a constant share of national income by steadily lowering the parameter b ; that is, diminishing the degree of progressivity through time. The appropriate time path for values of b is not as simple as is the case for a , but tax rates may be reduced (or increased) by changing b through time.

Our model may now be generalized in a number of ways. First, consider a tax system with proportional and progressive components. It will behave over time as our model suggests in that aggregate tax revenues will rise at a faster rate than national income, though less rapidly than in our model. Revenue proportionality through time is easily achieved by applying our

³ A formal proof is given in [1] or is available from the author. Briefly, aggregate income is rising at rate z per year, while tax revenues are rising at rate $z(b+1)$ per year. Thus $dT/dY \cdot Y/T$ is $b+1$. The cross-sectional tax-income elasticity is also the aggregate time series tax income elasticity. The result holds regardless of the form of the income distribution function, provided only that it remains unchanged over time.

⁴ By "keeping progressivity unchanged," we mean that the after-tax distribution of income among families does not change through time. Bear in mind that, by assumption, the distribution of before-tax income is also unchanged through time.

⁵ Since the whole expression (6) is linear in a and would otherwise be rising at rate $z(b+1)$, dropping a at rate zb reduces the rate at which (6) rises to $z \cdot 1 = z$.

⁶ By (3) and (5) the after-tax income of the i^{th} household (at time $t=0$) is $f(x_i) - a_0[f(x_i)]^{b+1}$. Its before-tax income, growing steadily at rate z , is (at time t) $y_t = e^{zt}f(x_i)$, and its after-tax income at time t (with the parameter a falling at rate bz from its initial value a_0) is $y_t = e^{zt}f(x_i) - a_0 e^{-bzt}[e^{zt}f(x_i)]^{b+1} = e^{zt}f(x_i) - a_0 e^{zt}[f(x_i)]^{b+1}$. This is simply e^{zt} times the initial after-tax income which is therefore growing at rate z .

results to the progressive components of the tax structure.⁷ Next, consider an economy in which both the number of taxpayer families and family income rise through time. The first factor tends to produce rising tax revenue in proportion to rising national income, while the second factor introduces the effect treated in our model. Again, revenue proportionality may be maintained as suggested in our model.

Cases in which the public sector grows at some rate other than z may again be readily handled; the tax parameter a (or b , if progressivity changes are desired) is simply lowered slower or faster than rate bz , depending on whether the desired public sector growth rate exceeds or falls short of z . Insofar as contracyclical fiscal policy is desired, rate reductions are accelerated in downswings, and delayed in upswings (subject, of course, to the usual forecasting and lag problems). Moreover, the a and b parameters may be varied in offsetting ways. Progressivity may be increased (by raising b) while tax rates in general are reduced (by more than compensating reductions in a). As we shall see, this appears to be the case with respect to the recent tax cut.

II. *The Federal Personal Income Tax in the United States*

The federal personal income tax in the United States may be approximated very closely by the function $t = ay^{b+1}$ where y refers to each taxpayer's adjusted gross income and t to his tax liability. As noted above, this function has already been fitted to U.S. data for 1953. I have calculated mean adjusted gross income and mean tax liability in 1962 for each of the 29 income classes reported in [2] by dividing the number of returns in each class into total adjusted gross income and total tax liability for the class (data reproduced in Appendix I). Regressing logarithms of mean tax liability on logarithms of mean adjusted gross income gives the following estimates of the a and $b+1$ parameters in the above tax function (standard errors in parentheses)⁸

$$(7) \quad \log t = -5.4034 + 1.3589 \log y \quad (r^2 = .9936) \\ (.04615) \quad (.02189)$$

The bottom two income classes (0-\$600 and \$600-1000) were omitted since the first has a zero tax liability (impossible with an exponential function) and the second yields negligible tax revenue. As the small standard errors

⁷ We ignore regressive elements since they are swamped quantitatively in the United States by the federal personal income tax. The reader may easily further generalize our model to cover tax systems with larger regressive components.

⁸ The standard error in this case must be qualified in that we are regressing mean taxes on mean incomes for each income group, and all measures of error apply to these means. Individual taxes are much more widely dispersed, but this dispersion is, in effect, removed by averaging. This qualification is of no particular moment when one deals, as we do, with aggregate tax revenues. It should not be inferred that the standard error of estimate for any individual's tax liability is as low as the standard error of estimate for each class mean.

indicate, estimates of both parameters are significantly $\neq 0$ by the ordinary t test.⁹

The fit is improved, and the estimate of the $b+1$ parameter raised, if the top four income classes are omitted, as follows:

$$(8) \quad \log t = -6.6011 + 1.4943 \log y \quad r^2 = .9996 \\ (.00424 \quad (.00715))$$

The parameter estimates are again (much more) significantly $\neq 0$ by the t test. Equation (8) compared to (7) indicates that the cross-section tax income elasticity declines in the very highest income classes. Actually, the rate structure climbs so steeply up to an income level of \$100,000 that rates in the highest brackets would have to exceed 100 per cent to maintain the pattern of rate increases found below the \$100,000 income level.

Adjusted gross income, to be sure, is not conceptually identical to personal income in the national income and product accounts. Transfer payments are omitted, capital gains are included, and tax-free interest payments (significant only in the high tax brackets) are omitted, to name only a few of the more obvious discrepancies. It is not easy to determine the extent to which our conclusions would be modified if good data on personal income and tax liability arrayed by the same income classes were available. There is the further complicating issue of capital gains (realized and unrealized). Realized gains are included in our adjusted gross income figures, whereas they would not be included in officially estimated aggregate personal income. One might argue that capital gains in part reflect reinvested corporate earnings so that they might properly be regarded to some extent as income. In any event, it is difficult to imagine that the parameter estimates would be drastically affected, especially the comparisons of parameters before and after the 1964-65 tax cuts discussed below.

The next question concerns the parameters of the income tax function after the 1964-65 rate reductions. I have approached this question by taking the 1962 mean adjusted gross income for each of the 29 classes and estimating for each class the mean tax liability which would result from the 1965 rates, with further adjustments for the removal of the dividends-received credit and the establishment of the minimum standard deduction.¹⁰ Other changes in the tax code appear to have little effect on the overall progressivity of the tax and were not considered in estimating 1965 taxes for each income level. The estimates are given in Appendix I.

⁹ Mishan and Dicks-Mireaux in [1] estimated the coefficients as

$$\log t = -2.49801 + 1.42483 \log y \quad (r^2 = .9983)$$

for 1953 taxes. Their result differs from ours principally in the estimate of a , a result not surprising, given the temporary Korean War 10 per cent rate increase still in effect in 1953. The subsequent reduction is reflected in the 1962 rates which were the same in 1962 as in 1954. Various deductions and exemptions had, of course, changed but rates in general had not. Dropping the 10 per cent rate increase (as was done in 1954) would affect a , not b .

¹⁰ More specifically, the application of 1965 rates to mean 1962 adjusted gross income for each income class involved the following steps: (1) for 1962 mean taxable income in each class, calculate the tax liability using both single and married taxpayer rates; (2) select the calculation which most closely approximates the 1962 reported mean tax liability, and calculate the

When estimated mean tax liabilities are regressed on mean incomes (again omitting the two bottom groups), parameter estimates are:

$$(9) \quad \log t = -6.2081 + 1.4174 \log y \quad (r^2 = .9914) \\ (.06737) \quad (.02644)$$

As before, the fit is improved and the estimate of $b+1$ raised if the top four income groups are omitted:

$$(10) \quad \log t = -7.6538 + 1.5809 \log y \quad (r^2 = .9986) \\ (.01468) \quad (.01330)$$

Again, a glance at the standard errors indicates the significance of the parameters.

We may now examine the tax cut in the light of our earlier analysis. As expected, the parameter a is reduced as we compare (7) and (9) or (8) and (10). The hypothesis that a did not change is easily rejected at the .01 significance level in both cases. It appears that a fell about 55 per cent (or, omitting the highest brackets, 65 per cent). If the rate reductions had been confined solely to this parameter, tax revenues from 1962 income would have fallen by about 55 per cent. Using our results of Part I, such a reduction would have absorbed the increase in revenues—over and above a constant proportion of national income—for a period of over 65 years. Actually the reduction in revenues (income held constant) was about 20 per cent; the reduction in a was in part offset by an increase in b . The tax reduction, therefore, disposed of the increase in revenues (beyond a growth proportional to national income) for about 20 years.

Now we consider the hypothesis that the parameter $b+1$ was increased by the 1964 legislation. If we omit the 4 highest income classes and compare (10) with (8), the increase in $b+1$ is significant at the .01 level by the t test. Comparing (9) with (7), the results are less certain. At the .01 level, the difference is not significant; one would have to adopt a significance level slightly above 10 per cent before the increase in $b+1$ would meet the test. We may conclude that the progressivity of the tax clearly increased for income levels up to \$100,000, a result partly explainable by the adoption of a minimum standard deduction and the elimination of the dividends-received credit. For the very highest income groups, the new tax rates remain strongly progressive, but one can give no clear-cut answer as to whether the tax function in that area is more or less progressive than before. We should also mention the probability that errors in estimating tax liabilities on the basis of the 1965 rates are largest in the highest income brackets.

percentage deviation; (3) use 1962 taxable income as the base for estimating the tax liability at 1965 rates (this implies the same deductions and exemptions except as noted below); (4) apply the 1962 single or married rate for each income class as determined by (2), and adjust by the same percentage deviation; (5) recalculate for each class, beginning with the lowest; apply an estimated minimum standard deduction; continue until the results do not differ from the previous method (in this case, the 4000–4500 income bracket); (6) in higher income returns, separately calculate the tax on capital gains, and the dividends-received credit.

III. *Summary and Further Implications*

The foregoing analysis has suggested that federal personal income tax revenues will rise about 1.4 per cent for every 1 per cent increase in income per taxpayer. In a world of continual full employment private saving-investment equality, full employment would require a rising government share in total spending, or periodic tax cuts. If a constant tax-income relation, and a constant degree of progressivity are desired, the indicated tax cut pattern, as noted above, is a reduction in a at the rate bz per annum. In a cyclical, balanced-budget-over-the-cycle world, tax cuts would have to average rate bz per annum, but could be integrated into the pattern of contra-cyclical fiscal policy by delaying rate reductions during upswings, and accelerating them during downswings. If there appears to be some chronic

APPENDIX

MEAN INCOME, AND MEAN TAX LIABILITIES FOR 1962
(ACTUAL) AND AT 1965 RATES (Estimated)

| Income Class | Mean Adjusted Gross Income | Mean Tax Liability 1962 Rates | Mean Tax Liability 1965 Rates |
|-------------------|-------------------------------|-------------------------------------|-------------------------------------|
| (1) | (2) | (3) | (4) |
| 0-600 | 326 | — | — |
| 600-1,000 | 798 | 14 | 8 |
| 1,000-1,500 | 1,241 | 52 | 30 |
| 1,500-2,000 | 1,747 | 91 | 60 |
| 2,000-2,500 | 2,252 | 135 | 90 |
| 2,500-3,000 | 2,749 | 190 | 130 |
| 3,000-3,500 | 3,251 | 247 | 172 |
| 3,500-4,000 | 3,750 | 307 | 217 |
| 4,000-4,500 | 4,249 | 374 | 267 |
| 4,500-5,000 | 4,749 | 439 | 320 |
| 5,000-6,000 | 5,495 | 538 | 400 |
| 6,000-7,000 | 6,482 | 671 | 509 |
| 7,000-8,000 | 7,474 | 831 | 645 |
| 8,000-9,000 | 8,479 | 1,012 | 803 |
| 9,000-10,000 | 9,472 | 1,203 | 968 |
| 10,000-11,000 | 10,470 | 1,389 | 1,129 |
| 11,000-12,000 | 11,468 | 1,599 | 1,310 |
| 12,000-13,000 | 12,467 | 1,813 | 1,493 |
| 13,000-14,000 | 13,468 | 2,031 | 1,678 |
| 14,000-15,000 | 14,469 | 2,244 | 1,858 |
| 15,000-20,000 | 17,006 | 2,842 | 2,360 |
| 20,000-25,000 | 22,220 | 4,230 | 3,512 |
| 25,000-50,000 | 33,350 | 8,016 | 6,677 |
| 50,000-100,000 | 65,688 | 22,089 | 18,585 |
| 100,000-150,000 | 119,375 | 47,403 | 42,693 |
| 150,000-200,000 | 171,150 | 71,498 | 65,160 |
| 200,000-500,000 | 283,564 | 122,145 | 113,010 |
| 500,000-1,000,000 | 666,052 | 295,811 | 279,777 |
| over 1,000,000 | 2,020,222 | 875,761 | 819,659 |

Source: Cols. 1-3, [2, Table 1, p. 13]. Col. 4, Author's estimates.

tendency for private investment to fall short of full employment saving, the whole process (average rate cuts at rate $b\%$ per annum with cyclical adjustment) would have to take place at a lower level of aggregate tax revenues. A further conclusion is that the process of tax reduction is likely, over the long pull, to be a periodic occurrence unless government outlay is to rise consistently at a rate considerably greater than the rate at which total output increases. We should also consider whether smaller, more frequent tax cuts might be more desirable than large reductions of the 1964-65 variety.

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The Myth of Absolute Advantage

Perhaps the oldest, and certainly one of the most viable, myths in all of economics is that of "absolute advantage." Although absolute advantage is a logical impossibility in the context of interregional and international trade theory (as I hope to show in what follows), that has not prevented it from surviving to its one hundred and fiftieth birthday (or beyond if we credit Torrens rather than Ricardo with its origin).

I. Ricardo

The myth begins with Ricardo's exposition in his *Principles* [6, p. 71]. His familiar example is as follows:

To produce an equal quantity of cloth requires: in England, 100 man-years of labor; in Portugal, 90 man-years of labor. To produce an equal quantity of wine requires: in England, 120 man-years of labor; in Portugal 80 man-years of labor. And Portugal is said (in modern terms) to have an "absolute advantage" over England in production of both commodities.¹

Suppose we ask this question: Why does it take 100 man-years to produce in England an amount of cloth which requires only 90 man-years in Portugal? Ricardo says, "It would undoubtedly be advantageous to the capitalists of

¹ Ricardo's example is sometimes turned around with other writers by using an equal number of units of labor in both countries and deriving therefrom different amounts of product. Thus, Ricardo's example could be expressed by saying that with an equal quantity of labor: England will produce 90 yards of cloth or 80 gallons of wine; Portugal will produce 100 yards of cloth or 120 gallons of wine. This, obviously, does not alter the basic question raised, namely, why labor in Portugal is more productive than labor in England.

England and to the consumers in both countries, that under such circumstances the wine and the cloth should both be made in Portugal, and therefore that the capital and labour of England employed in making cloth should be removed to Portugal for that purpose" [6, p. 72]. This does not happen, according to Ricardo, because capital is not mobile between nations.

Consider the implications of the last paragraph: English labor and English capital are not in any inherent respect less efficient than Portuguese labor and capital. The only difficulty with English labor and capital is that *they are in England*. Move them to Portugal and the man-years required for them to produce the given quantity of cloth will drop from 100 to 90. What is it about Portugal that causes this increase in productivity? Ricardo does not say, and the explanation of why he does not say is because there is no reason. To provide a reason for the increase in productivity upon moving to Portugal would require the introduction of a third productive resource available in different proportions in each country or available only in Portugal in addition to labor and capital.² But, in logic, what could be proven about a combination of resources can be proven by limiting the number of resources to two. And to have two resources in one country and three in the other is to vitiate the analysis. Ricardo's original statement about the man-years required to produce the given quantity of cloth in the two countries is inconsistent with his other assumptions.³

II. *The Modern Treatment*

If Ricardo had not made the statement quoted above, his presentation would follow closely the usual modern treatment of absolute advantage in which there is no clear-cut statement that the productive resources considered are, or are not, identical in each location. In fact, what modern writers usually do is one of two things: (1) follow Ricardo and endow one location with a mysterious, and unidentified, *additional* resource which enhances productivity of the named resource(s) in that location, *or* (2) change the standard of measurement of a productive resource. In effect, they are saying that a "man-year" of labor in one location contains, say, 200 man-days in terms of produc-

²One possibility which might occur to the reader, especially since we are dealing with Ricardo, is that Ricardo did, indeed, have a third productive resource in mind—one found in both England and Portugal—land. We can discard this immediately for in Ricardo's thought rent was not a price-determining factor. Ricardo's international trade theory and his value theory are not, however, entirely consistent as is well known.

³Another possibility suggests itself. Could it be that English workers are dull-witted and their machinery rusty and slow in operation? We can note, first, that this was not Ricardo's assumption since merely moving the labor and capital to Portugal would not enlighten the one and oil the other. But can we "save the phenomena" by this assumption? The answer is "No." If we make this assumption, we can explain why England would have a lower per capita real income than Portugal but that is not what we are after. In fact, if we make the assumption we are saying that a "man-year" in England is not the same thing as a "man-year" in Portugal in terms of productivity.

One other thought—diminishing returns—we can discard immediately. Labor and Capital cannot both suffer, simultaneously, diminishing returns with respect to each other. Of course, the actual Ricardian assumption is of constant costs.

tivity, while in the other location a "man-year" contains 100 man-days.⁴ Of course, one can "prove" absolute advantage in this way, but the proof is not likely to be convincing. Since productivity is related to price for a resource and the resource price is related to the cost of production of the product, the two—productivity and cost of production—cannot be separated.

The term "absolute advantage" is also sometimes applied in modern treatments of trade theory to the production of *one* product in a location compared to the production of the same product in a different location (e.g., oranges in Florida and Maine). Analysis of such use will reveal that here, too, one of two different things is meant: (1) that the production can be produced in the favored location very much more cheaply than in the other location. This is merely a statement that the favored location has a large comparative advantage in production of that particular product relative to the second location. But comparative advantage does not become absolute advantage because it happens to be a large comparative advantage. This can be seen if we consider that we have no rule for saying when the comparative advantage in a particular case becomes so large as to cross the line into absolute advantage. Or, (2) that some physical reason makes it impossible to compare the two locations, for example, orange production in Florida as compared with Maine.

This latter case could be turned into a case like (1) above, if we observe that one *can* grow oranges in Maine (in a hothouse) but only at a cost very much greater than oranges may be grown in Florida. The more usual treatment, however, is to say that the same quantities of inputs produce different quantities of output in the two locations. Such statements contain the curious error of assuming that a production function knows where it is and acts differently in different locations. This would be so only if we put location explicitly into the function. But if we do that (e.g., one acre of Florida land in one function and one acre of Maine land in the other function), we no longer have the "same quantities" in both functions. Then, the fact that we get different outputs is not surprising and has no significance. The alternative—to leave location out of the functions, but allow location to affect output—is an obvious misuse of production functions in trade theory.

III. *The One-Resource Case*

"It is not so immediately obvious, but it is no less true, that international trade is mutually profitable even when one of the two countries can produce *every commodity* more cheaply (in terms of labor hours or all resources) than the other country" [7, p. 661]. I quote Professor Samuelson because the appendix to this trade chapter is an excellent, correct, elementary treatment of trade theory in real terms.⁵ No absolute advantage appears in the appendix.

⁴Sometimes a writer makes clear that this is so. See, for example [4, p. 49]. Here we are told explicitly that the land in two different locations is of different quality. Yet an acre of land in one location is treated as equivalent for cost calculations to an acre of land in the other location.

⁵I cite Professor Samuelson also because of the high professional regard in which he is held by economists—including this writer. If he has swallowed the myth, the rest of us do not have to be ashamed of having done so.

Nor can it appear in that (correct) treatment. What, then is wrong with the quotation, which is taken from the main body of the chapter? Let us see.

"... more cheaply (in terms of labor hours . . .)." The word "cheaply" implies money cost although it may be interpreted to mean "number of hours." However interpreted, we can accept it if we know that total cost per unit of product includes more than labor cost. But the original statement would not be a statement of absolute advantage unless we interpret labor to be the only productive resource. In that case we are back with the Ricardian error for that must mean there is some mysterious additional resource which makes labor more productive in one country than the other or we are changing our standard of measurement, and a "labor-hour" in one country does not mean the same thing in terms of productivity as a "labor-hour" in the other country.

IV. *The Two-Resource Case*

An easy way to see the error that economists make in the "more realistic" case of a two-resource model of absolute advantage is to look at a problem posed to students in a popular study guide accompanying a very successful principles text [1, pp. 16-17].

The problem is as follows:

| | <i>Economic Units [Areas]</i> | |
|------------------|-------------------------------|----|
| | A | B |
| Units of capital | 10 | 15 |
| Units of labor | 30 | 45 |
| Production: | | |
| Units of X | 60 | 60 |
| Units of Y | 20 | 25 |

One of the questions asked the student is which area has the absolute advantage. The answer given, of course, is that area A has the absolute advantage.

If a "unit of capital" is the same thing in area B as in area A, and a "unit of labor" is the same thing in area B as in area A, then if area B used 10 units of capital and 30 units of labor, it would produce the same output as area A, namely, 60 units of Product X and 20 units of Product Y. Now, as given in the problem, area B does produce 60 units of Product X, but produces 25 units of Product Y. It does this by using 5 *additional* units of capital and 15 *additional* units of labor.

Therefore, the last 5 units of Product Y *cost* 5 units of capital and 15 units of labor. This is impossible in the light of the costs implied in the rest of the table. In fact, consistency with the assumptions would require production in area B to be 90 units of Product X and 30 units of Product Y and there is no specialization (and no problem).

The only other way out is to say that a "unit of capital" in area B is not the same thing as a "unit of capital" in area A and similarly for a "unit of labor." In that case the numbers "15" and "10" for the amount of capital in each area, respectively, are noncomparable as are the numbers given for the

amounts of labor. But, then, the fact that production is different in the two areas would not surprise us, but we have no way to compare the two areas.

V. *The All-Resources Case*

"... more cheaply (in terms of . . . all resources) . . ." (See full quote beginning Section III, above.) If this is meant in real terms, our mysterious additional resource must be present in the favored country although how it can be when we already have all resources is not easy to see. If this is meant in terms of money costs, it is logically impossible. Since the demand for productive resources is a derived demand, there would be no demand for productive resources in the country with higher money costs for all products. What, then, would keep up the price of productive resources and, thus, money costs, in that country? The answer is "Nothing." Therefore, the prices of productive resources in the higher money-cost country would have to fall until the total cost per unit of producing some product was cheaper in that country than the other.

VI. *The Source of the Error*

The error, although it is not always easy to see, is made whenever absolute advantage is demonstrated in real terms. Let us look at Samuelson's example of absolute advantage [7, p. 663]:

| <i>Product</i> | <i>Labor Requirements for Production</i> | |
|--------------------|--|------------------|
| | <i>In America</i> | <i>In Europe</i> |
| 1 unit of food | 1 day's labor | 3 days' labor |
| 1 unit of clothing | 2 days' labor | 4 days' labor |

It is immediately obvious that a day's labor is not the same thing in Europe as in America in terms of productivity. We cannot, therefore, measure cost of production in terms of "labor-days" since these are not standard for both areas. Since products exchange according to their prices (assumed to equal cost of production where produced) we can conclude nothing about trade until we provide these prices. Since we have no *numeraire*, the only thing we can do is to express food costs in terms of clothing and clothing costs in terms of food. Thus we obtain relative prices for each product. When we do this we obtain the following table:

| <i>Product</i> | <i>Product Prices</i> | |
|--------------------|--------------------------------|--------------------------------|
| | <i>In America</i> | <i>In Europe</i> |
| 1 unit of food | $\frac{1}{2}$ unit of clothing | $\frac{3}{4}$ unit of clothing |
| 1 unit of clothing | 2 units of food | $1\frac{1}{3}$ units of food |

Expressed in this manner, no one is likely to be surprised to find that America specializes in food production, Europe in clothing production. But our table looks like the kind of table used to explain comparative advantage, not absolute advantage. The reason for this is simple: we cannot construct any table like Samuelson's without having it convert to a table like ours when we price the products being considered. (Except, of course, the uninteresting case in which prices are the same in both countries for each product.)

Now, the reason our table of prices will always turn out to be a comparative advantage table is also very simple. The only way the price of home-produced food can be higher in Europe than America is for the price of home-produced clothing to be lower in Europe than America. The only way you can be taller than your friend is for your friend to be shorter than you.

What Samuelson's table tells us is something quite different from what it purports to tell us. What it tells us is that per worker real income in Europe will be lower than per worker real income in America, that is, Europe is (very likely) a poorer country than America. What is called "absolute advantage" turns out to be something quite different—a demonstration that a poor country may trade with a rich country to the mutual benefit of both. This may be a useful thing to demonstrate, but economists should not treat it as one of two mutually exclusive cases (comparative advantage being the other). After all, no two countries in the world have precisely the same per capita or per worker real income so all trade is trade between a richer country and a poorer country. And all trade is based upon comparative advantage.⁶

VII. Conclusion

Is it not true that modern economists really know that absolute advantage does not exist—that it is a logical impossibility? After all, in solving the problem of trade in cases of absolute advantage we do convert the case to a comparative advantage case even though we tend to use awkward phrases like "absolute disadvantage" in the process. On this question I am inclined to take the naïve view that if we knew absolute advantage did not exist we would not write about it. As a matter of fact, there are examples which professional decency prohibits my citing in which an economist makes absolute advantage work only by having consumers, faced with the choice of paying two different prices for an identical product, choose to buy the higher-priced one. I do not believe economists should demand such a sacrifice of consumers in order to retain a cherished myth.

One way out for an author is simply to ignore the whole subject of absolute advantage rather than attempt to destroy the myth. At the text level of presentation this would appear much the best solution for pedagogical reasons. P. B. Kenen's little book [3, p. 7-8] follows this path as did my own effort [2, pp. 267-71] five years earlier. Both of these treatments obviously owe much to B. G. Ohlin's classic work [5].

In 1967, we celebrate the sesquicentennial of the publication of Ricardo's *Principles*. It would appear fitting that economists discard the myth of absolute advantage as a way of paying homage to that great figure in the history of economic thought.

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⁶ We do not mean, of course, that differences in per capita or per worker real incomes are necessary as a basis for trade. Trade might well take place if these measures of economic well-being were equal in both locations or both countries. Those measures are simply irrelevant to any analysis of the basis for trade.

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On the Determinants of Income Equality

The purpose of this note is to report further evidence of the tendency for the personal income of a region to be more equally divided the more maturely developed is the region. But more importantly our findings, which apply to the U. S. economy on a state cross-sectional basis, derive from an attempt to isolate development per se as a causal variable. Thus the conclusions reached in this study are not subject to the same criticism as might be directed toward "evidence" of the Kuznets [3, 4] phenomenon were it used to support the hypothesis of the state of economic development as a "reason" for income equality differences between regions or nations.

The results of our efforts to relate income equality in the states to development and various noneconomic factors, besides lending support to the above hypothesis, also point up the importance of racial barriers to increased income equality in the United States. The proportion of non-white population in the states is a significant explanatory variable (negatively) in the regression models of the study. Of additional interest, both the average age and the proportion of the population engaged in mining and manufacturing appear also as significant explanatory variables.

I. Introduction to the Models

The recent discussion in this *Review* concerning the Kuznets thesis that income is more equally divided in mature than developing economies involves a straightforward observational problem, its form ultimately depending upon the concept of income employed in constructing the data, the reliability of the samples, and the measures of equality used. It is obvious that these factors make a good deal of difference in determining the specific conclusions reached [8, 9, 15].

Of much greater significance is an understanding of the development process and reasons for such apparent distributional differences in income: Is there anything about the process of capital accumulation and hence economic development that tends to make the personal distribution of income become more equal? There exists only introductory work on the subject at present.

Soltow's article [14] indicates that educational level plays an important role in support of such a relationship, as shown by the existence of greater equality among more highly skilled or educated groups of people. And in an effort to explain the parallel movement of equality of the overall distribution of income and per capita income differences between the states, Smolensky [13] conjectures that increased mobility, particularly the movement from rural to urban areas during the period 1880-1950 in the United States, is the primary factor tending to reduce dispersion in incomes. While decreased regional dispersion is not necessarily coincidental with decreased dispersion within regions, one would expect that mobility tends to diminish both interregional and intraregional inequality in incomes if it is a significant factor at all.

Despite these efforts and the work of Kravis [2] and Kuznets [3, 5] elsewhere, there is no formal theory available which satisfactorily concerns itself with the relationship between development and the inequality of incomes. So the matter has generally rested on observations of the income distributions in a variety of countries, where the data bases used for such observations are rather different and thus lead to debatable conclusions concerning consistency or inconsistency with the Kuznets generalization. The question just raised cannot be broached with such observations and the recent discussion contributes but another set of them.

Recognizing that development inexorably brings many noneconomic changes, to approach the problem we pose it would be appropriate to hold social, cultural, and political factors constant in order to identify changes in the equality of income derived from the economic development process per se. And, certainly, comparability of the data used is a necessary ingredient in the observation process. The 1960 Census provides us with detailed data on the distribution of family income in the fifty states of the United States and, since data were similarly constructed in all cases and the political and economic structure is rather homogeneous at least across the continental United States, these data should yield some interesting insights into the impact of regional development on the personal distribution of income.

One would expect that social and cultural attributes have their impact on the distribution of income, Pareto's thesis not to the contrary [12]. Kravis suggests the following attributes which, aside from the state of development, could be expected to impinge on the income distribution: (1) human characteristics, (2) barriers to mobility, e.g., legal and such social barriers as are involved in racial discrimination, (3) economic structure, and (4) social and political organizations [2, p. 411].

Using the development hypothesis as a base, we have formulated a linear regression model for the states (excluding Alaska and Hawaii) in an attempt to explain variations in equality of the income distribution, as measured by the equality ratio,¹ by mean family income of the population as development indicator, and a variety of control variables which substantially implement the factors outlined by Kravis, as follows:

¹The area under the Lorenz curve as a percentage of maximum possible area. This measure is the complement of Gini's concentration ratio in percentage terms.

Quantitative Factors:

- Y : Area under Lorenz curve as a percentage of maximum possible area (measure of equality)
- X_1 : Percentage of population white
- X_2 : Percentage of population urban
- X_3 : Median school years completed
- X_4 : Percentage unemployment
- X_5 : Median age of population
- X_6 : Percentage of population employed in mining and manufacturing
- X_{13} : Mean family income

Qualitative Factors:

- X_7 : 1 if consistently Republican; 0 if not
- X_8 : 1 if consistently Democratic; 0 if not
(X_7 and X_8 both 0 if no voting consistency)
- X_9 : 1 if using sales tax; 0 if not
- X_{10} : 1 if using income tax; 0 if not
(X_9 and X_{10} may be both 0 or both 1)
- X_{11} : 1 if conservative state; 0 if not
- X_{12} : 1 if liberal state; 0 if not
(X_{11} and X_{12} both 0 if rated middle-of-the-road)

The dummy variables X_7 and X_8 are based on state voting records during the period 1940-60 in presidential elections, both variables assuming zero values if the given state was split 3-3 as between Republican-Democrat over the six elections considered. Obviously strict party affiliation is not a sufficient indicator of adherence to given social policies, particularly for some Southern states. Thus an additional set of dummy variables, X_{11} and X_{12} , is included to offset whatever biases may be inherent in X_7 and X_8 . These variables are assigned values on a purely judgmental basis and hence may be questioned on subjective grounds.²

Since we have little more than judgment to rely upon in anticipating the partial relationships of income equality and these variables, any speculating we do as regards these relationships cannot be derived from more than a naive theory except for the case of mean income, where the Kuznets thesis suggests a positive relationship.³ Without launching into a lengthy harangue concerning the remaining variables, for all of which some a priori case can be made, our interest centers more toward an evaluation of their relevance than testing any hypotheses about them.

² Here we use the terms "liberal" and "conservative" to denote, on a state basis, commitment to large redistribution and welfare programs or a basic *laissez faire* philosophy, respectively.

³ Thomas Hopkins, in a recent article [6] on the use of median income as an appropriate indicator of state fiscal capacity and/or need in the allocation of federal grants-in-aid, finds a strong relationship between state median income and the Gini ratio, using the 1960 data on families and unrelated individuals. One could thus justly expect a similar relationship between the Gini measure and mean income (for this study, mean family income) to present itself.

II. *The Empirical Results*

Table 1 contains the results of six alternative configurations of variables, with partial regression coefficients, *t*-ratios,⁴ and *R*²s (unadjusted) noted. It is significant to the reliability of these results that there is an overwhelming lack of intercorrelation in the independent variables, as seen in Appendix A.

Throughout the six variants, if a 99 per cent confidence level is used, the significant variables are (1) mean family income and (2) the percentage of population which is white. In variant (6) the percentage of the population employed in mining and manufacturing is also significant at the 99 per cent level. With respect to the behavior of net regression coefficients for X_1 and X_{13} it is interesting to note that they are very stable over five of six variants, with b_1 ranging between .17 and .21 in all but variant (4) and the estimated coefficient for mean income taking on values between .0018 and .0022 except in variant (5).

Thus it would seem that the Kuznets hypothesis is supported in a situation where a high degree of control is exercised over other population attributes, implicitly by the basic similarity of political and economic structure throughout the United States and explicitly by the other variables employed. The significance of X_1 as an explanator of variations in income equality would seem to enforce already prominent evidence on the effect of racial differences on income and its distribution.

Of the "traditional" variables other than X_6 , median age of the population is significant at the 95 per cent level in both variants (5) and (6). This variable could be expected to exhibit a negative partial regression coefficient for a variety of reasons⁵ but primarily because an older population would likely have a high degree of rigidity of skills and hence cause enforcement and proliferation of already established equality barriers. Variable X_3 , median school years completed, was chosen to describe the basic skill level of each state population and is significant in variant (6) at the 90 per cent level.

The significance of X_2 at the 80 per cent level is interesting also in light of Kuznets' views on income equality in developed versus underdeveloped economies. If we adopt the urban center as a focal point for capital formation, then through Kuznets' eyes we should expect a tendency toward greater inequality of income for those states with large urban population concentrations. While this conjecture is partially borne out by the data, the evidence is undoubtedly clouded by interactions with racial barriers in the urban center and the occupational mix variable, X_6 . Such interactions, while expected in general, would appear inconsequential for these data as inferred from the values for the appropriate correlation coefficients.

For the qualitative variables, in variants (3) and (4) a particular political affiliation and "philosophy" are of significance (95 per cent level) in explaining variations in income equality, X_7 appearing in variant (4) with a positive sign

⁴ Critical values for "*t*" are approximately ± 2.70 for 99 per cent, ± 2.02 for 95 per cent, ± 1.68 for 90 per cent, and ± 1.30 for 80 per cent.

⁵ See, e.g., Morgan [8].

TABLE 1—REGRESSION COEFFICIENTS
(*t*-ratios in parentheses)

| Regression Variant | Variables Included | | | | | | | | | | | | | R ² (d.f. in parentheses) |
|--------------------|--------------------|------------------|-----------------|-------------------|------------------|----------------|----------------|------------------|----------------|-------------------|-----------------|-----------------|------------------|---|
| | X ₁ | X ₂ | X ₃ | X ₄ | X ₅ | X ₆ | X ₇ | X ₈ | X ₉ | X ₁₀ | X ₁₁ | X ₁₂ | X ₁₃ | |
| 1 | .198 (6.39) | | | -.005 (-.020) | | .034 (1.24) | | | | | | | .00180 (5.68) | .78 (43) |
| 2 | .204 (6.04) | | | -.061 (-.025) | | .035 (1.25) | | | .623 (0.95) | -.355 (-.0.55) | | | .00176 (5.49) | .79 (41) |
| 3 | .210 (6.93) | | | .181 (0.76) | | .030 (1.11) | | | | | 1.690 (2.42) | .378 (0.49) | .00216 (6.36) | .81 (41) |
| 4 | .155 (4.82) | | | .131 (0.59) | | .027 (1.03) | 1.44 (2.12) | -.722 (-1.06) | | | | | .00183 (5.67) | .82 (41) |
| 5 | .211 (6.52) | -.057 (-1.92) | | -.096 (-.0.45) | -.293 (-2.18) | .069 (2.45) | | | | | | | .00265 (5.85) | .83 (41) |
| 6 | .172 (4.18) | -.041 (-1.33) | 1.020 (1.83) | .149 (0.64) | -.333 (-2.53) | .096 (3.16) | .440 (0.63) | -.074 (-1.02) | .430 (0.77) | -.344 (0.60) | .970 (1.27) | .761 (1.01) | .00204 (3.88) | .89 (34) |

(corresponding to a consistently Republican voting record), and X_{11} in variant (3) with a like sign (corresponding to a conservative political and social attitude). Both variables fall to significance levels below 80 per cent in the main equation, variant (6).

It is, however, inappropriate to use the individual t -ratios in Table 1 for assessing the importance of the sets of dummy variables, e.g., political affiliation as a structural variable, since the dummy variables in question were constructed in tandem. Thus to inquire after the statistical significance of political party affiliation requires that the joint hypothesis $\beta_7 = \beta_8 = 0$ be tested. A similar remark applies in the case of tax structure (X_9 and X_{10}) and political philosophy (X_{11} and X_{12}).

TABLE 2—STATISTICS FOR TESTING QUALITATIVE VARIABLES

| Basic Variant | Hypothesis | F -statistic | d.f. f_1, f_2 | Selected F -levels for rejection |
|---------------|---|----------------|--------------------|---------------------------------------|
| 2 | $\beta_9 = \beta_{10} = 0$ | 0.87 | 2, 41 | $F_{.99, 2, 41} = 5.17$ |
| 3 | $\beta_{11} = \beta_{12} = 0$ | 2.99 | 2, 41 | $F_{.95, 2, 41} = 3.23$ |
| 4 | $\beta_7 = \beta_8 = 0$ | 4.55 | 2, 41 | $F_{.90, 2, 41} = 2.44$ |
| 6 | $\beta_7 = \beta_8 = \beta_{11} = \beta_{12} = 0$ | 4.13 | 4, 34 | $F_{.99, 4, 34} = 3.93$ |
| | $\beta_9 = \beta_{10} = 0$ | 6.77 | 2, 34 | $F_{.995, 2, 34} = 6.22$ |
| | $\beta_7 = \beta_8 = 0$ | 8.03 | 2, 34 | |
| | $\beta_{11} = \beta_{12} = 0$ | 6.77 | 2, 34 | |

Table 2 presents the relevant F -statistics, using variants (2), (3), and (4) and variant (6) as bases for the testing of coefficients taking on zero values simultaneously. Seven hypotheses are considered, with primary interest centering on those within variant (6). For this case, each individual qualitative attribute of the states as well as the composite political factor (political party and/or philosophy) is significant at a 99 per cent level or above.

III. Some Final Remarks

The Kuznets observation is primarily interesting because it suggests that economic development per se may be a harbinger of social justice. The results of the present paper support such a hypothesis, and that is a rather comforting thought. This is not to suggest that social and economic policies designed to redistribute resources are useless, but, instead, that time and wealth accumulation are on the side of equality.

That the evidence indicates racial makeup as an important determinant of income inequality is not surprising. And, if the distribution of resources is an important aspect of overall social justice, then it would seem that recent pub-

lic policies designed to remove racial barriers in the social and economic market places of the United States are not misconceived, at least on the basis of their intent.

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APPENDIX B—DATA AND SOURCES, U. S. BY STATES, 1960

| | Equality Ratio (per cent) ^a | Percentage of Population White ^a | Percentage of Population Urban ^a | Median School Yrs. Completed ^b | Percentage Unemployment ^c | Median Age of Population ^d (years) | Percentage Employed in Mining and Manufacturing ^e | Political Affiliation ^f | | Tax Base ^g | | Political Philosophy ^f | | Mean Family Income ^h (dollars) |
|-------------|--|---|---|---|--------------------------------------|---|--|------------------------------------|---------------------|-----------------------|------------------------|-----------------------------------|----------------------|---|
| | | | | | | | | Rep. X ₇ | Dem. X ₈ | Sales X ₉ | Income X ₁₀ | Cons. X ₁₁ | Lib. X ₁₂ | |
| Alabama | 56.79 | 69.9 | 55.0 | 9.5 | 5.6 | 25.9 | 26.5 | 0 | 1 | 1 | 1 | 1 | 0 | 4816.47 |
| Alaska | 65.03 | 77.2 | 37.9 | 12.0 | 14.4 | 23.3 | 7.1 | 0 | 1 | 0 | 1 | 0 | 1 | 8197.05 |
| Arizona | 61.70 | 89.8 | 74.5 | 10.9 | 5.3 | 25.6 | 12.8 | 0 | 0 | 1 | 1 | 0 | 0 | 6573.85 |
| Arkansas | 55.74 | 78.1 | 42.8 | 9.2 | 5.6 | 28.8 | 20.1 | 0 | 0 | 1 | 1 | 0 | 0 | 4131.41 |
| California | 64.93 | 92.0 | 86.4 | 11.9 | 5.8 | 30.0 | 24.2 | 1 | 0 | 1 | 1 | 0 | 0 | 7789.70 |
| Colorado | 64.75 | 89.4 | 80.7 | 10.6 | 4.3 | 31.2 | 31.9 | 1 | 0 | 1 | 1 | 0 | 0 | 7573.79 |
| Connecticut | 66.08 | 95.6 | 78.3 | 10.8 | 3.9 | 31.8 | 40.2 | 0 | 0 | 1 | 0 | 0 | 0 | 8212.22 |
| Delaware | 62.58 | 86.1 | 65.6 | 10.9 | 4.7 | 28.8 | 32.8 | 0 | 0 | 0 | 0 | 0 | 0 | 7493.84 |
| D.C. | 59.46 | 45.2 | 100.0 | 11.6 | 4.4 | 32.2 | 6.2 | 0 | 1 | 0 | 0 | 0 | 1 | 7723.47 |
| Florida | 59.13 | 82.1 | 74.0 | 10.7 | 4.9 | 31.1 | 13.1 | 0 | 0 | 1 | 0 | 1 | 0 | 5847.65 |
| Georgia | 57.98 | 71.4 | 55.3 | 9.5 | 3.9 | 25.9 | 26.3 | 0 | 1 | 1 | 1 | 1 | 0 | 5167.43 |
| Hawaii | 64.08 | 32.0 | 76.5 | 11.2 | 3.3 | 24.3 | 16.0 | 1 | 0 | 1 | 1 | 0 | 1 | 7645.55 |
| Idaho | 66.08 | 98.5 | 47.5 | 11.3 | 5.4 | 25.9 | 13.5 | 0 | 0 | 0 | 1 | 0 | 0 | 5935.70 |
| Illinois | 64.74 | 97.0 | 73.7 | 11.9 | 3.9 | 27.8 | 15.7 | 0 | 1 | 1 | 0 | 0 | 0 | 6684.67 |
| Indiana | 65.69 | 94.1 | 62.4 | 10.7 | 3.9 | 28.9 | 35.4 | 1 | 0 | 1 | 0 | 1 | 0 | 6557.53 |
| Iowa | 62.34 | 99.0 | 53.1 | 11.2 | 3.3 | 30.3 | 18.6 | 0 | 0 | 1 | 1 | 1 | 0 | 5826.94 |
| Kansas | 63.07 | 95.4 | 61.0 | 11.5 | 3.6 | 29.9 | 16.6 | 1 | 0 | 1 | 1 | 1 | 0 | 6165.20 |
| Kentucky | 56.92 | 92.8 | 44.5 | 8.9 | 6.3 | 27.6 | 21.2 | 0 | 1 | 0 | 1 | 0 | 0 | 4914.82 |
| Louisiana | 57.20 | 67.9 | 63.3 | 9.3 | 6.3 | 25.3 | 15.6 | 0 | 1 | 1 | 1 | 1 | 0 | 5303.10 |
| Maine | 66.73 | 99.4 | 51.3 | 10.7 | 6.2 | 29.3 | 33.1 | 1 | 0 | 1 | 1 | 0 | 1 | 5474.50 |
| Maryland | 64.61 | 83.0 | 72.7 | 10.5 | 4.7 | 28.6 | 24.5 | 0 | 0 | 1 | 1 | 1 | 0 | 7366.72 |

^a Calculated from data or data taken from 1960 Census of Population, Vol. I, Parts 2-52 [hereafter referred to as Census], Table 65. Equality ratio is area under the Lorenz curve as a proportion of maximum possible area, viz., one minus the concentration ratio.

^b Census, Table 47.

^c Census, Table 52.

^d Census, Table 16.

^e Census, Table 62.

^f See text.

^g Retail Sales and Individual Income Taxes in State Tax Structures, Tax Foundation, Inc., Project Note No. 48 (1962).

APPENDIX B—(continued)

| | Equality Ratio (per cent) ^a | Percentage of Population White ^a | Percentage of Population Urban ^a | Median School Yrs. Completed ^b | Percentage Unemployment ^c | Median Age of Population ^d (years) | Percentage Employed in Mining and Manu- facturing ^e | Political Affiliation ^f | | Tax Base ^g | | Political Philosophy ^f | | Mean Family Income ^a (dollars) |
|---------------|--|---|---|---|--------------------------------------|---|---|------------------------------------|---------------------|-----------------------|--------------------------|-----------------------------------|----------------------|---|
| | | | | | | | | Rep. X ₇ | Dem. X ₈ | Sales X ₉ | In- come X ₁₀ | Cons. X ₁₁ | Lib. X ₁₂ | |
| Massachusetts | 59.42 | 97.6 | 83.6 | 11.3 | 4.3 | 32.1 | 35.5 | 0 | 1 | 0 | 1 | 0 | 1 | 6113.88 |
| Michigan | 66.13 | 90.6 | 73.4 | 10.8 | 6.8 | 28.2 | 38.0 | 1 | 0 | 1 | 0 | 0 | 1 | 7104.92 |
| Minnesota | 63.69 | 98.8 | 62.1 | 10.9 | 5.4 | 28.5 | 19.5 | 0 | 1 | 0 | 1 | 0 | 1 | 6342.03 |
| Mississippi | 52.98 | 57.7 | 37.7 | 9.1 | 5.0 | 24.1 | 19.1 | 0 | 1 | 1 | 1 | 1 | 0 | 3886.70 |
| Missouri | 60.53 | 90.8 | 66.6 | 10.1 | 5.0 | 31.6 | 24.7 | 0 | 1 | 1 | 1 | 0 | 0 | 5979.07 |
| Montana | 65.34 | 96.4 | 50.2 | 11.3 | 6.6 | 27.4 | 10.2 | 0 | 1 | 0 | 1 | 0 | 0 | 6100.73 |
| Nebraska | 62.51 | 97.4 | 54.3 | 11.5 | 3.0 | 30.2 | 12.2 | 1 | 0 | 0 | 0 | 1 | 0 | 5692.67 |
| Nevada | 66.07 | 92.3 | 70.4 | 12.0 | 5.8 | 29.4 | 6.3 | 0 | 1 | 1 | 0 | 0 | 1 | 7770.51 |
| New Hampshire | 67.98 | 99.6 | 58.3 | 10.7 | 3.9 | 30.9 | 39.8 | 1 | 0 | 0 | 1 | 1 | 0 | 6376.09 |
| New Jersey | 66.41 | 91.3 | 88.6 | 10.6 | 3.9 | 32.3 | 36.1 | 0 | 0 | 0 | 0 | 0 | 0 | 7910.12 |
| New Mexico | 61.34 | 92.1 | 65.7 | 10.9 | 6.0 | 22.7 | 7.6 | 0 | 1 | 1 | 1 | 0 | 0 | 6238.57 |
| New York | 64.05 | 91.1 | 85.4 | 10.8 | 4.9 | 33.0 | 28.6 | 0 | 0 | 0 | 1 | 0 | 1 | 7625.27 |
| No. Carolina | 57.98 | 74.6 | 39.5 | 9.5 | 3.6 | 25.5 | 31.7 | 0 | 1 | 1 | 1 | 1 | 0 | 4817.37 |
| No. Dakota | 63.15 | 98.0 | 35.2 | 10.0 | 5.9 | 26.0 | 3.7 | 1 | 0 | 1 | 1 | 1 | 0 | 5229.42 |
| Ohio | 66.15 | 91.8 | 73.4 | 10.8 | 5.4 | 29.4 | 37.0 | 1 | 0 | 1 | 0 | 1 | 0 | 7006.17 |
| Oklahoma | 59.24 | 90.5 | 62.9 | 10.6 | 4.4 | 30.0 | 13.2 | 0 | 0 | 1 | 1 | 0 | 0 | 5542.32 |
| Oregon | 65.94 | 97.9 | 62.2 | 11.5 | 6.0 | 30.7 | 25.4 | 1 | 0 | 0 | 1 | 0 | 1 | 6699.01 |
| Pennsylvania | 65.56 | 92.4 | 71.6 | 10.4 | 6.5 | 32.0 | 36.4 | 0 | 0 | 1 | 0 | 1 | 0 | 6612.76 |
| Rhode Island | 66.53 | 97.6 | 86.4 | 10.1 | 4.7 | 31.9 | 39.3 | 0 | 1 | 1 | 0 | 0 | 1 | 6348.73 |
| So. Carolina | 57.42 | 65.1 | 41.2 | 9.2 | 3.6 | 23.4 | 32.1 | 0 | 1 | 1 | 1 | 1 | 0 | 4618.46 |
| So. Dakota | 60.99 | 96.0 | 39.3 | 10.5 | 4.1 | 27.6 | 6.6 | 1 | 0 | 1 | 0 | 1 | 0 | 4936.89 |
| Tennessee | 56.73 | 83.5 | 52.3 | 9.2 | 5.1 | 27.9 | 26.0 | 0 | 0 | 1 | 1 | 1 | 0 | 4878.87 |
| Texas | 58.82 | 87.4 | 75.0 | 10.4 | 4.4 | 26.9 | 16.2 | 0 | 1 | 0 | 0 | 1 | 0 | 5897.28 |
| Utah | 68.04 | 98.1 | 74.9 | 12.1 | 3.9 | 22.9 | 16.0 | 0 | 0 | 1 | 1 | 0 | 0 | 6658.19 |
| Vermont | 65.26 | 99.8 | 38.5 | 10.7 | 4.5 | 29.2 | 24.9 | 1 | 0 | 0 | 1 | 1 | 0 | 5618.82 |
| Virginia | 59.51 | 79.2 | 55.8 | 10.1 | 4.0 | 27.1 | 22.4 | 0 | 0 | 0 | 1 | 1 | 0 | 5940.37 |
| Washington | 66.85 | 96.4 | 68.1 | 11.8 | 6.2 | 29.5 | 24.7 | 0 | 0 | 1 | 0 | 0 | 1 | 7032.00 |
| W. Virginia | 60.08 | 95.1 | 38.2 | 9.1 | 9.3 | 28.4 | 33.3 | 0 | 1 | 1 | 0 | 0 | 0 | 5177.91 |
| Wisconsin | 63.84 | 97.6 | 63.8 | 10.6 | 3.8 | 29.3 | 32.9 | 1 | 0 | 0 | 1 | 0 | 1 | 6696.73 |
| Wyoming | 66.23 | 97.8 | 56.8 | 11.8 | 5.0 | 27.1 | 7.7 | 1 | 0 | 1 | 0 | 1 | 0 | 6680.70 |

The Biological Interest Rate and the Social Utility Function

Samuelson [5] considered the problem of the optimal allocation of goods between per capita consumption of different generations living in a given period of time, in an economy with no capital and in which goods do not keep. He concluded that the optimal allocation would be the same as the allocation an individual would make for his own consumption in the different periods of his life, if he were facing a market rate of interest equal to the rate of growth of the labor force, which he called the biological rate of interest. Diamond [1] has extended this conclusion to an economy with capital, which is controlled by a central planning authority and is on the Golden Rule Path.

The Samuelson and Diamond economies are similar in two important respects: changes in investment cannot increase the amount of consumption goods available, and the allocation of these goods between the working and retired generations living in the same period is independent of investment decisions and the return to investment. These two attributes of the Samuelson model follow from its definition which ensures that any investment, or attempt to store goods, results in the complete loss of the goods involved, and therefore some universally observed rule is necessary to provide for the retired. In Diamond's Golden Rule economy, it can be shown that changes in investment cannot increase the flow of consumption goods, when alternative steady states are considered, even though the marginal product of capital is positive. The distribution of this maximum flow of consumption goods between the generations living in a given period is then determined by the central planning authority. In both models, therefore, the nature of the inter-generation allocation follows separately after the specification of the society's utility function for consumption of these two generations.

Samuelson approached this problem from the point of view of a representative man and used his representative man's utility function to decide optimal allocation, while Diamond's interpretation of the results obtained with the social utility function in his model, indicate that it too is identical to the utility function of a representative individual. However, a different formulation of the social utility function, and thus a different optimal allocation of consumption, is not only possible but may be preferable, even if the central planning authority is to be guided in its decisions by individual tastes. This alternative social utility function, which is concerned with the utility of all individuals living in a given period, was put forward by Lerner [2, 3] in his comments on Samuelson's article.¹ The points he raised also apply to Dia-

¹ The disagreement between Samuelson [6] and Lerner [2, 3] over the optimality of the biological rate of interest can be reduced to a disagreement over the appropriate social utility function. Should it be concerned with the utility of a representative man over the two periods of his life (Samuelson) or with the utility of all individuals living in a given period, with the relative weight given to the individual consumption of workers and retired determined by the representative man's attitude to consumption in the two periods of his life (Lerner)? Lerner believes that his approach should be followed because it does not depend, as does Samuelson's, on the indefinite extension of the rate of growth of the labor force. It deals with each period separately but always in the same consistent way.

mond's extension of Samuelson's conclusion to a Golden Rule economy. If population is growing, the number of workers in each period is greater than the number of retired. Use of the Lerner social utility function in such a situation, when there is no individual time preference, would result in equal per capita consumption of both groups, while the Samuelson social utility function would prescribe greater individual consumption for the retired.

Diamond's conclusion holds without qualification for a very special case which he does not consider in this context. In the individualistic and competitive economy he investigates in the main body of his paper, the consumption of the working and retired generations is determined solely, given the technical characteristics of the system, by their investment decisions and the return to investment. The only relevant utility function is that of a representative man. If this economy is on a Golden Rule Path, then the intergeneration allocation of consumption goods is the same as the individual allocation when facing the biological rate of interest.

I. *Social Compact or Centrally Planned Economy*

In Samuelson's economy, goods do not keep, and workers can only provide for their retirement by imposing restraints on their behavior which are also binding on all future generations. An individual life can be taken to consist of two equal periods, one of work and the other of retirement. The question he posed is, what common rule for the allocation of goods between generations will "lead each representative man to the highest point on his preference field?" [6, p. 520]. His answer, that this rule would provide an allocation identical to what an individual would make for his own consumption in the two periods of his life, if he were facing a market rate of interest equal to the biological rate of interest, is derived by using a social utility function which is identical to that of his representative man's. This can be readily demonstrated. Samuelson's society maximizes utility subject to the physical constraint of the output available. That is, it maximizes $U(c_1, c_2)$ subject to $c_1 + c_2/(1+n) = 1$, where:

output per worker is equal to unity;

c_1, c_2 represent consumption per capita of worker and retired generations, respectively; or, what is equivalent, it is the consumption of an individual in the two periods of his life;

n rate of growth of the labor force is assumed to be constant, with $L_1 = L_2(1+n)$;

L_1, L_2 the size of the worker and retired generations, respectively.

The optimal allocation then clearly must satisfy the condition

$$\frac{\partial U}{\partial c_1} \bigg/ \frac{\partial U}{\partial c_2} = 1 + n$$

and, given the specification of the utility function, this means that output is allocated between per capita consumption of the two generations in the same way that an individual would distribute his consumption if faced with a market rate of interest equal to the rate of growth of the labor force.

Another specification of the social utility function which gives a different result is, however, admissible. If the central planning authority is constrained to make its allocation decisions on the basis of individual tastes, that is, to treat individual consumption in each generation, in a given period, in exactly the same way a representative man treats his own consumption in the two periods of his life; then the social utility function must formally differ from the individual utility function. It must take into account not only per capita consumption and time preference but also the size of the two generations. A unit of output distributed to the generation with the fewer members will have a greater effect on per capita consumption than the same output distributed to the larger generation. If no adjustment is made for this, then the social utility function will be automatically biased against individuals in the generation with more members. To prevent this bias, its specification should be such that, for the same values, its marginal rate of substitution between per capita consumption of the two generations equals the ratio of their numbers multiplied by the individual's marginal rate of substitution between consumption in the two periods of his life. For example, if there is no time preference, then the planning authority's marginal rate of substitution,

$$\frac{\partial U}{\partial c_1} / \frac{\partial U}{\partial c_2}$$

should equal L_1/L_2 , when $c_1 = c_2$, since the individual's marginal rate of substitution in the same circumstance is equal to unity.² (If p is the rate of time preference, then

$$\frac{\partial U}{\partial c_1} / \frac{\partial U}{\partial c_2} = L_1/L_2, \quad \text{when} \quad c_1 = (1 + p)c_2$$

If a social utility function of the type described is introduced into Samuelson's analysis, the result obtained is superficially the same as his. Equilibrium is achieved where

$$\frac{\partial U}{\partial c_1} / \frac{\partial U}{\partial c_2} = 1 + n.$$

But now, in the absence of time preference, this means that $c_1 = c_2$ (or, more generally, $c_1 = (1 + p)c_2$), which is Lerner's solution. The optimal allocation of per capita consumption between the two generations is the one which individuals would make between the two periods of their lives if the rate of interest facing them were zero.

A Golden Rule economy, controlled by a central planning authority, has

² In his "Reply" [6] to Lerner, Samuelson gives a numerical example which makes use of an individual utility function (with no time preference) represented by $\log c_1 + \log c_2$. An equivalent social utility function which would give the same weight to the per capita consumption of the two generations as an individual does to consumption in the two periods of his life is represented by $L_1 \log c_1 + L_2 \log c_2$.

important similarities to Samuelson's pure consumption model. This economy is the dynamic analogue of a stationary state with a zero rate of interest. Although in an economy on the Golden Rule Path the marginal productivity of capital is positive and equal to the rate of growth of the labor force [4], the transfer of a unit of output from consumption to investment does not increase the amount of consumption available to society in any subsequent period [7]. Under the rules of the game, any change in the capital-labor ratio must be maintained permanently. The net output resulting from any subsequent increase in the capital-labor ratio, once the Golden Rule level is achieved, is at least completely absorbed in maintaining the new level. For individuals on a Golden Rule Path, further abstinence from consumption does not increase society's command over consumption goods³—they are in the same position as the individuals in Samuelson's model. This similarity is recognized by Diamond, but his analysis is incomplete, as our argument above shows, in assuming that the social utility optimal intergeneration allocation is necessarily equivalent to an individual's allocation when facing a market rate of interest equal to the rate of growth of the labor force. The correct comparison may, once again, be with individual allocation when the rate of interest is zero.⁴

The Samuelson-Lerner discussion brought forth cases in which one or the other consumption configuration is clearly to be preferred. If the labor force were to grow *forever* at a constant exponential rate, then the use of the Lerner social utility function would mean that *all* individuals would be worse off than if Samuelson's social utility function were used. But if the society were to be abruptly wound up at any time, then, as Samuelson admits [6, p. 522], the Lerner configuration might be preferable. There is a meaningful choice to be made between these two social utility functions.⁵

II. Individualistic and Competitive Economy

In Diamond's competitive economy the consumption of the retired is completely determined by their investment decisions and the returns to investment. They consume all their savings as well as the interest received

³ Let k be the Golden Rule capital-labor ratio and therefore $f'(k) = n$, with $f''(k) < 0$. If this ratio is to be permanently increased, to $k + \Delta k$, then the resulting increase in output per period is not greater than $f'(k)\Delta k$, or $n\Delta k$. The extra amount of capital required to maintain the new ratio is also $n\Delta k$, therefore the increase in saving does not increase consumption if the steady state is to be maintained.

⁴ It is clear from our analysis that in a centrally planned economy with a Lerner utility function, and with no individual time preference, the working and retired generations in each period will always have, unlike the Samuelson-Diamond plan, equal per capita consumption. But, if the planning authority gives equal weight to per capita consumption in each period, this level of consumption will increase from period to period as long as the marginal productivity of capital is greater than the rate of growth of the labor force. This supposition appears to be at variance with Lerner's statement that "the Samuelson plan *would* be optimal if one could really get \$2 for \$1 by postponing consumption and reaping the rewards of a positive marginal efficiency of investment" [3, p. 525]. However, if Lerner is assumed to be referring to the individualistic economy discussed below, there is no conflict.

⁵ For use as analytical tools in the development of optimal social security policies, which both Samuelson and Lerner briefly consider, the writer believes that the Lerner approach is preferable.

from the workers for use of these savings as capital during the production part of their retirement period. The society is completely individualistic; there is no inheritance. Because of this inter-relation between consumption and investment decisions, the allocation of consumption goods in one period between per capita consumption of the two generations, in a steady state, is the same as an individual's allocation of his consumption between the two periods of his life when faced with the constant rate of interest. His allocation satisfies the condition.

$$\frac{\partial U}{\partial c_1} \bigg/ \frac{\partial U}{\partial c_2} = 1 + r.$$

He consumes $c_1 = f(k) - rk - (1+n)k$, when he is a worker, and $c_2 = (1+r)(1+n)k$, when he is retired; where k is the capital-labor ratio, $f(k)$ is the output per worker, and r , the rate of interest, equals $f'(k)$. This is also the allocation of per capita consumption between the working and retired generations in a given period in the steady state. The amount available for consumption and investment is equal to the output of that period plus the initial capital, or $L_1 f(k)$ plus $L_1 k$. Subtracting from this quantity the total consumption of workers, $L_1 [f(k) - rk - (1+n)k]$, and their total investment $L_1 (1+n)k$ we find that the total consumption of the retired is $L_1 k(1+r)$, or $(1+n)(1+r)k$ per capita.

If this individualistic economy should not only be in a steady state, but also happens to be on the Golden Rule Path, then r equals n . There is here no separate problem of determining how to distribute the maximum amount of consumption goods between the two generations. The competitive mechanism ensures that the intergeneration allocation of consumption is identical to the individual's allocation of consumption between the two periods of his life when faced with the biological rate of interest. The Samuelson-Diamond configuration will prevail, without any qualification, only in this very special case.⁶

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⁶ There is no reason to expect that a competitive economy will necessarily reach a Golden Rule Path [1, p. 1135].

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Time Deposits in the Definition of Money

The original study by Milton Friedman and David Meiselman [3]¹ on the competitive abilities of a stock of money and autonomous expenditures to predict money income in the United States has been subjected recently to an intensive reappraisal and review [1] [2] [4]. The essence of the arguments seems to be that the accuracy of autonomous expenditures in predicting money income or consumption depends critically on the definition of autonomous expenditures chosen as a predictor. To a lesser extent this same issue can be raised about the various possible inclusions made in constructing "the" quantity of money used for testing purposes. Essentially, the question boils down to the "moneyish" influence time deposits exert on the "narrow" stock of money (currency and demand deposits).

The analysis presented below purports to develop a pragmatic answer to this question by allowing annual first differences in three diverse stocks of money to compete at predicting annual changes in money income. In order to check the results of the simple correlations, annual first differences in the narrow stock of money and annual first differences in all time deposits are then structured in the form of a multiple correlation to estimate annual changes in money income. The multiple correlation analysis also tests the data for a significant coefficient of moneyiness in time deposits.

The raw data for currency, demand deposits, and time deposits may be used to construct an infinite number of money stocks. Each stock would include the first two of these items plus some percentage (weight) of all time deposits. F-M chose to include in their money stock the percentage of time deposits in commercial banks. Both expediency and logic recommended this choice. It is expedient because reliable estimates for this definitional stock of money can be obtained for a much longer time period than can be obtained for other money stocks;² and it is logical because of the distinct possibility that time deposits and demand deposits may be held as close substitutes for each other when they are claims against the same commercial bank. However, the case can also be made that the narrow stock of money is the only one that can be used for transactions and is, therefore, the only stock that influences spending.³ Yet another and contrary view sees *all* time deposits generating liquidity in the monetary system, and thus making more efficacious the spendability of the

¹ Hereafter, references to the work of these authors is abbreviated to "F-M".

² Reliable data for *all* deposits in commercial banks are available back to 1875, but accurate breakdowns of these data into time and demand components are available only from 1914.

³ This argument is tantamount to assigning irrelevant monetary influence to financial assets that are not transactive.

narrow money stock. Each of these concepts is logical, and each has intuitive plausibility. The choice, however, must be made in terms of the empirical relevance shown by the various stocks in predicting money spending.⁴

The groundwork for this study required time series data of the three most relevant stocks of money: the narrow stock, M_1 , the F-M stock, M_2 and the narrow stock plus all time deposits in commercial and savings banks, M_3 . Data for money income from the spending side, Y , were taken from the original work by F-M and supplemented to 1965 using their definition. Annual first differences in the various money stocks were then correlated with annual first differences in money income for the reference cycle periods defined in the original F-M study.⁵ The results of this series of tests are summarized in the first four columns of Table I.

The correlation values shown here make possible some interesting inferences that are obscured when only the values for longer periods are computed. First, the correlation coefficients for all the money stocks in most of the subperiods covered show extremely high degrees of association between changes in money and changes in income. Second, while the F-M money stock, M_2 , has the highest correlation value over the entire period, the narrow money stock, M_1 , has higher values in more of the years than either M_2 or M_3 . This seeming anomaly results from the inclusion of data from the war years (1942-46) in the tests, and the much poorer performance of M_1 in that period. Third, time deposits improve the correlation values only in the 1933-38 period.

The first inference—that any of the stocks of money influences spending—needs no interpretation beyond that given by F-M. Second, the irregular values for the periods embracing the war years obviously result from biases in the raw data and confirm Donald Hester's observation on the original F-M study, viz: "Indeed it is remarkable that the monetary model failed to reflect these conditions [in the 1942-46 period] more vividly" [6, p. 367].⁶ The raw data associations for the various money stocks and money income do not reflect wartime discrepancies because of the dominance of trend in the series. First difference correlations do emphasize the warpings of normal spending relationships by abstracting trend. Price controls, rationing, much higher taxes, and exhortations not to spend, dammed up money in peoples' pockets or caused "under the table" spending, the effects of which could not be measured. This alteration continued to some degree through most of the Korean War. The 1953-65 correlation values indicate a gradual return to more normal relationships.

Third, the better performance of M_3 during 1933-38 can be attributed to two "real" factors also not measurable cardinally. First, very low interest

⁴ This methodological postulate is the one adopted by F-M and not seriously challenged by anyone so far.

⁵ First difference correlations for the whole period are mentioned in the original study and are computed in a later investigation by one of the authors of this paper (Timberlake) [7], but only for the whole period (1897-1960) and the two half-periods (1897-1929) and (1930-1960).

⁶ F-M's effort to include *all* data in their original study is commendable, but surely they leaned too far when they included monetary and income data for 1942-46.

TABLE 1—SIMPLE CORRELATIONS OF FIRST DIFFERENCES IN THREE CONCEPTUAL STOCKS OF MONEY ON FIRST DIFFERENCES IN NOMINAL MONEY INCOME, THE PER CENT OF ALL TIME AND SAVINGS DEPOSITS IN COMMERCIAL BANKS, AND COEFFICIENTS FOR A MULTIPLE REGRESSION OF FIRST DIFFERENCES IN NARROW MONEY AND TIME DEPOSITS ON FIRST DIFFERENCES IN NOMINAL MONEY INCOME

| Period Annually | r_{Y-M_1} (Narrow) | r_{Y-M^2} ($F-M$) | r_{Y-M_3} (All Time) | Per Cent All Time Deposits in Commercial Banks (Average for period) | b_1 | b_2 | b_2/b_1 | R_{Y-M_1} | $R_{Y-M, T}$ |
|--------------------|-------------------------|--------------------------|---------------------------|---|-------|---------|-----------|---------------------|--------------|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| 1897-1908 | b | .890 | .820 | b | b | b | b | b | b |
| 1903-1913 | b | .788 | .813 | b | b | b | b | b | b |
| 1908-1921 | b | .766 | .726 | b | b | b | b | b | b |
| 1913-1920 | .796 | .786 | .727 | 63 | 3.086 | -1.131 | — .3664 | .796 | .803 |
| 1920-1929 | .775 | .700 | .702 | 70 | 4.939 | -1.035 | — .2096 | .775 | .779 |
| 1921-1933 | .883 | .801 | .772 | 68 | 6.922 | -1.128 | — .1630 | .883 | .894 |
| 1929-1939 | .891 | .882 | .865 | 58 | 3.170 | .4467 | .1409 | .891 | .893 |
| 1933-1938 | .785 | .766 | .865 | 53 | 1.224 | 9.791 | 7.997 | .902 ^a | .987 |
| 1938-1953 | .028 | .006 | — .145 | 63 | .3812 | -1.620 | -4.249 | — .419 ^a | .471 |
| 1939-1948 | — .019 | — .009 | — .171 | 63 | .3681 | -1.660 | -4.510 | — .410 ^a | .458 |
| 1948-1960 | .495 | .408 | .285 | 64 | 1.162 | — .4902 | — .3032 | .496 | .514 |
| 1953-1965 | .667 | .609 | .633 | 68 | 1.919 | .2903 | .1521 | .667 | .692 |
| 1929-1960 | .398 | .501 | .427 | 62 | .8945 | -1.146 | -1.281 | .401 ^a | .504 |
| 1897-1960 | b | .573 | .517 | 54 | 1.004 | .7277 | .7251 | b | .518 |

Sources: Original data for M_1 and T to 1957 were taken from: U. S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957*, Washington, D. C., 1960, Series X, p. 646. Data for M_2 and Y to 1957 were taken from [3, pp. 259-60, Table II-B]. Data for the period 1953-1965 were obtained from current issues of the *Survey of Current Business* and the *Federal Reserve Bulletin*. Percentages in Column (5) were computed by the authors.

^a Most of these values correspond to ones given by $F-M$ in [5, p. 375, Table I].

^b Separate estimates for demand and time deposits in commercial banks before 1913 are not reliable.

^c These values are for the correlation coefficient of *time* deposits on income. The computer kicks out only the higher of the two values in a multiple correlation (without regard to sign). To compare the simple coefficient of the narrow stock of money on income, see Column (2).

rates on time deposits discouraged their attractiveness as investments and encouraged their use as quasi-transaction balances. Probably more important, however, was the influence of the bank debacle in the early 1930s on deposit holdings. Demand deposits lost some of their moneyness due to the additional risk imputed to them by depositors after the monetary blood-letting of 1932-33. Time deposits seemed less risky to both banks and their depositors, so the deduction reasonably can be made that moneyness was lost by demand deposits and gained by time deposits.

To test these conclusions further, and also to test for a significant monetary coefficient of time deposits, a multiple regression analysis was conceived using first differences in the narrow stock of money, M_1 , and in all time deposits, T , to predict changes in money income, Y .⁷ The form of the testing equation is:

$$(1) \quad \Delta Y = a + b_1 \Delta M_1 + b_2 \Delta T,$$

or

$$(2) \quad \Delta Y = a + b_1 [\Delta M_1 + b_2/b_1 \Delta T].$$

If time deposits have some degree of moneyness, the ratio (b_2/b_1) should be greater than zero but less than one. A value of one for this fraction would imply that time deposits had moneyness equal in degree to the items in the narrow stock of money. A negative value for (b_2/b_1) implies that time deposits serve more in the nature of investments: that people actively reduce their transactions balances to "buy" time deposits.

As can be seen from Table I, the fraction (b_2/b_1) is between -1 and 0 except for the period 1933-38. In this period the ratio jumps up to 7.997 ! Furthermore, bringing time deposits into the picture as a multiple correlate to the narrow stock of money does not add significantly to the simple correlation coefficient in any of the periods except 1933-38. [See Columns (9) and (10).] But in this period first differences in time deposits are better predictors of changes in money income than are changes in the narrow money stock, and the multiple correlation coefficient becomes a whopping $.987$! Such dramatic results confirm the supposition that time deposits gained appreciably in moneyness due to the depreciation in confidence people had in demand deposits.

The value for (b_2/b_1) then becomes large and negative for the periods that include the war years, emphasizing the efforts of people to keep purchasing power they were enjoined from spending in a form that obtained some return. Only in the last 12 years does the ratio of (b_2/b_1) become a positive fraction of a magnitude that would give some credence to the theory of moneyness in time deposits, and even in this case the additional predictability gained from including time deposits in the analysis is insignificant ($.667$ to $.692$).

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⁷ We are indebted to Milton Friedman for suggesting this method.

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Surplus Agricultural Labor and Development— Facts and Theories: Comment

In a recent communication [1] Morton Paglin presented data on Indian agriculture in an attempt

... to show that large-scale opportunities for additional employment exist within agriculture, and that the output of the current labor force could be increased by redistribution of labor within the sector. Chronic or disguised unemployment of family labor, presumably because of small landholdings, will also be called into question as a generalization applicable to farms in the smaller size classes. Finally, the common assumption that economic development will produce a large exodus of labor from agriculture is shown to be in need of drastic revision [1, p. 816].

From his interpretation of these data he concluded that they

... throw doubt on the validity of the dual economy models which assume a free transfer of labor from agriculture to industry without loss of agricultural product. The evidence also negates those formulas which suggest a maximization of output via the maximization of the average productivity of capital. ...

Finally, on the basic issue of the zero marginal product of labor and disguised unemployment, the evidence offered here supports the position taken by Viner, Schultz, and Oshima, against the more generally accepted views of ... many others that Indian agriculture suffers from a redundant labor force with a zero marginal value product. The data indicate not only a positive marginal product of labor, but also lend support to the hypothesis that the rationalization and improvement of agri-

cultural techniques, generated by development efforts, will exert a strong upward pull on the demand for agricultural labor [1, pp. 831-32].

The following comments show that the data used by Paglin in his article, insofar as they are relevant to the above subjects, clearly support the opposite conclusions.

I. Paglin's Data and the Return to Capital

In Columns 2-6 of Table 1 are presented the relevant data on inputs and outputs of Indian agriculture from Paglin's Tables 1, 3, and 4. Labor input in rupees is "all cash outlays for hired labor" and "family labor of men, women, and children is given an imputed value equivalent to the market wage appropriate to each category." Other Type A costs are all nonlabor "cash outlays for . . . seed, etc., except for rent paid by tenant farmers" plus "depreciation of equipment" or, alternatively, "working capital, depreciation of fixed capital, . . . , etc." Rent in Table 1 is the difference between Paglin's Type A and Type C inputs, which is "rent, paid and imputed." Gross output is the "value of agricultural output (in rupees)." Profit—the difference between gross output and total costs—is identified by Paglin as "entrepreneurial profit."

These are the only definitions of the concepts which were given by Paglin. In order to evaluate carefully the data presented, one would need a substantial amount of additional information on (1) the basis for computing imputed labor costs, (2) the specific items which were included in Other Type A costs, (3) the basis for computing depreciation, (4) the percentage of Other Type A costs accounted for by depreciation, and (5) the extent to which rent is a payment for the services of capital and the extent to which it is payment for the use of land. In this comment we assume that sufficient information on (1) and (2) would lead us to use Paglin's data. The information for (3) and (4) is needed to obtain an estimate of the quantity of capital per acre for the various size groups; we will have to proceed without this information.

However, it is impossible to avoid a discussion of Paglin's data on rent. Paglin asserts that systematic differences in fertility among size groups were unlikely [1, pp. 816-18]. If his rent figures reflected differences in fertility and location, then he is correct in noting that this would support the surplus labor hypothesis. On the other hand, if differences among size groups do not reflect differences in land quality, then they reflect either differences in bargaining power or differences in "rent" on improvements. Paglin does not suggest the former, and since some rents are imputed this explanation would be difficult to accept. Thus, deductively we arrive at the conclusion that differences in rent largely reflect differences in returns to capital improvements. This interpretation of the statistics is also borne out by the high correlation of Type C inputs per acre with gross output per acre—especially in view of the curiously unresponsive reported rent data for Madhya Pradesh. Paglin reports a correlation coefficient of .94 for the log of Type A inputs with gross output, and .91 for the log of Type C inputs with gross output. One suspects that if the effects

TABLE 1—AVERAGE VALUE OF INPUTS AND OUTPUT PER ACRE
(Values in Rupees per Acre)

| Size of Farm (acres) | Type C Costs | | | Total Costs | Gross Output | Profit |
|-------------------------|--------------|-------|------|----------------|-----------------|--------|
| | Type A Costs | | Rent | | | |
| | Labor | Other | | | | |
| Madhya Pradesh | | | | | | |
| 0-5 | 32.3 | 37.6 | 30.0 | 99.9 | 143.3 | 43.4 |
| 5-10 | 27.6 | 30.2 | 30.0 | 87.8 | 115.1 | 27.3 |
| 10-15 | 25.7 | 30.6 | 30.0 | 86.3 | 118.9 | 32.6 |
| 15-20 | 23.7 | 22.8 | 30.0 | 76.5 | 98.5 | 22.0 |
| 20-30 | 21.6 | 20.8 | 30.0 | 72.4 | 107.9 | 35.5 |
| 30-40 | 22.7 | 22.4 | 30.0 | 75.1 | 106.1 | 31.0 |
| 40-50 | 26.7 | 25.3 | 30.0 | 82.0 | 98.7 | 16.7 |
| 50 and over | 22.7 | 25.1 | 30.0 | 77.8 | 107.3 | 29.5 |
| Madras | | | | | | |
| 0-2.5 | 69.4 | 148.5 | 73.9 | 291.8 | 219.9 | -71.9 |
| 2.5-5.0 | 32.8 | 86.8 | 55.2 | 174.8 | 205.2 | 30.4 |
| 5.0-7.5 | 26.1 | 69.2 | 45.1 | 140.4 | 157.4 | 17.0 |
| 7.5-10 | 22.0 | 68.4 | 47.5 | 137.9 | 134.8 | - 3.1 |
| 10-15 | 15.4 | 46.6 | 31.2 | 93.2 | 76.8 | -16.4 |
| 15-20 | 12.9 | 34.2 | 25.8 | 72.9 | 67.7 | - 5.2 |
| 20-25 | 15.3 | 34.7 | 24.4 | 74.4 | 73.3 | - 1.1 |
| 25 & over | 11.4 | 27.9 | 21.7 | 61.0 | 82.4 | 21.4 |
| Punjab | | | | | | |
| 0-5 | 53.1 | 73.8 | 92.5 | 219.4 | 176.5 | -42.9 |
| 5-10 | 51.2 | 56.4 | 73.7 | 181.3 | 170.5 | -10.8 |
| 10-20 | 47.0 | 61.5 | 70.1 | 178.6 | 169.8 | - 8.8 |
| 20-50 | 34.2 | 43.4 | 60.4 | 138.0 | 142.6 | 4.6 |
| 50 & over | 35.1 | 39.1 | 56.9 | 131.1 | 159.6 | 28.5 |
| Uttar Pradesh | | | | | | |
| 0-5 | 64 | 177.8 | 17.2 | 259 | 277 | 18 |
| 5-10 | 56 | 136.2 | 16.8 | 209 | 240 | 31 |
| 10-15 | 53 | 115.6 | 16.4 | 185 | 204 | 19 |
| 15-20 | 48 | 109.6 | 15.4 | 173 | 200 | 27 |
| 20 & over | 39 | 91.7 | 14.3 | 145 | 205 | 60 |
| West Bengal | | | | | | |
| 0.01-1.25 | 97.7 | 30.9 | 57.9 | 186.5 | 202.1 | 15.6 |
| 1.26-2.50 | 80.7 | 30.2 | 48.3 | 159.2 | 194.8 | 35.6 |
| 2.51-3.75 | 88.8 | 36.4 | 52.9 | 178.1 | 175.9 | - 2.2 |
| 3.76-5.00 | 73.4 | 29.8 | 43.7 | 146.9 | 181.0 | 34.1 |
| 5.01-7.50 | 74.2 | 27.1 | 51.5 | 152.8 | 186.9 | 34.1 |
| 7.51-10.00 | 61.2 | 18.3 | 56.7 | 136.2 | 172.0 | 35.8 |
| 10.01-15.00 | 51.2 | 21.9 | 25.1 | 98.2 | 149.1 | 50.9 |
| 15.01 & over | 60.0 | 36.2 | 20.1 | 116.3 | 141.9 | 25.6 |

Source: [1, pp. 817, 822-23].

of the Madhya Pradesh rent data had been eliminated from the latter regression, it would have explained more than the former.

One feels relatively safe, then, in assuming either that fertility and location improved as the size of farm declined in a region or that returns to differences in capital improvements largely accounted for differences in rent among different size groups in a region. The importance of these assumptions becomes apparent in Section III below.

II. *Optimum Factor Combinations*

Paglin never explicitly shows profits per acre (Column 7, Table 1), but several times he implies that his data show a direct relationship between labor inputs per acre and profits per acre. For instance he says "... the present data ... show clearly that hired labor adds considerably to output, and the employer hires this labor not because of social pressure but because it is profitable." He also refers the reader to "low-aspiration" models to explain "poor utilization" of larger holdings [1, pp. 824, 828].

He does not note that profits per acre were highest for the size group using the *least* labor per acre in West Bengal and Uttar Pradesh, and that in Punjab the largest size group had highest per acre profits and a labor per acre utilization virtually the same as the lowest. In Madhya Pradesh and Madras the size groups with lowest labor inputs per acre were the second most profitable per acre; in Madhya Pradesh the size group with highest labor inputs per acre was the most profitable per acre, and in Madras the size group with the second highest labor inputs per acre showed the highest profits per acre. Thus, it is seen from Table 1 that, of the twenty-nine factor combinations alternative to the *least* (or virtually the least) labor per acre combinations of their region, only two would increase profit per acre. Let us analyze these two exceptional cases more closely.

The systematic and large variations in rent per acre in all other states make one reluctant to accept the reported rent figures of a constant 30.0 rupees per acre for Madhya Pradesh. If, in fact, rent was a modest 23 per cent higher for the smallest size group than for the least labor-using size group, then the latter would be the most profitable per acre in Madhya Pradesh.

Thus, only in Madras is it clearly true that the *least* labor-using size group was not the *most* profitable *per acre*. But profit per acre alone has little relevance for the decision on choice of factor combinations. Of much greater relevance is profit per unit of capital invested, and Paglin shows no figures on capital invested nor does he discuss the opportunity cost of using capital in agriculture. In fact Paglin seems to suggest that credit availability theoretically would be the only factor limiting the application of additional capital per acre in India. He does at that point mention the supply of capital goods (irrigation field channels), but he dismisses this limit on the grounds that its production is labor intensive. However, the opportunity cost of investment, even if it only involves payments to labor, certainly should be considered by the private Indian farmer.

By making two assumptions we can estimate crudely the additional capital required in Madras to change from the least labor-using technique to the most-profit-per-acre technique. Assume that the labor-intensive technique is double cropping and that the alternative is not, so that one-half of additional Other Type A inputs (seed, equipment, depreciation, etc.,) represents a conservative estimate of additional working capital requirements. Further assume conservatively that differences in rent input represent a 25 per cent return on improvements. (If we assume a smaller percentage return, the capitalized earning value of the improvements would be even larger.) On the basis of these assumptions a shift to the labor-intensive technique in Madras would require an investment of 29.4 rupees $[\frac{1}{2}(86.8-27.9)]$ of working capital and 134.0 rupees $[(55.2-21.7)/.25]$ of improvements—a total additional investment of 163.4 rupees per acre to increase profit and rent by 42.5 rupees per acre. This gives a 26 per cent return on the additional investment. The return falls below the assumed opportunity cost (25 per cent) of capital if working capital requirements are underestimated by more than 6.6 rupees per acre or if equipment costs (which are assumed to be the same) are more than 6.6 rupees greater for the labor-intensive technique. If the opportunity cost of capital is assumed to be only 10 per cent, then the above calculations yield an increased necessary investment of 364.4 rupees per acre and an 11.7 per cent return on that investment—which would drop below 10 per cent if working capital requirements were underestimated by more than 60.6 rupees or if additional equipment requirements were more than 60.6 rupees. These calculations show that consideration of even a modest degree of uncertainty would lead us to reject the hypothesis that owners of large farms even in Madras were not profit maximizers.

Paglin's data, then, lead us to conclude that it is misleading for him to say of Indians that

Since the farmer with a relatively large holding can eke out a moderate income without the trouble of hiring a high per cent of nonfamily labor, or the risk of borrowing additional working capital for other inputs associated with intensive cultivation, he frequently seems to prefer the low-effort, low-risk, low-output package to the higher-risk, higher-profit, higher-output combination [1, pp. 827-28].

and to say *on the basis of his data* that

... limited incentives in many areas of Indian agriculture, rather than a critical man-land ratio, may be the more important bottleneck to increased production and employment. However, insofar as population growth leads to smaller average holdings through a breakup of the larger holdings, and to the extent that current government land policies produce a more equitable distribution of land, the bottleneck is relaxed and a safety valve, effective at least in the short run, is brought into play [1, p. 828].

Undoubtedly there are cases of underutilization of labor on land in India, and we agree with Paglin that output could be increased if this were not the case, but his data indicate that these cases are exceptional rather than general.

III. *The Marginal Value Product of Labor*

None of Paglin's data bear directly on the question of the marginal value product of labor in Indian agriculture. With some assumptions we can use his data to cast some indirect light on this question. In Table 2 (Column 5) we present data on the change in gross output per acre less nonlabor inputs per acre which would accompany a shift in each region from the least labor-using technique (except for Punjab where we use the second least labor-using technique) to each of the alternative techniques. Ideally to measure the marginal value product of labor we would wish to vary the labor costs as all other costs were constant. It is usual to permit some obviously complementary inputs such as seed costs to vary with labor, but to measure revenues net of these costs; Paglin does not follow this procedure. We assume that rent differentials within each district measure differences in "rent" on improvements and/or differences in returns resulting from differences in fertility and location between size groups. By deducting differences in rent from gross output per acre (from Table 1) we are eliminating to some extent variations in revenues resulting from differences in improvements, fertility, and location; but we are unable to eliminate variations in output due to differences in working capital and equipment requirements. For comparison, changes in labor costs are also shown (in Column 4) for these alternative techniques.

From these data it is seen that the "marginal value product" of labor was significantly negative for all alternatives to the *least* labor-using technique in Punjab and Uttar Pradesh. The rent data for Madhya Pradesh discussed earlier make us suspect that the same was true for that state as well. For West Bengal four of the seven alternative techniques had "marginal value products" of labor which were positive, but in no case was that "marginal value product" as much as one-half of the "marginal cost" of labor. Thus, only in Madras do we find "marginal value products" of labor high enough to cast doubt on the assumption of zero marginal value product of labor—and in Madras only for two of the seven alternative techniques. And if it is true, as seems likely, that working capital and equipment requirements increased significantly as labor, seed, and other current costs increased significantly, then these data understate significantly the extent to which the "marginal value product" of labor was negative generally in India.

IV. *Rationalization and the Future Course of Indian Agriculture*

Paglin was successful in his attempt to show "that the output of the current labor force could be increased by redistribution of labor within the sector [“1, p. 816]. His data do not show that an increase in the quantity of agricultural labor would result in an increase in aggregate agricultural net product, unless we assume that capital is a free good. Let us use "rationalization" to mean redistribution of laborers among the acres of land in these five regions in such a manner that the aggregate net product is at least as great as at present, *aggregate capital stock is no greater than at present*, and aggregate labor input to obtain that output is minimized. The output which is socially significant is Paglin's gross output less Other Type A costs, since only this net output can add

to the welfare of an Indian. The data for this net output per acre are presented in Column 6 of Table 2.

For Punjab and Uttar Pradesh we see immediately that by using all the acres as the largest size groups were used, we could use a substantially smaller quantity of labor and obtain a substantially larger quantity of net output. With data on the aggregate number of acres in each size group we could calculate the quantity of labor cost released by operating the entire region as the largest size groups were operated; with data on labor cost per man-hour of labor we could estimate the number of man-hours so released. On the formerly small-size farms extra working capital, equipment, and improvements per acre and per worker exist after the rationalization, but we are not permitted to use them in Punjab and Uttar Pradesh *and depreciate them*, since their depreciation was one of the Other Type A costs which made the marginal net product of labor-capital negative. Temporarily we can substitute the improvements for some of our remaining labor until they must be replaced, but we will permit the released workers to take their proportion of the equipment and working capital with them as they move to other regions.

Next we must decide to which region these surplus workers should be moved. They will have the equipment and working capital which they used in their native regions, but they cannot take with them the improvements with which they labored. The rent data in Table 1 for Madhya Pradesh, Madras, Punjab, and Uttar Pradesh indicate that improvements per acre were greater for all other production techniques than they were for the least labor-using technique; hence, from these data we get no clue concerning what happens to the marginal product of labor-capital as the improvements-labor ratio declines. From the data for West Bengal we do get such a clue when we analyze data for the three largest size groups. For the "7.51-10 acres" and the "15 acres and over" groups, labor costs were approximately 10 rupees per acre greater than for the "10.01-15 acres" group; Other Type A costs increased with size, and rent decreased significantly as size increased. Net output per acre decreased significantly as size increased; hence, as the improvements-labor (or fertility-labor) ratio declined, even with the equipment-labor ratio rising, net output per acre and per worker declined. Thus, we conclude that the "marginal value product" of this labor released from Punjab and Uttar Pradesh will in all probability be lower than that indicated for the other regions by Paglin's data. And as we noted in Section III, only in Madras was that "marginal value product" clearly positive.

The significant point of the analysis is that, while labor can be removed from Punjab and Uttar Pradesh without loss of output, *all of this labor* cannot be added in the other regions without reducing the capital-labor ratio and, thus, reducing the marginal product of labor. This is because improvements cannot be transferred with the labor and, without violating the requirement of a constant quantity of aggregate capital in agriculture, only the depreciation on the improvements with which the transferred laborers formerly worked can be used to accumulate improvements in their new location. Thus Paglin's data indicate that by moving workers out of agriculture net agricultural output

TABLE 2—SELECTED DATA ON LABOR PRODUCTIVITY
(Values in Rupees per Acre)

| Size of Farm (acres) | Labor Costs | Adjusted Net Revenues ^a | Change in Labor Costs | Change in Adj. Net Revenues | Net Revenues |
|-------------------------|----------------|--|-----------------------------|-----------------------------------|-----------------|
| Madhya Pradesh | | | | | |
| 0-5 | 32.3 | 105.7 | 10.7 | 18.6 | 105.7 |
| 5-10 | 27.6 | 84.9 | 6.0 | - 2.2 | 84.9 |
| 10-15 | 25.7 | 88.3 | 4.1 | 1.2 | 88.3 |
| 15-20 | 23.7 | 75.7 | 2.1 | -11.4 | 75.7 |
| 20-30 | 21.6 | 87.1 | — | — | 87.1 |
| 30-40 | 22.7 | 83.7 | 1.1 | - 3.4 | 83.7 |
| 40-50 | 26.7 | 73.4 | 5.1 | -13.7 | 73.4 |
| 50 & over | 22.7 | 82.2 | 1.1 | - 4.3 | 82.2 |
| Madras | | | | | |
| 0-2.5 | 69.4 | 19.2 | 58.0 | -35.3 | 71.4 |
| 2.5-5.0 | 32.8 | 84.9 | 21.4 | 30.4 | 118.4 |
| 5.0-7.5 | 26.1 | 64.8 | 14.7 | 10.3 | 88.2 |
| 7.5-10 | 22.0 | 40.6 | 10.6 | -13.9 | 66.4 |
| 10-15 | 15.4 | 20.7 | 4.0 | -33.8 | 30.2 |
| 15-20 | 12.9 | 29.4 | 1.5 | -25.1 | 33.5 |
| 20-25 | 15.3 | 35.9 | 3.9 | -18.6 | 38.6 |
| 25 & over | 11.4 | 54.5 | — | — | 54.5 |
| Punjab | | | | | |
| 0-5 | 53.1 | 67.1 | 18.0 | -53.4 | 102.7 |
| 5-10 | 51.2 | 97.3 | 16.1 | -23.2 | 114.1 |
| 10-20 | 47.0 | 95.1 | 11.9 | -25.4 | 108.3 |
| 20-50 | 34.2 | 95.7 | -0.9 | -24.8 | 99.2 |
| 50 & over | 35.1 | 120.5 | — | — | 120.5 |
| Uttar Pradesh | | | | | |
| 0-5 | 64 | 96.3 | 25 | -17 | 99.2 |
| 5-10 | 56 | 101.3 | 17 | -12 | 103.8 |
| 10-15 | 53 | 86.3 | 14 | -27 | 88.4 |
| 15-20 | 48 | 89.3 | 9 | -24 | 90.4 |
| 20 & over | 39 | 113.3 | — | — | 113.3 |
| West Bengal | | | | | |
| 0.01-1.25 | 97.7 | 138.4 | 46.5 | 11.2 | 171.2 |
| 1.26-2.50 | 80.7 | 141.4 | 29.5 | 14.2 | 164.6 |
| 2.51-3.75 | 88.8 | 111.7 | 37.6 | -15.5 | 139.5 |
| 3.76-5.00 | 73.4 | 132.6 | 22.2 | 5.4 | 151.2 |
| 5.01-7.50 | 74.2 | 133.4 | 23.0 | 6.2 | 159.8 |
| 7.51-10.00 | 61.2 | 122.1 | 10.0 | - 5.1 | 153.7 |
| 10.01-15.00 | 51.2 | 127.2 | — | — | 127.2 |
| 15.01- & over | 60.0 | 110.7 | 8.8 | -16.5 | 105.7 |

Source: Table 1.

^a Gross Output from Table 1 less Other Type A Costs and less differences in Rent from that of the least labor-using technique (except in Punjab where the second least labor-using technique is used).

would increase, but they do not indicate that increasing the number of workers in agriculture can increase net output, unless capital is assumed to be costless.

We agree with Paglin that there probably is no significant limit to the absolute quantity of output per acre of Indian land if additional labor *and capital* are applied. However, we find it impossible to follow him in assuming that in India capital should be regarded as a free good. Capital is scarce in India, as it is in the United States; for this reason the opportunity cost of using it in agriculture must be considered in private and public planning. From Paglin's data it appears that generally in India the farmers in the larger size groups have considered this opportunity cost of capital to a greater extent than farmers in the smaller size groups. One hopes that India's public planners will continue to concentrate on changing techniques in agriculture, to consider the opportunity cost of investing in agriculture, and to analyze carefully their accumulating body of facts.

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REFERENCE

1. MORTON PAGLIN, "Surplus Agricultural Labor and Development: Facts and Theories," *Am. Econ. Rev.*, Sept. 1965, 55, 815-34.

Reply

Dr. Bennett raises some important issues but his attempted resolution of the problem reveals the inadequacy of a mechanical application of the theory of the firm to peasant agriculture. Apparently having little knowledge of Indian agriculture, Bennett fails to see which assumptions underlying the market economy model of the firm have to be modified for the average Indian farmer who purchases only a small percentage of his inputs, whose capital stock and land improvements represent in large part accumulated family labor in the classical sense, and whose objective is not just to maximize entrepreneurial profit but to maximize farm business income (the total return to all factors owned by the farm family) and yet keep the family functioning as a unit. This latter constraint means that the imputed wage rates for women and children¹ applied to part of the family labor inputs, as well as imputed rent and interest used in the input figures, are not always viable economic alternatives for the Indian farmer. From the social point of view it is also questionable whether the entire family labor force used on the farm could be used outside agriculture. Legally child labor is prohibited in industry and the part-time agricultural labor performed by women with families may have little use in the industrial sector. Thus the opportunity cost of a considerable portion of the labor inputs used on the family farm may be near zero.

¹ In the Farm Management data they were valued at $\frac{3}{4}$ and $\frac{1}{2}$ of the male wage-rate.

If the farmer were to rent his land at the imputed figure, the family labor income would be both precarious and probably smaller. Operation of the farm provides guaranteed employment for the family labor force and the opportunity to transform some of this labor in off-peak periods into capital through the digging of irrigation channels, the building of farm structures and fences, the raising of work animals, etc. There is probably an underestimation of the output in the subsistence sector by not taking into account the value of the capital formation. This phenomenon of direct transformation of labor into capital explains how seemingly overpopulated agricultural areas have been able to absorb and support additional workers by the tens of millions. When Bennett speaks of scarce capital yielding a better return outside agriculture, he doesn't realize that the family labor transformed into agricultural capital cannot alternatively be transformed into industrial capital without scarce foreign exchange and other capital being used in the process.

Bennett's first technical error lies in his assumption that the imputed rent figures measure intrinsic soil fertility or permanent improvements on the land which are independent of the level of inputs used: "We assume that rent differentials within each district measure . . . differences in returns resulting from differences in fertility and location between size groups. By deducting differences in rent from gross output per acre we are eliminating to some extent variations in revenue resulting from differences in improvements, fertility, and location." Let us review the evidence on rent insofar as it bears on this problem.

If we assume that the farms randomly selected in each sample district show no correlation between intrinsic fertility and size, then differences in the marginal productivity of land would still appear if the larger farms using hired labor were applying inputs much less intensively than the small farms using mainly family labor, part of which had a lower opportunity cost. The marginal product of land would increase as inputs of labor per acre went up and the marginal product of labor declined. I previously gave evidence that Indian farms in fact correspond to the above hypothesis. The field work of such careful observers as J. W. Mellor and T. V. Moorti, who analyzed sample farms in Agra, Uttar Pradesh confirms the judgment that the land of the larger, wealthier farmers is not intrinsically inferior to the land of the smaller farmers but that frequently the labor inputs are "too thinly applied" on the large holdings—cited in [6, pp. 825, 827n]. Differences in rent would be the *result* of varying inputs per acre and not an independent causal factor in explaining output variations as Bennett assumes. It is for this reason that the inclusion of imputed rents in the input data "C" *reduced* the percentage of explained variance in output as compared to results obtained when rent was eliminated from the inputs ("A" data) [6, p. 819]. If this hypothesis on rent is true even in part, then we cannot assume that small farms reorganized and cultivated with the lower inputs now prevailing on the large farms would earn the same rent differentials as before. Yet this is exactly what Bennett postulates in his Table 2 ("adjusted net revenue") and the discussion connected with it.

The survey workers in attempting to estimate the rental value of each farm

(typically made up of several scattered fragments) had little in the way of market guidelines. India, especially since the Zemindari laws, is a nation of peasant proprietors, and 80 to 90 per cent of the land is owned by the cultivators. Of those who do rent land, only a tiny fraction pay rent on a fixed cash basis. Rather, the wide variety of arrangements commonly used make the landlord a participating entrepreneur who sometimes contributes seed, etc. and who gets a certain percentage of a highly variable pie. Even the rented land provides no adequate sub-sample for imputing specific rent figures by size of farm. Faced with the requirement of imputing rent to each farm, the survey workers in many cases used the land tax as an indicator of rental value. Unfortunately, land assessments in India are frequently 30 and 40 years old, and there is the well-established tendency to underassess larger properties. There is also every reason to believe that the Indian tax assessor was not capable of soil analysis but, if anything, based his notion of land value on output per acre which would reflect the higher marginal productivity of land due to higher labor inputs on small farms.

I do not think there is any strong reason for believing that in each fairly homogeneous sample area the larger farms would be systematically inferior in soil quality to the smaller ones, especially since small and large holdings are thoroughly fragmented and interspersed.² In any case, natural fertility is now rightly considered of less importance than it once was. E. Boserup in her extensive report of agricultural practices in underdeveloped areas has suggested that fertility in the classical sense as a fixed quality of land should be replaced by the concepts of frequency of cropping, long-fallow, etc. [1, p. 13]:

Traditional economic theory . . . regarded as immutable natural conditions many features [of land] which scientists now consider to be man-made and, in particular, the distinction between naturally fertile land and less fertile land was considered a crucial element in the explanation of agricultural change. . . . By contrast, when the analysis is based upon the concept of frequency of cropping, there can be no temptation to regard soil fertility exclusively as a gift of nature, bestowed upon certain lands once and for all. Thus, soil fertility, instead of being treated as an exogenous or even unchangeable initial condition of analysis, takes its place as a variable, closely associated with changes in population density and related changes in agricultural methods.

She then develops the thesis that increases in agricultural density induce a shift from a low input, long-fallow technique, to more intensive methods involving short-fallow, multiple cropping, and more labor-intensive crops of higher value. Also particularly relevant to our discussion here are the historical and current examples (including India) in which reductions of agricultural population density have led to a deterioration of agricultural practices, a ne-

² But A. K. Sen has suggested (*Economic Weekly*, Feb. 1964) that small farms may be more fertile than large ones because the superior survival rate of children on fertile farms leads to more rapid subdivision. Our analysis below suggests that given a random distribution of fertility (re farm size), the families with small farms and higher labor-land ratios are induced to shift to more intensive cultivation, making the land appear more "fertile." The data on irrigation-use and double cropping support this position [6, pp. 818, 825n].

glect of the soil and a consequent deterioration of "fertility" [1, pp. 62-64]. I do not think it is necessary to spell out all the consequences this analysis has for Bennett's calculations of "adjusted net revenue" and marginal productivity—both of which are based on the concept of intrinsic fertility differences. The dire results stemming from "declining labor-fertility ratios" would also seem to evaporate.

If then differences in fertility in any one region are less important than intensity of cultivation as an explanation of output, (and for the other reasons cited), it makes sense to look at the total surplus (rent and profit) generated by different levels of input. Now for this total we do have reliable figures (gross output minus type "A" inputs). These are given in Table 1. Most of the farms previously appearing as economically untenable now are seen to earn a surplus. If we set up maximum profit and rent per acre as a test of efficiency, then the appropriate procedure is to see how many groups are below, and how many above, the input level of the optimum group. (This would also be the correct method to use with Bennett's profit figures.) In place of Bennett's conclusion that only two out of 29 factor combinations could improve their position by increasing inputs, we find that 16 out of 29 could do so—these are mainly the medium and the larger farms in West Bengal, Madras, and Madhya Pradesh. Since all labor and capital inputs have been priced at the market rate, including imputed wages and imputed interest where necessary, it is reasonable to suggest that these farms are operating below the optimum level—that is, underutilizing their land. On the other hand, 13 groups which are below the profit-rent maximum have higher levels of inputs per acre, implying that the marginal productivity of labor has fallen below the imputed value of labor used in the input data. The last two columns of Table 1 show deviations of labor inputs and deviations of net product from the best performing group. They represent revisions of Bennett's adjusted net revenue data in line with the previous argument that varying inputs, not intrinsic fertility differences account for variations in net product. Given the size distribution of Indian farms, the underutilized large farms include a much larger total acreage than the smaller ones which have exceeded the optimum point [5, p. 48]. This means that a given reduction in labor inputs per acre on the small farms can be absorbed by a lesser increase in labor inputs on the large farms.

I do not share Bennett's belief that the marginal product of capital and labor shifted to these states would be negligible when the data show that increasing rents and profits per acre are obtainable with higher inputs. His speculation about the offsetting effects of a declining "fertility-labor ratio" is based on the static and erroneous notion that the imputed rent figures accurately measure fertility and that fertility is a fixed inherent quality of the soil that is unrelated to cultivation techniques. He also neglects dynamic factors. The Indian experience with movement of populations into new areas showed these populations to be more receptive to improved agricultural practices (which shifted the production function upward) than the long-established population in the area—a break with their old environment apparently

TABLE 1—LABOR INPUT AND NET OUTPUT DEVIATIONS FROM OPTIMUM LEVEL
(Values in Rupees per Acre)

| Size of Farm (Acres) | Rent and Profit ^a | Change in Labor Input ^b | Change in Net Product ^c |
|-------------------------|---------------------------------|---------------------------------------|---------------------------------------|
| Madhya Pradesh | | | |
| 0-5 | 73.4 | — | — |
| 5-10 | 57.3 | - 4.7 | -20.0 |
| 10-15 | 62.6 | - 6.6 | -17.4 |
| 15-20 | 52.0 | - 8.6 | -30.0 |
| 20-30 | 65.5 | -10.7 | -18.6 |
| 30-40 | 61.0 | - 9.6 | -22.0 |
| 40-50 | 46.7 | - 5.6 | -32.3 |
| 50 and over | 59.5 | - 9.6 | -23.5 |
| Madras | | | |
| 0-2.5 | 2 | +36.6 | -47.0 |
| 2.5-5.0 | 85.6 | — | — |
| 5.0-7.5 | 62.1 | - 6.7 | -30.2 |
| 7.5-10 | 44.4 | -10.8 | -52.0 |
| 10-15 | 14.8 | -16.4 | -88.2 |
| 15-20 | 20.6 | -19.9 | -84.9 |
| 20-25 | 23.3 | -16.5 | -79.8 |
| 25 and over | 43.1 | -20.4 | -73.9 |
| Punjab | | | |
| 0-5 | 49.6 | +18.0 | -17.8 |
| 5-10 | 62.9 | +16.1 | - 4.4 |
| 10-20 | 61.3 | +11.9 | -12.2 |
| 20-50 | 65.0 | - .9 | -21.3 |
| 50 and over | 85.4 | — | — |
| Uttar Pradesh | | | |
| 0-5 | 35.2 | +25 | -14.1 |
| 5-10 | 47.8 | +17 | - 9.5 |
| 10-15 | 35.4 | +14 | -24.9 |
| 15-20 | 42.4 | + 9 | -22.9 |
| 20 and over | 74.3 | — | — |
| West Bengal | | | |
| 0.01-1.25 | 73.5 | +36.5 | +17.5 |
| 1.26-2.50 | 83.9 | +19.5 | +10.9 |
| 2.51-3.75 | 50.7 | +27.6 | -14.2 |
| 3.75-5.00 | 77.8 | +12.2 | - 2.5 |
| 5.01-7.50 | 85.6 | +13.0 | + 6.1 |
| 7.51-10.00 | 92.5 | — | — |
| 10.01-15.00 | 76.0 | -10.0 | -26.5 |
| 15.01 and over | 55.7 | - 1.2 | -48.0 |

Source: Table 1 (Bennett).

^a Gross Output minus "A" inputs.

^b Difference between labor inputs in group with rent-profit maximum and all others.

^c Differences in net product between group with maximum rent-profit and all others.

induced them to give up traditional agricultural techniques [2, p. 114]. As for improvements (such as irrigation field channels), they tend to be a function of more intensive cultivation, i.e., more labor per acre in crop production, and

are a kind of bonus resulting from the greater number of labor hours which then become available in the off-peak months when the alternative use of such labor is limited or nil.

But there is a much more fundamental point relevant to the question of moving family labor out of agriculture. For a market economy where most of the inputs are purchased (both current and capital goods) and the output is sold, the true net output of the farm can be clearly determined. India's farms, particularly the small ones, have a remarkably low level of monetization of inputs—in Orissa only 8 per cent of the inputs are cash, in Uttar Pradesh 23 per cent [3, p. 61] [4, p. 81]. These are averages; the small farms drop below the stated figures. While Bennett's net revenue would be a close measure of net product in a market economy, it significantly understates the true output of family farms in India. The error lies in neglecting to include in total output the value of capital goods (bullocks, fences, buildings, sheds, etc.) that are largely produced with family labor (given an imputed value of market labor) but depreciated and charged as a cost against crop production. These depreciation charges are included in type "A" costs and reduce the net value of the farm crops, but in fact are not paid out, and family income is not thereby reduced. Hence output is understated if there is no estimate of the value of capital goods produced on the farm. Labor expended on the production of capital goods must also be added on the input side, but since this is family labor—much of it with zero opportunity cost—there is a question as to whether imputed labor inputs should be increased.

This underestimation of net output is more pronounced on the smaller farms because cost data show that capital maintenance and depreciation charges per acre are higher as the size of farm declines, while the percentage of purchased capital assets is lower. This is most apparent in bullock charges (a major cost item, second only to labor) and depreciation of farm structures. Bullocks are bred and raised on the farm and are an important element of capital formation. A detailed breakdown of the cost of raising bullocks shows a minor cash expenditure for concentrates and medicine, the rest being non-cash expenditures of imputed family labor used in tending the animals and gathering fodder for them, depreciation of animal sheds, etc. The annual costs of bullocks are then converted into daily costs and charged against the various crops as inputs. Regional grain prices determine the gross value of crops.

Since we have no estimates of the total value of capital goods produced on the family farms, we could use the Farm Management depreciation figures as a measure of such output, but this understates the output in cases where net capital accumulation is taking place—that is, where gross capital formation exceeds depreciation. The basic problem here involves imputing value to inputs which have little opportunity cost and where the capital goods produced have no market price. If we place a zero price tag on the family labor units used in off-peak periods to produce such goods, then the net value of crop production will correspond to the actual net product which the farmer retains—his farm business income. As we raise the imputed price of family labor, the net value of crop output drops and the imputed value of capital

goods (on a cost basis) rises, but capital goods are not usually included in estimates of farm output.

To a large extent, the appropriate price to attach to family labor depends on the problem being considered. In the surplus labor problem, the question is one of matching gains from added labor inputs in industry against possible losses of agricultural product. In this case only the family labor that can be used in industry should be given an imputed wage in agriculture. It would justify placing a zero price tag on a significant portion of child and female labor. How would this affect the relative net output figures of small and large farms? Women and children work both in the fields and on maintenance, tending bullocks, etc. Let us assume that all labor expended on field work and crop production has an imputed value equal to the market wage for male laborers, and that all indirect labor inputs are attributable to women and children with a zero opportunity cost. We are now able to compare Bennett's net product figures which are based on a full imputation of family labor expended on bullocks, capital maintenance, etc. (the depreciation of farm-produced assets included in his "other type A costs") with the alternative approach. Using data for Uttar Pradesh which initially showed the small farms producing less net output (with more resources) than the largest farms, we now see a significant change (Table 2). Farm business income (the income from all family-owned

TABLE 2—DATA ON FARM BUSINESS INCOME AND NET PRODUCT—
UTTAR PRADESH, 1955-56
(Values in Rupees per Acre)

| Size Group (Acres) | Cash and Kind Expenses ^a | | Gross Output | Farm Business Income ^b | Net Product ^c | Bennett's "Net Revenue" ^d |
|-----------------------|--|--------|-----------------|---|-----------------------------|--|
| | As Per Cent of Cost "C" | Rupees | | | | |
| 0-5 | 31.7 | 82 | 277 | 195 | 208 | 99.2 |
| 5-10 | 33.4 | 70 | 240 | 170 | 181 | 103.8 |
| 10-15 | 34.6 | 64 | 204 | 140 | 155 | 88.4 |
| 15-20 | 38.5 | 67 | 200 | 133 | 150 | 90.4 |
| 20 and over | 39.9 | 58 | 205 | 147 | 162 | 113.3 |

^a Includes all actual cash expenditures for seed, fertilizer, machinery, parts, total replacements, hired labor, land tax, irrigation charges, hired bullock labor, etc., plus "kind" expenses: crop retained for seed, animal feed, etc. Since we have a large sample, the annual purchases for tools, parts, etc. should approximate annual depreciation of purchased capital goods [4, p. 81].

^b Gross output minus cash and kind expenses. Total return to all factors owned by farm family, including labor.

^c Net value added (net product) equals farm business income plus payments in cash and kind to hired labor. Slightly understates net value added by not including very small interest charge on borrowed capital.

^d Bennett's net product, Table 2. It is lower than our net value added by depreciation charges for family labor contained in farm buildings, bullocks, etc.

Source: [4, p. 81], [6, pp. 817, 822].

factors including labor) which indicates the actual cash and produce available for consumption, is higher on the small farms. The important net product figures which measure the total welfare contribution of agriculture now are higher on the small farms and more in line with inputs and gross product (which is not a bad indicator of performance in the subsistence sector). This points to the conclusion that when we are dealing with farms which are in large part outside the market economy, the imputation problem is of central importance in evaluating comparable efficiency.

Finally, with reference to policy, Bennett seems overly bound by static functions and a conventional approach. I doubt that the larger Indian farmers, with the lowest average output per acre found anywhere, have rationally considered the return to additional capital investment and rejected it because of low expected yield. All the evidence indicates that a significant number reject offers of loans for seed, irrigation, fertilizer, and technical advice because they are satisfied with one crop a year dry cultivation [5, pp. 48-51]. On an above average size farm, the simplest method of cultivation will yield an adequate subsistence—by Indian standards. Not only is government investment needed to help shift agricultural production functions upward, but government land-tax and food-price policies must be revised so as to offer both pressures and inducements to the Indian farmers to respond to the output requirements of the market economy.

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The Integration of Capital Budgeting and Stock Valuation: Comment

In a recent paper [4] Lerner and Carleton (hereafter L&C) attempt to integrate existing hypotheses of capital budgeting and stock valuation into one comprehensive theory. L&C's general idea is that this purpose can be achieved by replacing "the single-equation nature of recent capital budgeting and security valuation models" [4, p. 683] with a multiple-equation model. Their attempt should be welcomed, since such a general approach promises to be fruitful for further exploration into the theory of finance. Nevertheless, it is here suggested that L&C's model involves implicit as-

sumptions that are inappropriate for a general theory, and, moreover, owing to a technical error, the conclusions of their own model are misinterpreted.

L&C develop a two equation model. The first (equation 1.4) is a valuation function, and the second (equation 2.3) is an investment opportunities function. This model determines the optimal rate for two decision variables: the rate of return on assets and the earning retention rate which serves as a surrogate for the firm's investment budget. The optimal rates are defined as those which maximize the firm's market value.

The basic fallacy of the proposed model is that it is constructed to determine the optimal *average* rate of return rather than the marginal rate.¹ Since the average rate of return does not reflect the specific form of the investment-opportunities function, it is not uniquely related to the marginal rate. Thus the average rate is ambiguous and is, therefore, an inappropriate variable on which to base capital investment decisions. Moreover, such a model is liable to lead to misinterpretations.

One example is provided by L&C's conclusion that the optimal rate of return should in general be greater than the firm's cost of capital (k). L&C misinterpret this result: "This conclusion that firms should not in general invest until $r = \alpha/(1 - \psi)$ [or equivalently until $r = k$] is admittedly *at variance with traditional statements* on the subject" [4, p. 699, italics ours].

This statement should be challenged. L&C's optimal average rate of return is determined in a model based on a downward sloping investment-opportunities function. This in itself implies that the marginal rate is lower than the average rate. In order to maximize its present value the firm should invest—according to "traditional statements"—until the marginal rate of return equals the cost of capital. It follows, therefore, that the average rate of return is higher than the cost of capital.² Thus L&C's explicit conclusion does not vary with traditional theory.

Nevertheless, a rigorous analysis of L&C's model, which explicitly differentiates marginal from average rates of return, proves that the implied marginal rate is *lower* than the cost of capital. We now turn to the proof of this statement.

The valuation equation is given by 1.4' [4, p. 686]³ as

$$P_0 = \frac{(1 - b) Y_0}{k - rb},$$

¹ Although the rate of return is not explicitly defined in L&C's model our interpretation is implicit in equation 1.4' and the following text, [4 pp. 687–88]. Moreover, after defining the marginal rate of return (r') and the average rate (r) in connection with equation 2.3 [4, footnote 14, p. 695], L&C go on to state that "The reason for our formulation rests on the fact that (from the stock-valuation equation) r rather than r' is the focus of capital-budget decisions designed to maximize share price."

² In terms of the traditional theory the existence of investment opportunities with internal rate of return greater than the market cost of capital "means that adjustment to a competitive equilibrium is incomplete" [7].

³ L&C attribute this equation to M. J. Gordon [3, Ch. 4]. The equation was, however developed earlier by J. B. Williams [8, Chs. 6, 7]. Cf. D. Durand [2, p. 44].

where:

P_0 = the present market value of the firm.

Y_0 = the present annual earnings.

b = the earning retention rate, which serves as a surrogate for the investment budget.

r = the average rate of return on the investment budget.

L&C then substitute rA_0 for Y_0 (where A_0 stands for current total assets) to obtain equation 1.4'':

$$P_0 = \frac{(1 - b)rA_0}{k - rb}.$$

After assuming that r changes with b , they solve equation 1.4'' for r [4, p.688] to obtain equation 1.4''':

$$r = \frac{Pk}{b(P - A) + A}.$$

It should be emphasized that r , in the numerator of equation 1.4'', stands for the average rate of return on *existing* assets A_0 and is, therefore, a *constant* (hereafter marked r_0). In the denominator, however, r represents the rate of return on the current capital budget b , and is, therefore, a *variable*. Since L&C do not differentiate between the r 's in solving equation 1.4'', the solution they obtain for 1.4''' is invalid.⁴

The correct solution for r is⁵

$$(1.4^*) \quad r = r_0 \frac{A_0}{P} + \frac{1}{b} \left(k - r_0 \frac{A_0}{P} \right).$$

Both equations 1.4'' and 1.4* represent a family of iso-price hyperbolas which intersect the line $r=k$ at $b=1$. The two equations differ, however, in their asymptotes. L&C's curves (1.4''') all asymptotically approach the same horizontal line $r=0$, whereas the 1.4* curves do not all approach the same horizontal asymptote. For a given P the latter curve approaches the horizontal line

$$r = r_0 \frac{A_0}{P}.$$

The greater the value of P , the lower the asymptotic line.

⁴ $r_0 = Y_0/A_0$ by definition. Nevertheless, L&C substitute Y_0 of equation 1.4' by rA_0 instead of r_0A_0 [4, p. 687]. This slip is insignificant as long as r is a constant. It becomes, however, a crucial error at a later stage, when L&C make the assumption that r is a variable. Had L&C worked with the 1.4' version of the valuation equation, this error might be avoided automatically.

⁵ Or equivalently: $r = \frac{Y_0}{P} + \frac{1}{b} \left(k - \frac{Y_0}{P} \right).$

Given the investment-opportunities schedule, L&C find the optimal capital budget b from their tangency condition [4, eq. 2.6, p. 697]

$$\left(\frac{\partial r}{\partial b}\right)_{\text{eq. 1.4}'''} = \left(\frac{\partial r}{\partial b}\right)_{\text{eq. 2.3}}$$

Since 1.4''' is invalid, the proper tangency condition should be:

$$(2.6^*) \quad \left(\frac{\partial r}{\partial b}\right)_{\text{eq. 1.4}^*} = \left(\frac{\partial r}{\partial b}\right)_{\text{eq. 2.3}}$$

From equation 2.6* it follows that

$$(2.6^{**}) \quad \left[\frac{\partial(rb)}{\partial b}\right]_{\text{eq. 1.4}^*} = \left[\frac{\partial(rb)}{\partial b}\right]_{\text{eq. 2.3}}$$

at the point of tangency. Since

$$\left[\frac{\partial(rb)}{\partial b}\right]_{\text{eq. 2.3}} \equiv r'$$

is the marginal rate of return on investment it follows that at the point of tangency

$$r' = \left[\frac{\partial(rb)}{\partial b}\right]_{\text{eq. 1.4}^*}$$

The latter derivative is constant and equal to⁶

$$r_0 \frac{A_0}{P}$$

As we showed above,

$$r = r_0 \frac{A_0}{P}$$

$$^6 (1.4^*) \quad r = r_0 \frac{A_0}{P} + \frac{1}{b} \left(k - r_0 \frac{A_0}{P} \right)$$

$$rb = r_0 \frac{A_0}{P} b + k - r_0 \frac{A_0}{P}$$

$$\frac{\partial(rb)}{\partial b} = r_0 \frac{A_0}{P}$$

or equivalently:

$$\frac{\partial(rb)}{\partial b} = \frac{Y_0}{P}$$

represents the horizontal asymptotes of the hyperbolas given by equation 1.4*. These hyperbolas intersect the line $r=k$ at $b=1$ and, therefore, their horizontal asymptotes are lower than the line $r=k$. Thus, we can conclude that the optimal marginal rate of return on investment, implied by L&C's model, is lower than the cost of capital.⁷

Figure 1 summarizes our analysis. P_1 and P_2 represent the family of the iso-price hyperbolas 1.4*. $L_m C_m$ is the investment-opportunities function

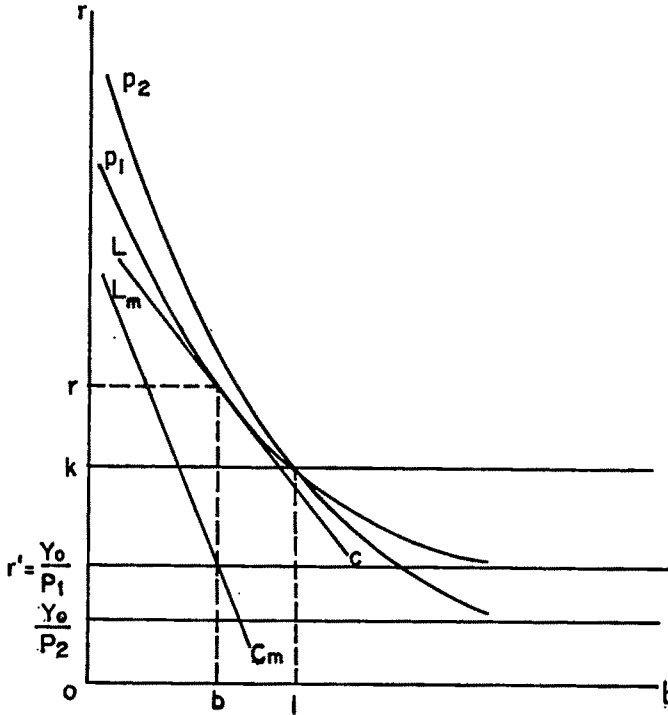


FIGURE 1

which links the *marginal* rate of return on investment with the investment budget. LC is that same function linking the investment budget with the *average* rate of return, as employed by Lerner and Carleton [4, cf. n. 14, p. 695]. The figure shows that at the point of tangency the average rate of return is higher than the cost of capital. Nonetheless, the figure also shows that the marginal rate of return is lower than the cost of capital. The figure also implies that, as the investment-opportunities function moves upwards, the optimal average rate of return on investment may increase, while the optimal marginal rate of return declines.

These results can be explained, however, by the nature of the valuation equation 1.4'. It is there implicitly assumed that, as the firm increases its

⁷ This same conclusion was reached by D. Bodenhorn [1, p. 487] and J. Lintner [5, p. 302].

investments by raising the rate of retained earnings (*b*), the *future* investment opportunities increase proportionately and thus become more profitable.⁸ The increase in future profitability constitutes an additional (indirect) return which compensates the firm for the loss caused by the marginal investment. A correct definition of the marginal rate of return should include not only the direct return, as employed by L&C, but the indirect return as well. Once this is done, the marginal rate of return at the optimum is equal to the cost of capital, and thus "Lerner and Carleton's results are no longer inconsistent with the traditional theory."⁹ Nevertheless, since the particular dependence of future investment opportunities on the current capital budget has no general economic validity, it is questionable whether this valuation equation is appropriate for a general theory of finance.

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⁸ Cf. D. Bodenhorn [1] and M. H. Miller [6].

⁹ See the following comment by Jean Crocket and Irwin Friend, this *Review*.

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The Integration of Capital Budgeting and Stock Valuation: Comment

In a recent article in this *Review* [2],¹ Professors Lerner and Carleton (hereafter L&C) state as perhaps the most important substantive conclusion of their analysis that "a firm should not in general invest until [the] internal rate of return equals the market discount rate" [2, p. 684]. They note that "this . . . is admittedly at variance with traditional statements on the subject" [2, p. 699]. We shall point out in this Comment that their

¹ L&C have used the same theoretical structure in two subsequent publications [3] and [4].

conclusion denying the validity of the traditional investment criterion seems to be incorrect. We shall also develop briefly the implications of a stock valuation model which we consider to be more useful for capital budgeting purposes than the constant growth model which they apply.

A basic difficulty in the L&C analysis is that the internal rate of return (r) is used in several different senses which are at times inappropriate to the analysis—a difficulty compounded by their use of a constant growth model. They use r at times as (i) an historic variable, $r = Y_0/A_0$ where Y_0 and A_0 are initial income and assets respectively. This ratio is given for any particular firm (though of course it may and does vary among firms) and cannot be affected by current decisions. At other times r is used as (ii) an average return on future investment. This return is relevant to future earnings growth and r is used in this sense when L&C express g , the assumed constant growth rate in dividends, as the product of r and b , where b is the fraction of earnings retained. In their general model, it is this r which they wish to optimize by appropriate decisions as to net investment per time period or in other words appropriate payout (and at times external financing) policies. Finally, r appears at times to be used as (iii) a marginal return on future investment. This again is a controllable variable depending on the size of future capital budgets. Clearly, r must be used in this sense when we consider the minimum rate of return which makes an investment project acceptable.²

In determining whether to accept or reject an investment proposal, indirect as well as direct returns on marginal investment are of course relevant. Current investment—in addition to the income stream which it generates directly—may open up the possibility of highly attractive future investment which would not have been feasible otherwise. To the extent that such investment opportunities are created, the value of the firm is enhanced, and current investment should be credited with an indirect return on this account in making the investment decision. However, there is no immediate contribution to earnings and asset growth related to this indirect return, so that in computing $g = br$ it is appropriate to use the (average) rate of direct return only.

The basic model L&C use for growth-stock valuation assumes that the rate of return is expected by investors to remain constant over time, the corporation is expected to retain a constant proportion of its income, all growth comes via retained earnings, and as a consequence dividends grow at a constant rate. If k is the market rate of discount, stock price (P_0) is initially expressed as

$$(1) \quad P_0 = \frac{(1 - b) Y_0}{k - rb}$$

² Unless Lerner and Carleton are thinking of r in this third sense, it is difficult to understand their assertions quoted at the beginning of this Comment.

or

$$(2) \quad P_0 = \frac{(1-b)rA_0}{k-rb}.$$

We prefer the first formulation since the second involves the assumption that the average return on future investment, the r in the denominator, is equal to the historic return Y_0/A_0 , the r is the numerator. For predictive purposes this may be a reasonable first approximation for many firms, though inappropriate for others; but for optimization purposes it is entirely unsuitable, unless in some sense we wish to solve for what the current yield on assets would have been if optimal decisions had been made through all past history.

L&C appear to have some difficulty in determining the optimum retention rate from these relationships by direct procedures, without recourse to a simultaneous equations approach. In their simplest case, where r and k are both taken as given and therefore independent of b , the difficulty vanishes when we observe that P is a substantially different function of b depending on whether $r=k$, $r<k$, or $r>k$, so that we must obtain separate solutions in each of the three cases. In the first case P is not a function of b at all and there is little point to computing a partial derivative with respect to b . In the second case it is a monotonically decreasing function of b and there is a well-defined corner maximum at $b=0$. In the third case there is a maximum at $b=k/r$, where price becomes infinite.

In the somewhat more realistic case where r is taken to be a (decreasing) linear function of b

$$(3) \quad r = \gamma_0 + \gamma_0\gamma_1 b, \quad (\gamma_0 > 0, \gamma_1 < 0)$$

the price function becomes

$$(4) \quad P_0 = \frac{(1-b)Y_0}{k - b(\gamma_0 + \gamma_0\gamma_1 b)},$$

by substitution in equation (1) above. Again there is no difficulty in determining the optimum retention ratio, though different solutions will occur in different regions of the $\gamma_0\gamma_1$ plane.

Assuming for convenience that k is determined independently of b and is greater than zero, the optimum retention ratio is zero when $\gamma_0 < k$, since in this case r in (3) can never exceed k for b in the relevant range. This is analogous to the case $r < k$ under the simpler assumption above. When $\gamma_0 + \gamma_0\gamma_1 > k$, an infinite price maximum results, since the denominator of (4), which is a quadratic function, must vanish for some value of b between zero and one. In this case, which is analogous to the case $r > k$ above, average return exceeds k even with 100 per cent retention.

However, for $k - \gamma_0\gamma_1 > \gamma_0 > -2\gamma_0\gamma_1$ it can readily be shown that a finite price maximum occurs. This condition simply implies that average return will fall below k with 100 per cent retention while marginal direct return will still exceed zero at that point. By setting the partial derivative of (4) with

respect to b equal to zero, we may determine the optimum retention ratio in this range:

$$(5) \quad b = 1 - \sqrt{1 + \frac{\gamma_0 - k}{\gamma_0 \gamma_1}}.$$

At this value of b it is quite true that the marginal rate of direct return, $\gamma_0 + 2\gamma_0\gamma_1b$, is lower than k . This is because two types of indirect return are rather artificially introduced by the special assumptions of the model used by L&C. First, in the absence of either external equity financing or increases in the debt-equity ratio, current investment is necessary to generate future retained earnings to take advantage of future high return investment possibilities. Second, and more basic, since average and therefore marginal r is assumed to be a constant function of b and not of dollars of new investment, the investment opportunities curve in terms of the latter must be shifting to the right over time for any $b > 0$, and in fact shifts faster the larger we make b , so long as the marginal direct return on investment remains positive.³ As time goes on, a given retention rate implies an increasing dollar amount of reinvested earnings, if b and r are both greater than zero. Thus the assumption that the average return, r , is a constant function of b means that a given r is obtainable on increasing dollar amounts of investment over time, and the higher the b selected the more rapidly will grow the dollar amounts on which that given r can be obtained (so long as each dollar reinvested yields some earnings growth). Hence current investment, in addition to providing an immediate and continuous direct return, provides an indirect return by causing the investment opportunities curve (defined in this context as a functional relationship between the volume of investment per time period and direct marginal return) to shift in a more favorable way over time than if the investment were not made.⁴

Once indirect return as well as direct return is considered, L&C's results are no longer inconsistent with the traditional theory. However, in our judgment the above considerations seriously prejudice the usefulness of the extremely popular constant growth model, at least as a device for determining the optimal retention ratio, though not perhaps for descriptive purposes when the retention ratio is predetermined. The dependence of future investment opportunities curves on the retention rate, particularly a dependence in which future opportunities are favorably affected by higher and higher

³ A similar point has been made by Merton Miller [5], in discussing the implications of a model presented by John Lintner.

⁴ In their May 1966 paper [4] Lerner and Carleton make the average return, r , a function of the growth rate of assets, rather than the retention rate. However, the implicit assumption that future investment opportunities are favorably affected by high retention rates is still present, since a time shift in the intercept of the rate of return schedule is posited, such that the sum of

$$(\alpha_0 - \beta_0)\lambda + (\alpha_1 - \beta_1)\lambda^2 A_t$$

plus the shift remains constant over time, where A_t is assets in period t , other symbols refer to constants, and $\alpha_1 - \beta_1 < 0$. As assets grow over time, larger and larger upward shifts are required each period and the higher the retention rate the more rapidly the required shifts increase [4, p. 211].

retention rates, is open to considerable question. Nevertheless, it is implicit in the assumption that a constant growth rate is attainable regardless of the (constant) retention ratio chosen.

The alternative assumption that the investment opportunities curve shifts over time in a manner independent of the firm's retention decision suggests an almost trivial solution to the retention decision under the basic assumption of a constant growth rate. Suppose that the curve shifts over time in such a manner that the dollar amount of investment bringing a given direct marginal return grows by a given percentage, say γ each year (based perhaps on the growth rate of the economy). Then b should be so chosen that the growth rate of investible funds and of earnings will equal that of investment opportunities or $br = \gamma$. This is in fact the only constant growth rate of earnings available to the firm under these circumstances and the only one which maintains a stable relationship of r to b over time.

Realistically, we would expect that certain investments will have favorable effects on future investment opportunities, but the magnitude of these effects will vary among investments and there is no reason to suppose that the aggregate effect will conform to the specifications of the L&C model, for an arbitrarily selected retention ratio.

It may be useful to demonstrate more directly that a stock-valuation formula can be combined with an investment opportunities schedule (and if desired with other relevant equations) without affecting the validity of the traditional theory. For this purpose it is desirable to introduce an alternative valuation equation which allows for the interdependence of investment decisions in a more explicit and flexible way than does the constant growth model. Then it can readily be shown (though in a sense the proposition is tautological) that value is maximized when marginal return on investment (direct plus indirect) equals the earnings capitalization rate, regardless of whether the profitability of investment in one period is or is not a function of investment in another period or whether the risk class of the enterprise is affected by new investment.

We shall assume a company has current net income of Y_0 which it would earn each year in perpetuity if it made no new net investments; plans to invest I_1 at the beginning of this year and I_2 at the beginning of next year on which it would earn r_1 and r_2 respectively in perpetuity; raises the necessary funds by issuing common stock at a "cost" of i which is equivalent to the capitalization rate on the old equity and pays out all net income in dividends.⁵ Then the current total value of the stock may be written as

$$(6) \quad S = \frac{Y_0}{i} + \frac{I_1(r_1 - i)}{i} + \frac{I_2(r_2 - i)}{(1 + i)i}.$$

Maximizing S with respect to I_1 requires $dS/dI_1 = 0$. For simplicity we shall assume $di/dI_1 = 0$ but, following L&C, we shall consider $r_j = f(I_j)$. The resulting equation is

⁵ The model, but not the analysis and conclusion, is essentially identical with that used by Diran Bodenhorn [1].

$$(7) \quad \frac{dI_1 r_1}{dI_1} + \frac{1}{(1+i)} \frac{dI_2(r_2 - i)}{dI_1} = i.$$

In other words, the direct marginal rate of return on I_1 plus the discounted indirect marginal rate of return must be equal to the capitalization rate at the optimum. If I_2 and r_2 are both independent of I_1 , the indirect return is of course zero. Similarly, if $dr_1/dI_1 = 0$, the direct marginal rate of return may be written as r_1 . However, the introduction of the functional relation between r and I or of interdependence among investment decisions does not affect the validity of the traditional investment criterion.

A change in the required rate of return on the company's shares as a result of new investment simply requires an allowance for another type of indirect return—i.e., the rate of return represented by the capital gain (or loss) per unit of investment as a result of the change in the capitalization rate. Thus, if we allow for a change in the capitalization rate as a result of a single new investment, the current value of stock may be written as

$$(8) \quad S = \frac{Y_0}{i} + \frac{I_1(r - i)}{i}$$

but now $i = F(I)$ so that $di/dI \neq 0$. Maximizing S as before, the resulting equation is

$$(9) \quad \frac{dI_1 r_1}{dI_1} - \left(\frac{Y_0 + I_1 r_1}{i} \right) \frac{di}{dI} = i$$

so that again the direct marginal rate of return on I_1 plus the indirect marginal rate of return (which is negative if $di/dI > 0$, with the rate of loss measured by the capitalized value of both old and new earnings multiplied by the marginal rate of increase in the capitalization rate) must be equal to the new capitalization rate at the optimum.

In concluding, we should like to question briefly the usefulness of the forms used by L&C to specify the very important functional relations (1) between the market rate of discount (or capitalization rate) and company growth in per share earnings [2, p. 690] and (2) between the rate of return and the retention ratio [2, p. 696]. The first implicitly assumes that the capitalization rate is equal to a constant plus a fraction of the growth rate, the second that the rate of return is a simple linear function of the retention ratio. Both assumptions seem tenuous to us. We doubt that all companies with zero growth expectations are regarded as being equally risky, while the rate of return is better expressed as a function of investment than of the retention ratio.⁶

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⁶ Somewhat different assumptions are used in the May 1966 Lerner-Carleton article [4]. In addition to the different rate of return function discussed earlier in this Comment, the new functional relation between the capitalization rate and company growth is postulated to be linear in the variance of the growth rate rather than in the growth rate itself. This new assumption seems more plausible to us.

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Reply

If we interpret the comments of Professors Crockett and Friend and of Ben-Shahar and Asher with respect to our 1964 [2] article correctly, their principal criticisms are directed to two related points: (1) an alleged failure to distinguish between a corporation's average and marginal rate of return;¹ and (2) an allegedly unreasonable assumption of a corporate profit opportunities schedule which is stationary through time.² These comments are constructive in that they require us to be more explicit in specifying our model than we were in 1964.

With respect to the first criticism, it is true that our model does call for the firm that strives to maximize shareholder wealth to select a target average rate of return on assets. Since we restricted ourselves to the case of a one-asset firm³ only one rate of return is possible for the firm in any given period. Our concern over the target average rate was dictated by the share price model itself, for when a person purchases a share of stock, he buys an expected average rate of return over an indefinitely long period. More importantly, the model referred to a firm's equilibrium position; hence it was possible to talk about the unique solution rate of return as if it lasted indefinitely into the future.

Both critics of our paper, however took us to task for not discussing the marginal r (which both believe in equilibrium will be less than k). The issue that must be raised before this problem can be discussed is "with respect to what is r marginal?"

The traditional capital budgeting approach answers the question by stating that r is marginal with respect to additional dollars of investment outlays. Since our price model postulates a constant growth rate as a variable, this approach will not do. Both critics assert that the r in the numerator of our valuation equation refers to a different time period (namely, historical) than the r in the denominator (future growth rate) of the equation.

¹ Vickers [6] has also suggested this criticism.

² In 1963, Lintner [1] suggested that for analytical purposes, it was convenient to think of a firm as having a stationary profit opportunities schedule. Miller's discussion [5] of this pioneering work raised some of the same issues underlying the two comments on our paper.

³ The implicit assumption of a homogeneous asset base, so that investments increase the scale rather than the diversity of the firm, is common to many articles in the field. See for example any of the several studies by Modigliani and Miller and [3], Ch. 4.

Hence, the r in the denominator may be marginal (or at least incremental) with respect to time. Since we describe a continuous and static equilibrium situation, for a one-asset firm, both of the r 's are equal in solution and invariant through time. The critics' assertions are thus inappropriate.

The issue of a marginal rate of return, however, can be examined in another context. If growth is defined as the product of two variables, r and b , and we ask "how does growth vary with b ," we find

$$(1) \quad \frac{\partial g}{\partial b} = r + \frac{dr}{db} b.$$

The first term on the right, r , our critics call the direct effect of the marginal rate of return; the second term, the indirect effect.

Our profit opportunities constraint (which we referred to as the LC function in our article) was specified as:

$$(2) \quad r = \gamma_0 + \gamma_1 b.$$

Obviously, if γ_0 of equation (2) corresponds to r of equation (1) and γ_1 of equation (2) to dr/db , our r is $\partial g/\partial b$. That is, the "marginal rate of return" or marginal growth rate with respect to changes in b , is simply the (unique solution) rate of return on assets as a function of b .

We have shown elsewhere [3, p. 136] that for a unique price maximization to occur, the specific form of the LC function depends upon the way the k function is specified. Hence more information is required before we can determine the variable that we want r to vary with. In the trivial case, when k is a constant, our specification $r = \gamma_0 + \gamma_1 b$ will satisfy the valuation model. In the more realistic case, when k itself is a function of the investors' alternatives and the riskiness of the firm, a more complicated profit opportunities schedule may be required to constrain the valuation equation. Specifically, if risk is measured by the variance of the firm's growth rate, under some conditions, a deterministic solution can be found if the profits opportunities function is specified as $r = \gamma_0 + \gamma_1 g$.

With respect to the second criticism, the specification of a time invariant profit opportunities schedule is, of course, necessary in order to be consistent with a constant expected growth rate share price model. By appealing to changes in national income or to changes in the prices of substitute and complementary products, the functions can plausibly be considered as stationary [3, Ch. 5]; also [4].

In a very general corporate financial model, it would be desirable to utilize time-varying profit opportunities and discount function. The appeal of such a model, however, is reduced by practical difficulties. Specifically, the solution to such an equation requires the use of the calculus of variations and we find time vectors of return, retention rates, and growth rates rather than single values. If an easier analytical method than this were found for handling the problem, a major breakthrough in the field could occur.

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The Soviet Collective Farm as a Producer Cooperative: Comment

Professor Domar has set up a very strange model of an ideal cooperative farm.¹ A certain area of land is being cultivated by a group of workers and the net produce shared amongst them. They have to decide how much work to do. According to Domar, they choose to maximize the return per unit of work. He does not offer any explanation of why they should do so.

It would be more natural to suppose that they are interested in income per man year. To maximize income requires the marginal product per unit of work to be zero. The members of the cooperative may be supposed to prefer a smaller amount of work. If they are all alike, they can decide what is the marginal disutility of effort at various levels, that is, how much income an individual would forgo in order to do less than any given amount of work. They then fix the amount of work by the rule that marginal disutility is not greater than marginal product. If they all have different evaluations, some means has to be found to arrive at an agreed plan.

In the article by Benjamin Ward to which Domar refers the problem is different. A number of workers have been allotted a plant (on payment of rent to the state), and are free to regulate the numbers to which employment is offered. If the initial labor force (at the moment when this system is introduced) is less than that at which income per man is maximized, they recruit more colleagues, up to the point where marginal revenue is not less than marginal nonlabor costs. But if the initial labor force was larger than this, how are they to proceed? Income *for those that remain* could be increased by dismissing some. How do they choose who is to become unemployed or to seek work at a less prosperous enterprise?

¹ "The Soviet Collective Farm as a Producer Cooperative," *Am. Econ. Rev.*, Sept., 1966, 56, 734-57.

In setting up models of this type it is necessary to begin by asking the elementary economic question *cui bono*?

JOAN ROBINSON

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Reply

I fully share Professor Robinson's objection to the assumption that workers in a co-op would "choose to maximize the return per unit of work," and I said exactly that on page 742 of my paper. The exclusion of this assumption gave rise to the "Model with a Supply Curve of Labor" (pp. 742-47) which embodied the essence of the suggestion made in the third paragraph of her note. Hence its purpose escapes me.

Turning to another matter, I have been informed that I failed to acknowledge an important contribution to the theory of cooperatives by Professor Peter G. Helmberger, "Cooperative Enterprise as a Structural Dimension of Farm Markets," published in the *Journal of Farm Economics* in August 1964.¹ For that matter, a number of other papers on co-ops by Helmberger and others have been published in that journal (the latest appearing in the August 1966 issue). I have to plead guilty to having neglected an important periodical.

EVSEY D. DOMAR

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¹ The exact reference is *Jour. Farm Econ.*, Aug. 1964, 46, 603-17.

Returns to Scale and the Spacing of Isoquants: Comment

In a recent issue of this journal [1, pp. 501-5] A. M. Levenson and Babette Solon show the error in the usual textbook statement that greater (less) than unitary scale elasticity implies that successively numbered isoquants are getting ever closer (farther apart). A feature of their paper, which in no way detracts from the main results, is that the authors say "It is the purpose of this note to show (1) that isoquant spacing depends on marginal, not average, returns to scale; and (2) that the above usually stated relationships between average returns to scale and spacing of isoquants are accurate only for homogeneous production functions" [1, pp. 501-2], but their paper does not show how isoquant spacing depends on marginal returns to scale, nor does it show (2) which is, in fact, false. This comment is intended to supplement the Levenson-Solon paper by clearing up these two points.

To show the relationship between isoquant spacing and marginal returns to scale for a production function $f(K, L)$ it is convenient to express K and L in terms of $u = (K^2 + L^2)^{1/2}$ and $v = K/L$, the distance from the origin of, and the slope of the ray through, the point (K, L) .¹ Then marginal returns to

¹ The explicit expressions for K and L are $K = uv(1+v^2)^{-1/2}$, $L = u(1+v^2)^{-1/2}$ whence $f_u = f_K K_u + f_L L_u = (K^2 + L^2)^{-1/2} [K f_K + L f_L]$.

scale can be defined as $f_u(u, v)$ the partial derivative of output with respect to distance from the origin holding K/L constant. At (K, L) the distance, along the ray, between isoquants representing output levels c apart is determined by the solution for t of $f(u+t, v) - f(u, v) = c$ which, on taking differentials, yields the equation $(du+dt)f_{u+t}(u+t, v) - du f_u(u, v) = 0$, and so $dt/du = f_u(u, v)/f_{u+t}(u+t, v) - 1$. Since f_u and f_{u+t} are assumed positive for a production function this shows that $dt/du < 0$ or > 0 according as f_u is increasing or decreasing; so that, for sufficiently small c , whether or not isoquants representing output levels c apart are getting closer in the neighborhood of (K, L) depends on whether or not marginal returns to scale are increasing there. This condition, of course, also determines whether or not the vertical section of f by the plane through the ray with slope K/L is convex above that point.

With respect to (2) of Levenson and Solon's statement of purpose average returns to scale can be defined as $f(u, v)/u$ and the coefficient of scale elasticity, E , as $(u/f)f_u$, or marginal divided by average returns to scale. With v fixed, all these quantities are functions of u alone and by the well-known relation between marginal and average functions $E > 1$ (< 1) is equivalent to the statement that average returns are increasing (decreasing). Thus $E > 1$ (< 1) will imply that isoquants are getting closer together (farther apart) for any production function which is such that average and marginal returns always increase or decrease together, and only for such functions will these usually stated relationships hold. That such productions need not be homogeneous is shown by considering f of the form $R(K, L) + S(K, L)$ where R is homogeneous of degree one and S is homogeneous of degree $n+1$ (> 0). It is readily verified that this f is not homogeneous; and by using note 1 and Euler's theorem, that, for it, the rate of change of marginal returns to scale is $n+1$ times the rate of change of average returns to scale so that these will always move in the same direction and the usually stated relationships will hold.

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1. A. M. LEVENSON AND BABETTE SOLON, "Returns to Scale and the Spacing of Isoquants," *Am. Econ. Rev.*, June 1966, 56, 501-5.

BOOK REVIEWS

General Economics; Methodology

On Political Economy and Econometrics: Essays in Honour of Oskar Lange.

No editor listed. Warsaw, Poland: Polish Scientific Publishers, 1965;
New York: Pergamon Press, distributor. Pp. viii, 661. \$18.50.

This volume began as a *festschrift* celebrating Lange's sixtieth birthday and ended as a *festschrift* in memoriam. In either case, it is not a suitable testimonial to a person of the intellectual power Lange exhibited in the 'thirties and early 'forties. Indeed, it is doubtless the worst *festschrift* ever published; and unless standards degenerate markedly, it will maintain this rating throughout all time. This is a pity; but the greater pity is that if one judges by the copious citations to Lange's recent work, Lange himself would not agree.

The reader gets a foretaste of what is to come on page one: "The crucial moment of his [Lange's] life came in 1918. On the occasion of the hundredth anniversary of the birth of Karl Marx, the fourteen-year-old boy gave . . . a lecture on Marxism. In the autumn of that year he took part in disarming the Germans, . . . immediately established contact with the Council of Workers' Delegates, . . . organized a circle of the Union of Polish Socialist youth," all the while reading Marx, Engels, and Kautsky and studying anthropology, history of religions, sociology, mathematics, and comparative linguistics. Add a little thinly disguised sex and one has an adolescent Helen MacInnes hero.

Naiveté and nonsense do not end here, however. P. Baran and P. Sweezy inform the reader that the U.S. economy became stagnant after World War II (p. 16); that bourgeois economics can be divided into two parts, each of which clearly shows "how developing contradictions in the economy itself call forth corresponding contradictions in the realm of theory" (p. 18); and, most surprising, that J. K. Galbraith "turns out on careful inspection to be the most enthusiastic apologist for a society dominated by *big* business" (p. 29).

This is not all. M. Kalecki tells the reader that historical materialism is a generalized econometric model; A. A. Konius that marginal utility theory confirms the theories of Marx; Kuczynski that a decrease in variable capital in the United States is connected with "the parasitic tendencies of capitalists while in socialist countries . . . [it] leads to a more rapid growth in gross total product"; März that Nietzsche was a dominating influence on Schumpeter; Mrs. Robinson that "market research in a capitalist world is directed to finding out how to bamboozle the housewife"; and Singh that "co-existence alone could help the Soviet oasis to survive in the capitalist desert." That's the way it goes with dispassionate rationality.

Things are not as bad as they seem; but they almost are. Some useful summaries of research, mostly their own, are given by R. Frisch, P. C. Mahalano-

bis, F. Perroux, K. Rothschild, R. Stone, J. Tinbergen, and G. Tintner. Neyman contributed an interesting, brief history of behavioristic views in regard to mathematical statistics. And, by alphabetical coincidence, the best was saved for last. The papers by A. Wakar, J. G. Zielinski, and L. J. Zimmerman are real contributions to the problems they tackle.

The price of the book itself should limit its audience. I would personally recommend its purchase only to those who delight in twice-told tales and thrice-told dogma.

C. E. FERGUSON

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**Price and Allocation Theory; Income and Employment Theory;
Related Empirical Studies; History of Economic Thought**

The Nature of Economic Thought. By G. L. S. SHACKLE. New York and Cambridge: Cambridge University Press, 1966. Pp. xiv, 322. \$9.50.

This is a collection of essays and reviews written by Professor Shackle between 1955 and 1964. Their topics vary considerably, but they are all distinguished by the author's pleasing style and frequently delightful turns of phrase and by his constant preoccupation not so much with the general "nature of economic thought" as with the philosophical riddle of how freedom of choice fits into a determinate world.

Most economists are prepared to liquidate the puzzle by granting that however strongly a man feels his freedom to choose among several possibilities of action, and however keenly he suffers all the agonies and ecstasies of conscious decision, from an outside point of view his choice must be seen as the determinate resultant of the characteristics of the chooser and the conditions under which he has to choose. Economists have indeed contributed to the philosophical understanding of the subjectivity of freedom by showing how all the economic subjects taken together are forced, by changes in their incomes, to choose freely to save just the amount that the investors are investing (while the investors are forced, by induced changes in their inventories and profit expectations, to choose freely to invest exactly as much as the savers are saving).

It may be impossible, or even demonstrably impossible, for anyone to know enough to be able to foretell with certainty an individual's choice, or indeed any future outcome, in our complicated universe. But the seeker for understanding must everywhere look for conditions which determine outcomes, and there is not much point in discussing whether those that will never be discovered *really* exist or not.

Shackle however refuses to dismiss freedom of choice as a subjective illusion. He insists that there *must* be some real freedom because he feels it. Furthermore if it is known with certainty that one course of action is definitely better than all the others there is again no real alternative and no real choice. Real choice is possible therefore only where there is uncertainty. Yet the traditional theory of probability provides no comfort for Shackle because it can

be applied only where the alternative possibilities, like the six sides of the die, are all known. Real freedom of choice involves the ability of the subject to *create* new courses of action (with new outcomes) which did not exist and certainly were not known before he invented them. A new world is continually being created which cannot be included in any previously existing closed set of possibilities and which is therefore not amenable to the probability calculus.

J. W. Dunne, once impaled on the same philosophical dilemma by a theory that dreams foretell the future and thus permit the future to be altered (by the avoidance of foretold calamities), was forced into a theory of a serial universe with infinitely manifold futures (*An Experiment with Time*, New York, 1927). Shackle seeks a less dramatic way out. Since men must act long before they can make rational calculations of the probabilities of all possibilities, he supposes that the decision maker considers only *possibilities* and focuses only on the best and the worst possible outcomes in choosing among alternative courses of action. This would seem no worse a simplification than to suppose that he decides on the basis of the actuarial value of each alternative, or even this together with some measure of the variance of the probability distribution; and it is definitely better than the assumption that he merely minimizes his maximum possible loss (i.e., with no regard to the possible gain). But Shackle recognizes that some possibilities seem more remote than others and can have little influence even if they involve extreme gains or losses. He therefore has the chooser *weigh* the possible gains or losses by the inverse of the "potential surprise"—the degree of surprise that the subject thinks he would feel if that outcome materialized. If a possibility is "hardly possible" and so would surprise him very much if it occurred he will give it very little weight, equating it to a smaller "standardized" gain or loss with zero potential surprise, being "perfectly possible."

This reviewer, having lost interest in the philosophical conundrum, is unable to distinguish between the "degree of potential non-surprise" and (subjective) probability, or to see why the sum of probabilities cannot always be made to sum to unity by adding the probability of "some other outcome" to those envisaged, or indeed why one need be concerned with such summation of probabilities. Consequently it seems to him that Shackle's troubles arise only from taking too seriously the apparent denial by certain probability theorists of the meaningfulness of subjective probability when they say that probability cannot be applied to *unique* events. Every actual decision constitutes a unique event and has a unique outcome—not a probable distribution of possible outcomes. (Even for an insurance company each issuance of a policy is a unique event and definitely will or definitely will not be followed by a claim.) What is not meaningful is the *frequency* of a unique event, but that is perhaps too obvious a statement on which to build a school of thought without confusing the issue by calling frequency "objective probability." Probability plain is meaningless when applied to *events*. Events don't have probabilities. They either happen or don't happen. Probability plain is meaningful only as a subjective feeling. It indicates the degree of a subject's ignorance of how things will turn out, and its "improbability" can very well be measured by "potential surprise." (This by no means questions the theory that potential surprise is

strongly influenced by knowledge of the *frequency* of such outcomes in similar situations.) Furthermore, the use of this measure or description of probability in no way necessitates any concern with the free will paradox.

Shackle's unorthodox terminology has in turn been taken too seriously by critics of his work and this has distracted economists from his positive contribution of interesting suggestions on the nature of some of the shortcuts that may be taken by men who have to make decisions.

The frequency model for explaining probability is not the only model that Shackle has taken too seriously. In one paper ("The Economist's Model of Man," p. 123, unfortunately directed to noneconomists), he takes too seriously the model of economic man in a perfectly competitive economy and is moved to some Veblen-like mocking of this oversimplification. (An oversimplification is after all only a simplification taken too seriously.) In another essay ("Resources and Demands," p. 189) he apparently takes too seriously the only too common economic illiteracy of social critics and goes to the other extreme of a Bastiat-like panygeric to the marvels of the price and market mechanism as a giant computer that gives everyone "his due" (p. 192, even while himself oversimplifying the description of the computation by supposing the computer to be fed on simple demand curves, i.e. neglecting the effects of the prices of other goods on the amount of a good demanded).

But now I am myself guilty of unjust oversimplification. The extravagances I have just mentioned are largely justified and corrected in their context. While there are many minor points one can pick a quarrel with, there are also some intriguing puzzles and many illuminating suggestions.

I was annoyed, for example, to find clear diagnoses of both cost inflation and of demand inflation followed by prescriptions for only the latter, or by some hints that suggest, if only very faintly, that a little inflation is as bad as or worse than a little unemployment or that unnecessary government spending is more harmful or more prevalent or more in need of criticism than unnecessary private spending.

I was puzzled by his enthusiasm for a metaphor, from Artilochus via Isaiah Berlin, "The fox knows many things but the hedgehog knows one big thing" (pp. 16-32, 297), and by his ability to see in the hedgehog "the seeker after one sole all-encompassing Principle of Nature" (p. 30). But perhaps that is because I have less faith in the existence of any such Principle of Nature than in the Postulate of the Scientist (that there is order in nature) which has enabled him to get to know "many things."

I was delighted by many flashes of insight and felicities of expression, e.g., by the definition of money as the "total amount of payments [that] could be made *simultaneously*" (p. 196) or the beautiful delineation of an example of the very oversimplification I have belabored in this review when he says of the "period of production" that "The Austrians were too profound in elaborating their theory and the English too fastidious in criticising it" (p. 289). An important and very useful part of the book is Shackle's survey "Recent Theories Concerning the Nature or Role of Interest" (pp. 225-81, from the *Economic Journal*, 1960) which is relatively free from the leitmotif of the implications of freedom of choice. The book concludes with three interesting book reviews

written with such typical charming gentleness that one almost fails to notice it when Shackle charges the author with missing the main point and flying off in the wrong direction. The review of Kaldor's two volumes of essays, a fascinating collage of sentimental memories of thirty years of economic discussions, ends with "What an item for inclusion in that list of 'books I would take to a desert island'!" I would not put *The Nature of Economic Thought* in that category but I unhesitatingly recommend it (to economists) as excellent evening reading for profit with pleasure.

ABBA P. LERNER

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A Keynesian Theory of Employment, Growth and Income Distribution. By SIDNEY WEINTRAUB. Philadelphia: Chilton Books, 1966. Pp. x, 147. \$7.95.

Much of recent growth theory is concerned with the following question: What must the path of the main economic variables through time be in order that the economy be in equilibrium at each moment of time? The answer, among other things, depends on initial conditions and so we may think of many equilibrium paths for an economy. Sometimes it can be shown that all such paths approach a particular one—usually the steady state path. One of the conditions of equilibrium is the fulfillment of expectations. It is clear that when we have discovered these questions we still have a long way to go before we can pretend to understand actual paths. In particular the actual behavior of effective demand through time as opposed to its behavior imposed by an equilibrium requirement needs attention. Similarly the present habit of taking "full employment" as one of the equilibrium conditions is limiting and certainly not "Keynesian."

Professor Weintraub's book is no doubt motivated by these deficiencies of present theory. It is my sad duty to report that he has not succeeded in remedying them. This is partly due to his quite imperfect command of what has already been achieved in this field, partly by his inability to distinguish identities from equations, and partly to the fact that, in the event, he too only considers equilibrium paths, except for some *obiter dicta*.

After a brief run through Harrod-Domar and "all that," Weintraub decides to investigate the theory of growth by means of schedules of aggregate monetary demand and supply. The schedules intersect at a moment of time and they shift through time. The monetary supply schedule is simply the money proceeds producers must expect in order to employ a certain amount of labor. It is equal to their production cost plus markup. For much of the analysis money wages are taken as fixed and the ratio of gross profits to wage payments assumed constant. The monetary demand schedule is made up of consumption demand (different propensities for wage earner and "others"), investment, and government. All systems are now at "go." Through time the intersection of the two schedules shifts. Evidently it can shift in a wide variety of ways. Relentlessly Weintraub pursues them all by means of an abundance of quasi-elasticities. If, for instance, as between two intersections money

proceeds have risen by a greater percentage than employment has, then the "Growth-Proceeds Elasticity" is greater than one. Since money wages are constant the truism that the share of wages declines follows—it is only another way of stating it. Then if we divide the equilibrium percentage increase in proceeds by the equilibrium increase in the value of capital we get the "capital-proceeds" elasticity. There are others. We can imagine umpteen combinations as well as the further amusing eventuality of these "elasticities" increasing or falling through time. At each stage the careful arithmetician will arrive at the correct answer—the economist may sleep awhile. For what is the point of this dreary catalogue of possibilities even though some are given economic names such as "capital-saving or capital-using," etc.? If the intersection points move in some particular fashion then they move in that fashion and we can leave it to the schoolboy to draw the arithmetical conclusions. What we want to know is: what makes the economy move from intersection point to intersection point, how do *relative* prices of inputs change, how are techniques chosen, how are expectations formed, what determines the innovations adopted, etc.? Weintraub allows only of passing references to some of these questions, and some he does not mean seriously, e.g., the suggestion that the "stability" problem here is like a cobweb"! The funny thing is that Weintraub knows what he is doing. Thus on noting the difficulties of analyzing technical progress in real economies he remarks: "Since our growth equation runs in terms of proceeds, investment ratios and capital-proceeds ratios, technical progress will not alter the truistic nature of these relations" (p. 119).

Although all sorts of other contingent matters are discussed, Weintraub continues his confusion between arithmetic and economic theory. I give only two instances. On relaxing the assumption that money wages are constant but retaining the assumption of a constant percentage markup on wage costs, he notes that if money wages rise by x per cent and employment and output remain constant, then money profits will rise by x per cent. He calls this "a conclusion of prime importance," and *deduces* that "money wage changes are probably the prime mover in profit changes" (p. 55)! Again starting with the identity: sales of consumer goods equal to purchases of consumer goods, he then implicitly assumes that purchases are equal to *ex ante* consumption outlays and on substitution into the identity arrives at what he calls an "equation which should be of major significance in price level theory" (p. 112)! One could continue almost indefinitely.

Weintraub occasionally makes general remarks on the nature of society. Here is one: "The distinctive feature of a capitalist economy is that wage earners are hired and paid a money wage" (p. 53). His references are largely to himself and to Mrs. Robinson whose preoccupation with the subtleties of economic choice or with the problems of capital valuation are however well beyond the author's reach. His interpretation of the meaning of $C_r \rightarrow 0$ in the Harrod equation is a classic in the misunderstanding of limits.

The book concludes with a chapter on optimum growth which largely says that since valuation of consumption at different moments of time depends on who is doing the valuing we cannot speak of "the" optimum growth rate.

There is no discussion either of efficient accumulation or of the necessary conditions for optimum accumulation under any given valuation.

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Churchill College, Cambridge

On the Theory and Measurement of Technical Change. By MURRAY BROWN. Cambridge: Cambridge University Press, 1966. Pp. xii, 214. \$8.50.

This study presents a theoretical analysis of technical change, a method for measuring neutral and non-neutral technical change, and an empirical application. Part I presents the theoretical analysis. The author is to be commended for bringing together: (1) a discussion of the concept of a technology; (2) a discussion of the characteristics of both the Cobb-Douglas and CES production functions; (3) an analysis and comparison of embodied and disembodied technical change; and (4) a particularly valuable discussion of short-run, long-run, and secular production functions, providing considerable insight into the dynamics of a changing technology. Unfortunately, this section, while of considerable interest, is unnecessarily vague and somewhat fuzzy, and is based on assumptions many of which are not made explicit.

Part II is concerned with the measurement of technical change, and builds on the theoretical work in Part I. The author reviews four methods that have been used: (1) the productivity ratio method, (2) the Solow method, (3) the Salter method, and (4) a method (used by Tinbergen) based on estimating Cobb-Douglas coefficients, assuming exponential, neutral technical change. The author then presents an ingenious method for measuring both neutral and nonneutral technical change. This method is based on the identification of technological epochs during which no non-neutral technical change occurs. Within each epoch, Cobb-Douglas coefficients and the ratio of neutral technical change are estimated.

Changes in the estimated coefficients from one epoch to another provide evidence on nonneutral technical change. More generally, interepochal differences in output are decomposed into (1) increase in inputs; (2) neutral technical change, and (3) nonneutral technical change.

There is some discussion also of the measurement of technical change in a CES world, but the subsequent empirical work is based on the Cobb-Douglas function.

The empirical application, presented in Part III, is based on the private domestic nonfarm sector of the U.S. economy for the period 1890-1960. Epochs are identified, and the parameters for each epoch are estimated. The principal results can be stated: (1) There were increasing returns during the first epoch (1890-1906), and decreasing returns thereafter. (2) There was a large increase in the use of labor-using techniques between 1939 and 1948. (3) There is evidence of large-scale structural change over the period 1890-1960. (4) The importance of technical change in raising output is a recent phenomenon: apparently the contribution of technical change increased sharply toward the end of the period studied.

There is an interesting discussion of the effect on employment of (1) techni-

cal change, (2) substitution of capital for labor, and (3) increases in output. The author notes that labor displaced by technical change has been absorbed by rising output. But this does not tell us a great deal about the underlying forces, which can be studied only at a much less aggregative level. Technical change in the widget industry may reduce the labor cost of producing a given number of widgets. If the demand for widgets is sufficiently inelastic, the result will be a reduction in employment in the widget industry. If, on the other hand, the widget demand curve is sufficiently elastic, then employment in the widget industry will rise. The net effect will depend on the biasedness of the technical change and on the elasticity of demand for widgets. For economy-wide technical change, the employment effect will depend on the biasedness and on the distribution among industries with differing elasticities of demand. Brown's discussion of this point is at times tautological, at times mystical, and does not appear to get to the heart of the matter.

Murray Brown has brought together a great deal of relevant material on technical change. The empirical work is of particular interest. However, the opaqueness of the theoretical sections detracts somewhat from the value of the book.

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Introduction to Price Theory. By MICHA GISSER. Scranton, Pa.: International Textbook Co., 1966. Pp. ix, 325. \$7.50.

The Market System. By ROBERT H. HAVEMAN AND KENYON A. KNOPF. New York: John Wiley & Sons, 1966. Pp. xiv, 223. \$1.95.

Introduction to Price Theory by Micha Gisser and *The Market System* by Robert H. Haveman and Kenyon A. Knopf are both microeconomic theory textbooks, but despite their similar content, they are intended for different readers. The former is appropriate for an intermediate level course while the latter is one volume of an introduction to economics series.

Introduction to Price Theory examines the conventional price theory subjects (demand and utility theory, theory of production, perfect competition, theory of supply, monopoly and imperfect competition, the labor market, and distribution theory), and, in addition, chapters entitled "Indirect Taxes" and "Price Theory as Applied to Agriculture" (Chs. 10 and 11, respectively) deal with specific applications. Gisser states in the preface that ". . . every effort has been made to present the basic elements of price theory by using simple tools such as tables and diagrams" (p. vii). He has been exceptionally successful in achieving this goal without sacrificing rigor. His lucid presentation is enhanced by including many numerical examples and by supplementing most chapters with appendices which contain the more difficult analytical concepts. There are instructive exercises at the end of each chapter, and the answers to numerical and "true-false-uncertain" questions are provided. Another excellent feature is the inclusion of a concise mathematical appendix (pp. 293-321) which will be appreciated by the more mathematically oriented students and teachers.

Gisser concludes with a somewhat disappointing chapter entitled "Capital

and Interest" (Ch. 15). As he states, "There are many approaches to the theory of capital. Since we do not want to provide the student with an encyclopedia in price theory, we had to select only one approach. We shall follow the theory of capital as formulated by Frank H. Knight and Milton Friedman" (p. 270). He therefore distinguishes human factors of production from "tangible capital" or all nonhuman factors of production. One unit of "*tangible capital*" is defined as a "complex of physical capital" capable of producing a net income flow of one dollar per unit time (pp. 272, 274). While this approach to capital theory is useful for some purposes, it does not, in my opinion, provide the student with an adequate introduction to economic models containing many *physically different, heterogenous* capital goods. For example, there is no reference to own-rates of return, which play an important role in models of economic growth and in the problem of efficient capital accumulation.

The book suffers from two deficiencies; the entire subject of general equilibrium is omitted, and there is only a superficial treatment of stability (pp. 143-45) in which no mention is made of the Walrasian, Marshallian, or Hicksian stability definitions.

Nevertheless, *Introduction to Price Theory* merits consideration by anyone teaching intermediate microeconomic theory.

In seven chapters *The Market System* provides an introductory level discussion of the economic concept of scarcity, supply and demand, consumption theory, supply and theory of the firm, the functioning of a competitive price system, imperfect competition, and welfare economics. Knopf, also editor of the series, explains the reason for an introductory text devoted exclusively to microeconomics:

Teachers of introductory economics seem to agree on the impracticality of presenting a comprehensive survey of economics to freshmen or sophomores. Many of them believe there is a need for some alternative which provides a solid core of principles while permitting an instructor to introduce a select set of problems and applied ideas. This series attempts to fill that need and also to give the layman a set of self-contained books that he can absorb with interest and profit, without assistance.

By offering greater flexibility in the choice of topics for study, these books represent a more realistic and reasonable approach to teaching economics than most of the large, catchall textbooks. With separate volumes and different authors for each topic, the instructor is not tied to a single track as in the omnibus introductory economics text (p. v.).

The Market System is consistent with this teaching philosophy; it offers a concise introduction to microeconomic theory in a comprehensible manner. It is well organized, and theoretical ideas such as indifference curves and marginal cost are carefully explained. A conclusion or summary is given at the end of each chapter, and numerical examples, tables, and graphs are instructively employed, but their use is not excessive. The book exhausts the list of appropriate introductory microeconomic topics with one exception; capital theory is completely neglected (present discounted value is not even mentioned), although perhaps this subject is scheduled for inclusion in another volume of the series.

"The Models and the Real Economy: Problems and Prospects," the concluding chapter (Ch. 7), warrants special attention. It is an unbiased and comprehensive introduction to the complex economic and ethical issues concerning efficient allocation of resources and social welfare both in a perfect competition model and in the real world. The existence of market power over supply and demand, frictions and uncertainties, immobilities, indivisibilities, and nonmarketable public goods are all discussed, and it is observed that perfect competition may result in an unethical distribution of income. Thus the authors make an excellent case for their conclusion that "Because of these qualities, the price system, by itself, cannot provide for maximum social welfare. It must be supplemented with other systems of social choice" (p. 217). This chapter makes excellent supplementary reading for *all* students of introductory economics.

EDWIN BURMEISTER

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National Income and Employment Analysis. By ARNOLD COLLERY. New York: John Wiley & Sons, Inc., 1965. Pp. xiv, 154. Paper, \$1.95.

Unlike many recent publications in similar formats, the purpose of this book is to supplant rather than supplement conventional texts. Many will find it offers a worthwhile pedagogical improvement over the macro sections of the integrated hardbound texts designed for elementary economics courses. The first five chapters develop the simple Keynesian model and its implications for monetary and fiscal policy. The final two chapters explain the possible undesirable side effects of the Keynesian "medicine" for full employment: price instability and balance-of-payments problems.

By intent, the scope of the book is, at most, equal to analogous sections in standard texts. Some instructors may therefore indict the book for sins of omission, although these should generally be considered minor ones which are easily remediable in lecture. Collery's explicit intention was to strip away some of the less important techniques and details which, he feels, detract from a student's understanding and retention of the more crucial lessons to be drawn from macro theory. This is not to suggest, however, that the material which survived the pruning process is necessarily the easiest nor that the discussion does not attain a sophistication which is quite commendable at this level.

Collery's step-by-step elaboration of Keynesian national income analysis is, for the most part, a model of clarity and common-sense logic. For instance, his use of simple examples results in a strikingly clear and effective explanation of the "why" of the fundamental Keynesian equation. By consistently exercising care to present the student with the common-sense rationale of each succeeding proposition, he makes light work of frequently sticky points such as the passage from tautology to equilibrium equation wherein one must distinguish between the *ex post* and *ex ante* equality of savings and investment. A set of questions is appended to each major proposition developed in the text. These should be extremely useful for pedagogical purposes since they eschew

"cleverness" in favor of a direct but thorough illustration of the principle involved.

The exposition is so well formulated that little or no lecture time ought to be wasted in clearing up difficulties for the average student. With the essentials disposed of, an instructor would be relatively free to fill out the course according to his personal disposition, with institutional, theoretical, or practical refinements which the book itself does not seek to provide. This is a high recommendation for a book.

Collery does leave some points to quibble about, of course. His relatively muddy explanation of the balanced budget multiplier might easily have been improved by an algebraic proof similar to those used in other sections. The volume's simplistic statement of the "laws of consumption" will never cause a student to appreciate just what a disappointingly elusive animal "the" consumption function proves to be when we attempt to give it empirical content. The discussion of labor productivity in the inflation chapter reinforces rather than corrects the error of defining this concept in average rather than marginal terms. Still, such peccadilloes do not seriously mar a book which stands up conspicuously well as judged by the standard of its competition.

In evaluating any book of this type for classroom use, it is essential not to lose sight of its basic role as part of a "package." Collery's book represents an improvement on the corresponding segment of a hardbound text, but its usefulness is conditioned by the acceptability of similar material which supplants the remainder of the orthodox Principles text. Even at its modest price, it would be difficult to justify the book's adoption for *joint* use with a standard text since it does not supplement such texts in any meaningful sense.

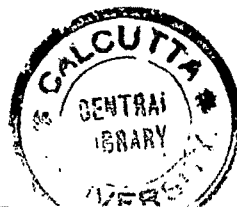
CHARLES J. GOETZ

University of Illinois

Economic Analysis of the Firm: Theory and Practice. By IVORY L. LYONS AND MANUEL ZYMELMAN. New York and London: Pitman Publishing Corp., 1966. Pp. xii, 422. \$8.75.

Teaching microeconomics to undergraduates in business can be a most frustrating experience. Many faculty members in a typical business school, as well as the students, want a course which is practical and filled with examples. Economists, on the other hand, often find little advantage in offering such a course and end up by offering a course in microeconomic theory. There have been a number of attempts to write a text for such a course, but there have been relatively few that have satisfied either group.

The authors of the present work have attempted to bridge the gap between theory and practice by first explaining the theory and then using problems or cases to permit the student to extend the analysis beyond that in the text. The cases include simple problems in computing marginal revenue and marginal cost, antitrust cases, the Kennedy-Steel controversy, and many others. The theory which is included is fairly standard with the exception of that of the last chapters where the authors take up investments, forecasting, and simple statistical techniques.



Unfortunately, however, the errors committed by the authors in their exposition of economic theory overwhelm the rest of the book. Such errors as "AFC is the slope of a line drawn from the origin to a point on TC" (p. 59); the industry demand curve and the firm demand curve have the same slope under pure competition for it is just the difference in scale which makes them appear different (p. 125-26); a rising average product curve when the marginal product curve is below the average (p. 271); the assertion that the monopolist can set either price or quantity but not both (p. 146); crediting increasing returns to scale to improved production methods (p. 66); the explanation of profit maximization (p. 82); and many, many others all serve to discredit the book.

But it is not only a matter of errors. In many places, the exposition is so obscure that it is difficult to determine what the authors have in mind (for example, the discussion of long- and short-run costs on pages 62-63); diagrams are not well placed in the texts and many diagrams are such a maze of lines that they are difficult to understand; many essential explanations are either omitted or superficial; and though the authors have included a mathematical appendix, there is no attempt to use calculus at points where it might well be used; and what mathematics is used is rather laborious.

In short, this book has nothing to recommend its use as a text.

JOHN M. KUHLMAN

University of Missouri

Economic History; Economic Development; National Economies

Growth and Welfare in the American Past—A New Economic History. By DOUGLASS C. NORTH. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xiv, 210. \$5.95; paper, \$2.95.

"A primary objective of this book," Professor North tells us, "is to present a nontechnical appraisal of America's economic experience, weaving together the summary results of our new quantitative knowledge of America's past with reappraisal of a number of basic issues and interpretations." What follows is the first attempt to apply the "new economic history" to U.S. development as a whole, from the Colonial period through 1945-65. The fourteen chapters include "Theory, Statistics, and History" (Ch. 1), "An Overview of the Development of the U.S. Economy" (Ch. 2), and twelve more arranged in a general chronological order grouped about specific, and often conventional, topics. They are concentrated in the period up to World War I; only the last two chapters, and small parts of the second, deal with the post-1914 years.

As North has defined it elsewhere, the new economic history is aimed at a reconstruction of the U.S. experience, "firmly grounded in statistical data . . . [which will provide] the quantitative information necessary to outline the contours and patterns of American economic change from the past." The ultimate product will be a "cumulating number of analytical insights into the

process of past economic change which are the essential prerequisite to evolving sound theoretical propositions about long-run economic development." (This *Review*, March, 1963, 53, 129-30.)

The school of economic history has been subjected to criticism from several quarters in recent years. While some of the complaints have merit, others have been unfair and irrelevant. To help resolve at least some of these doubts, it would seem all the more imperative for new economic historians to build analytical frameworks upon which the elements of their work can be grafted, and given meaning. No one has been more insistent on this point than North himself; *The Economic Growth of the United States, 1790-1860* sets a worthy example. Disappointment is thus compounded when his latest book suffers from lack of a clear theoretical structure—it tries to blend several approaches principally, but not exclusively, quantitative.

Economic theory and statistics can be applied to history in two basic ways: (1) by study of the macro- and microeconomic variables constituting the dynamic forces in economic growth, their trends through time, and the interactions among them; and (2) through use of economic theory to analyze more familiar historical topics, events, and institutions. Both approaches are covered by the theoretical discussions in Chapter 1 (pp. 3-11, 12-14). Someday it may prove possible to unify the two around the axis of quantitative measurement and inference, but North's present effort does not appear to result in a happy marriage.

The body of the first approach is tightly wedged into Chapter 2 (the "overview"), in which all bases are touched: output and output per capita, population and immigration, labor force composition, the rural-urban shift, transport costs, banking institutions, foreign investment, internal migration, business cycles, and corporate structure. The second approach is found throughout the book; it tends to develop into a pattern of confrontations with disparate issues, many of which are constructs of the more traditional historians. The overall effect is noticeably uneven.

This book is meant for the student, the uninitiated, anyone who might want to introduce himself to the new economic history. Yet I wonder what impressions such a reader would get. The essence of the quantitative approach to U.S. growth—the role of the market and the parametric function of its price system—would probably escape him in the woods of Chapter 2; and other sections of the book might too readily lead him to conclude that economic history involves employing deft statistical techniques to answer, more or less, a series of propositions that historians have posed for us down through the years. This latter fear is reinforced by stretches of the book that turn into accounts of intramural debates among economic historians. By no means do I downgrade the importance of these dialogues; I simply question whether a nontechnical appraisal is the place for them and whether much is accomplished by paying them respect in such truncated form.

These are some of the problems created by *Growth and Welfare*, and they make it appear that the audience for which it is intended will benefit less than North had hoped. More likely to benefit, though, will be the reader with a cer-

tain amount of knowledge about the new economic history before taking this book in hand.

Further on the positive side, this book can serve as a workable introduction to American economic development, if tied to a rigorous outline of this subject and supplemented by selected material (a good start is the bibliography, pp. 193-98). The tables and charts offer a core of quantitative matter that could be put to profitable use in its own right. Similarly, Chapters 3, 6, 9, and 11 are, by themselves, valuable summaries of the state of present knowledge and research in interregional trading patterns, transport costs, and prices and incomes in the agricultural sector from 1865 through 1914. Used so as to capitalize on these stronger features, *Growth and Welfare* will provide a guide to the direction of quantitative economic history and the tools it employs, as well as to some of its accomplishments and the frontiers it has opened up.

RICHARD B. DU BOFF

Bryn Mawr College

Output, Employment, and Productivity in the United States after 1800.

National Bureau of Economic Research Studies in Income and Wealth,
Vol. 30. New York: Columbia University Press, 1966. Pp. xiv, 660.
\$12.50.

Volume 30, as this book undoubtedly will be known, joins its earlier cousin, Volume 24,¹ in the indispensable library of the U.S. economic historian. These volumes add significantly to our store of quantitative knowledge about the growth of the U.S. economy and—even more importantly—to our understanding of the nature of the data. All of the series are accompanied by detailed descriptions of their derivation, comparisons with other estimates, and suggestions as to their reliability. Historical data, as Easterlin says in a comment (p. 89), “though useful, are imperfect and mortal.” The format of the volume encourages the adoption of this realistic view.

The first third of the volume is devoted to aggregate concerns: national product estimates by Gallman revising and extending Kuznets’ data, new price deflators in fine detail by Brady, and new labor force estimates based more closely on population census data than other estimates by Lebergott. The investigators have endeavored to link their series to those available for later years, an effort that is commendable but burdened with certain costs. Gallman’s data link with Kuznets’, but the link must necessarily be imperfect since his constant price estimates are in 1860 prices while Kuznets’ are in 1929 prices. Lebergott links his series to the current series of the Bureau of Labor Statistics; they therefore differ from most earlier series which are based on the census. These difficulties are recognized and discussed by the authors.

They may not even properly be described as difficulties. In the introduction, Brady asserts that such differences give rise to opportunities for “reconciliations” of different estimates; the result being new knowledge about the

¹ *Trends in the American Economy in the Nineteenth Century*, N.B.E.R. Studies in Income and Wealth, Vol. 24, Princeton 1960.

changing interrelationships in the economy. For example, in a closely related work Lebergott found that real wages did not rise between 1850 and 1880.² In the volume at hand (p. 26), Gallman finds that real per capita income in 1874-83 was 50 per cent larger than in 1844-53. Do these estimates imply a massive redistribution of income away from labor? This would be consistent with Gallman's discovery of an important rise in the share of national product devoted to capital formation at this time (pp. 11-13), but does this picture of the economy fit with Lebergott's further finding that the share of labor in agriculture stayed constant at about 50 per cent for these years (p. 119)? Here is an unresolved problem of interest to any student of economic growth.

The next half of the volume contains papers presenting data on particular sectors of the economy. The papers on major metal mining, petroleum, and steam power are based on sources believed to have reported on the entire industry in question—although they all extrapolate from the basic data at points. The remaining papers are based on sources containing information about only part of the industries in question. With the solitary exception of McDougall's fine paper on two machine-tool firms, all of these papers use their partial information to extrapolate series for the industry as a whole.

The extrapolations, of course, are only as reliable as the assumptions used in their derivation. Davis and Stettler derive a series for total production of the cotton textile industry on the basis of a sample of firms selected on the basis of the availability of data (p. 220). A detailed comparison of the resultant series with the one derived by Robert Zevin from the state and federal census returns seems warranted as a check on the procedure.³ Eliasberg inflates various census data on coal mining on the basis of Eavenson's adjustment of the census data on output, a procedure that again assumes the sample observed to be typical of the industry.

Two other papers require assumptions of a stronger sort. Robertson derived a series for the production of metalworking machinery on the basis of data for metal-cutting machinery after 1900 and on the basis of a shifting, *ad hoc* sample before then. As Robertson makes clear, the burden of incomplete data is compounded by confusion in the definition of the industry, making the result doubly problematical. Gottlieb derived a series for the value of building in Ohio on the basis of property assessment data. As David points out in his comment, this requires strong assumptions about the nature of assessment data, assumptions which Gottlieb's references to the public spirit of Ohioans and the pressures for assessed values to conform to market values do not completely justify. Investigators must exercise considerable care in making use of these articles.

The final two essays in the volume present new data on productivity change in two important sectors of the economy and—in contrast to the bulk of the volume—go on to attempt to explain the pattern their data reveal. Parker and Klein find that mechanization was responsible for over half the increased pro-

² S. Lebergott, *Manpower in Economic Growth*, New York 1964, p. 154.

³ "The Growth of Cotton Textile Production After 1815," in Fogel and Engerman, eds., *The Reinterpretation of American Economic History* (in preparation).

ductivity in the cultivation of wheat, oats and corn in the late nineteenth century, while almost all the remainder can be accounted for by the effects of the shifting location of agriculture and the interaction of this shift with the new techniques. Fishlow finds that steel rails and heavier locomotives were important in raising railroad productivity, but that air brakes and automatic couplers were not; approximately half of the gain in productivity in railroads remains unexplained, however, when account is taken of these specific innovations. Both essays are characterized by impressive erudition and attention to alternate sources of data; it is unlikely that the correction of a few remaining defects (such as Fishlow's comparison of a chain-linked index of output with a Paasche index of inputs) would change the conclusions.

This volume, therefore, is a combination of reference work, introduction to some of the methodology of the "new" economic history, and monographic work on a few sectors of the economy. If the price for this diversity is a regrettable lack of organization, it only emphasizes the need for a new synthetic work on the economic history of our country.

PETER TEMIN

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Development Planning. By W. ARTHUR LEWIS. New York: Harper & Row, 1966. Pp. 278. \$4.95.

Professor Lewis has written this small book on development planning "an an introduction . . . for people learning the elements of the subject, and not as a technical treatise for experts." After a short introductory chapter, in about 120 pages he discusses "Plan Strategy"; in a little less than 100, "The Arithmetic of Planning"; and in 30, "The Planning Process." There is a selective bibliography at the end of each chapter.

Under the heading of "Plan Strategy," he discusses the evaluation of projects in view of interrelationships among industries; optimum policy concerning foreign trade at successive stages of development; the problem of capital intensity; the considerations that ought to affect the regional balance of a development program; causes and remedies of unemployment during development; optimum income distribution policies; pressures on public expenditures, and appropriate principles of public expenditure, taxation, and saving policies; effects of inflation, and its control; and methods and costs of obtaining foreign aid. But this listing of topics gives little idea of the complexity of subject matter he manages to cover in brief compass. Under each topic, discussion of amazing richness is packed into between eight and eighteen pages of terse discussion.

Here and throughout the book, the discussion mixes theoretical analysis with rules of thumb derived from the author's experience. Thus: "Proposals that the amount invested should grow by more than 10 per cent per annum should normally be resisted" (since capacity of the construction industry cannot be increased faster, p. 157). And: (Except in the few countries where per capita is growing faster than 2 per cent per annum) "attempts at reducing the share of consumption by more than a half of 1 per cent per annum are likely to be frustrated by strikes, riots, inflation, or political ac-

tion" (p. 164). Lewis's least quality is mental uncertainty: he is entirely certain of his answers to the questions he poses. And, indeed, in this reviewer's judgment his answers are usually judicious. Occasionally, however, they are too pat; this is true, for example, of his explanation of the postwar flight from the countryside to the cities, and his prescription of methods to cause farmers to save.

Chapter 3, on the arithmetic of planning, outlines a procedure for arriving at a development plan. Lewis discusses successively, with arithmetical illustrations, the determinants of the rate of growth and then a method for the first approximation estimation of a feasible rate of growth, of whether the necessary financial resources will be available, and of the balance between demand and supply of commodities. (Inflation, Lewis states, is caused by excess demand for commodities. The demand for services is not directly relevant.) He then indicates how a balanced supply of the output of individual industries can be estimated by means of input-output analysis, and how the optimum allocation of production can be estimated by means of linear programming. (He discusses linear programming for pedagogical purposes, noting that the data necessary to use it will not be available.) Finally, he indicates how the relevant capital, manpower, and government budgets should be planned. All this is presented with great lucidity. It is a little disconcerting to have him note in two sentences, at the end rather than the beginning of this rather elaborate discussion, that if the "engine of growth" is import substitution, the input-output matrix is unnecessary, and if expansion of exports, irrelevant.

The final chapter presents an unexceptionable discussion of the optimum location of the planning agency and optimum planning procedures.

The description above of the contents of the book suggests its virtues, which are many. To this reviewer it seems subject to the following criticisms:

1. At least one bit of theorizing seems shaky. Lewis suggests that there are five stages of foreign trade in development (not the conventional stages of the balance of payments). He states that they are not historical states, by which he means that they do not form an inevitable succession. In the second stage, import substitution in manufacturing is exhausted. Structural inflation then occurs as the third stage, if investment in industry goes forward further without a breakthrough in agricultural production to increase the domestic supply of agricultural products. However, since by his prior reasoning there would at this point be little market for added industrial products, and since improvement in techniques alone would hardly involve an increased rate of investment, the source of the inflation-causing industrial investment is difficult to understand. The model brings the author to the conclusion, which seems remarkable to this reviewer, that Chile advanced in manufacturing early enough so that she exhausted import substitution possibilities before Brazil.

2. Except for top officials, who sometimes exhibit perverse tendencies, there are no men in the book, only cardboard cutouts. For example, though the reader is told how to plan to meet requirements for skilled, technical, and professional manpower, he is given no intimation that the success of the plan may also depend on the presence of a sufficient number of innovational entre-

preneurs. "In practice," Lewis states (p. 159), "those who invest most will also improve technology fastest," but it does not occur to him that the correlation holds only because investment is high where there is a great deal of innovational energy, and that increasing the rate of investment where the innovational drive is lacking will not yield the technical advance.

3. It is not unrelated that the author manifests little trust in the market. The logic of planning seems to have captured him as he wrote, and at times one gets the impression that without a plan no economic growth could occur anywhere. He suggests that in small countries coordination among the chief public and private decision makers can be achieved informally, but "in larger countries, like Britain or France or the United States, this argument for models is much stronger" (p. 17). One wonders whether for the moment he has forgotten reality, or whether he thinks planning *via* models would accelerate the rate of growth in Britain and the United States. As these examples illustrate, the discussion is somewhat "textbookish." A few of the recommendations seem, what Lewis notably is not, naive.

Nevertheless, there is wisdom in the book for a variety of persons interested in development planning.

EVERETT E. HAGEN

Massachusetts Institute of Technology

Economic Development and Growth. By ROBERT E. BALDWIN. New York: John Wiley & Sons, Inc., 1966. Pp. xi, 133. \$1.95.

K. A. Knopf, the editor of the Introduction to Economics Series, states that "underlying the series is the pedagogical premise that students should be introduced to economics by learning how economists think about economic problems" (p. v). Baldwin's contribution to the series serves admirably as an introduction not only to how economists think about the problems of economic development, but also to the complexities of the problems themselves.

Baldwin's presentation is organized around three general questions: "(1) What is the nature of the growth problem facing the less developed countries? (2) What are the main theories concerning the key relationships that determine a country's rate and pattern of development? (3) What are the major policy issues with which less developed countries must currently deal, and what is the best way of handling these issues?" (pp. vi-vii). The first question is discussed within the context of statistical comparisons of per capita incomes, sectoral compositions, and relative factor intensities among countries which represent widely different levels of economic development. In addition to suggesting the problems which less developed economies face, Baldwin's qualifications in respect to the comparisons provide for the beginning student valuable illustrations of the care with which a responsible economist must interpret data.

The second question is discussed within the framework of the classical, neo-classical, Marxian, Schumpeterian, and post-Keynesian theories of economic growth and the major post World War II contributions to the understanding of economic growth. The concise presentations and evaluations of the various theories are generally excellent. Some of the terminology (e.g., "liquidity

“trap,” “input-output,” “multiplier process”) and the discussion of the Harrod-Domar model, however, will require elaboration by the instructor before most beginning readers are able to understand completely the presentation.

The third question is discussed within the framework of the major policy controversies of recent years: the “Big-Push” versus the selective approach, the role of government and the role of planning, emphasis on agriculture or on manufacturing or on social overhead capital, and the problems of financing development. The discussions illustrate both the usefulness and the limitations of current development theory and of currently available empirical studies in evaluating policy alternatives.

It is virtually impossible for a text of the length of Baldwin’s to present an introduction to all the important aspects of how economists think about the problems of economic development. Shortcomings in Baldwin’s presentation include the failure to discuss explicitly the insights which historical studies of successful and unsuccessful development attempts have to offer to the less developed economies. Most instructors who assign this book undoubtedly will want to provide supplementary historical and current case studies. In most respects, however, Baldwin’s coverage is well balanced and his presentation is excellent. In the opinion of the reviewer, no other available text will better serve as an introduction to the complex problems of the less developed economies, and to how economists currently think about them.

JERE R. BEHRMAN

The University of Pennsylvania

Foreign Investment and Economic Development in China 1840-1937. By CHI-MING HOU. Cambridge: Harvard University Press, 1965. Pp. xiii, 306. \$8.95.

An important problem in forming a balanced judgment about the nature of postwar economic development in China is how to fit the Communist period into its historical context. Research in the West has concentrated so much on the former that we still do not have a clear enough picture of the state of China in the period from the end of the nineteenth century up to 1949. In viewing their own economic history, the Chinese argue that foreign political and economic domination damaged her traditional economy and retarded her overall development. Only when the Communist government finally evicted the foreigners and unified China in 1949, they argue, did modernization really begin. A basic aim of Chi-ming Hou’s book is to assess the validity of this claim.

He estimates that total foreign capital investment in China, at constant prices, rose by about 90 per cent in 1902-1914 and by 20 per cent in 1914-1931. Government borrowing abroad (which declined as a percentage, over time) was mainly used to finance the cost of civil disturbances, the heavy indemnity payments owed by China, and the construction of railways, while direct foreign investment in China was largely associated with trade. Very little went into agriculture. Hou argues that foreign capital brought several economic benefits to China and his main point here is that it played a “significant role” in creating the small modern sector which grew up between

the end of the nineteenth century and 1937. The activities of foreign enterprises stimulated the national consciousness of the Chinese and led them to take modern industrial development seriously; they created a favorable environment for profit-making, as well as providing a valuable training ground for the Chinese to acquire new skills and modern technology. Most of China's social overhead capital was financed by foreign investment. In 1933 foreign firms accounted for over 50 per cent of the total number in sawmilling, ship-building, water, gas and electricity, and during the 1930s, generally, they accounted for 40 per cent of cotton spindles and 70 per cent of looms.

Apart from being instrumental in the growth of a modern sector, Hou further argues that foreign investment did not retard or destroy Chinese economic activities. On the contrary, the Chinese share in the modern sector remained remarkably stable, despite the unequal treaties which China had to endure. In their turn the Chinese used boycotts effectively to restrict sales of foreign goods and in some cases maintained their position by dividing the market. Hou presents evidence that traditional supplies of coal, shipping, and cotton-weaving may have actually grown throughout the period, and he attributes the survival of the traditional sector to its lower costs and prices: modern (mainly foreign) firms, being capital intensive, operated with higher costs and prices than the more labor intensive, traditional enterprises and thus failed to capture the Chinese market. Finally, the author finds that, unlike the experience of some poor countries subject to foreign domination, China's trade became more diversified over time and not lopsided, mainly because so little foreign capital was invested in primary production. The deterioration in the commodity terms of trade which took place had no serious effects and could not, in any case, be blamed entirely on foreign activities.

Although it discusses under a single cover many important topics and provides an impressive amount of statistical data, this book is not a very original contribution. Professor Hou has, it is true, produced some new material relating to foreign investment but our understanding of its impact and role in the Chinese economy has been taken very little beyond the point established by C. F. Remer's researches. Similarly, in discussing various aspects of China's modernization, he has drawn extensively on the works of scholars such as A. Feuerwerker, K. C. Liu, T. C. Liu, Cheng Yu-kwei and Ou Pao-san but has added little to their findings. And while he has given weight to the point (already well established) that foreign enterprise was crucial in creating a modern sector, Hou's analysis of the effect of foreign investment on the traditional sector is too general to be acceptable. Despite the author's statement in the introduction (p. 6) that he intends to deal with the effect of foreign investment on agriculture he neglects this topic almost entirely when, in fact, it ought to have been a major subject. One of the few references to agriculture comes on page 175 when the author mentions the doubling of the cotton sown area between 1904-9 and 1929-33 but he fails to relate this to foreign investment. He does, however, devote considerable attention to handicrafts, but by treating China as a single unit ignores the crucial question: what happened to traditional economic activity in the vicinity of those few (mainly coastal) areas of China where modern industry did develop?

For the economist primarily interested in China's development, therefore, this is a disappointing and inconclusive volume and students of economic development in general will find it a trifle too specialized to be of great interest.

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The Economics of Development in Small Countries—with Special Reference to the Caribbean. By WILLIAM B. DEMAS. Montreal: McGill University Press, 1965. Pp. xv, 150. \$4.50.

This book incorporates the four Callard Lectures that Mr. Demas delivered in 1964 at McGill University in connection with the recently established Centre for Developing-Area Studies. It presages a regular flow of monographs on development from the Centre. And the quality of this work augurs that the forthcoming flow could potentially represent a high capital value for the profession.

Two of the four chapters of the book refer specifically to the Caribbean—defined to include the "British Caribbean" of the Windward Islands, the Leeward Islands, Barbados and British Guiana, as well as the two independent countries of Jamaica and Trinidad and Tobago. Chapter 3 describes the characteristics of the Caribbean economies while Chapter 4 gives some meaningful insights into the process and the problems of economic planning in the area. Chapters 1 and 2, on the other hand, refer to development strategy in small countries in general—defined as the countries with a population of 10 million or less and with usable land area of 10 to 20 thousand square miles or less.

In many respects the Caribbean countries resemble their Latin American neighbors. They have high rates of population growth and high unemployment. Their resource base, apart from tourist attractions and agricultural land, includes extractive minerals—oil and natural gas in Trinidad and Tobago, bauxite in Jamaica and British Guiana. With the contribution of primary or resource exports, the economies of the Caribbean are "export trade-dominated"; the ratio of exports to GDP ranges from 25 per cent for Jamaica to just over 50 per cent for British Guiana and Trinidad and Tobago. This phenomenon—which is fairly typical of most Latin American countries—contributes to a greater degree of "external dependence" for the countries. And it has attracted both the scrutiny and strictures of the "structuralist" school of economic development (Raul Prebisch, ECLA, etc.). The rather atypical feature of the Caribbean economies, again in comparison with other Latin American countries, is monetary stability, high per capita incomes (\$1058 and \$706 for Trinidad and Tobago and for Jamaica, respectively, in 1962), and high annual rates of growth in GDP (around 8 per cent per annum in the 1950's). With the standards of the modal underdeveloped country the Caribbean economies seem to be doing rather well—and they are expected to continue doing so despite the fact that their annual rate of growth is projected to 5 per cent for the balance of the '60's due to the nonrecurrence of the rapid expansion of the mineral-exporting sectors.

On the basis of the foregoing general characteristics of the area, one could make an a priori case for applying in the Caribbean one or a combination of

several of the development strategies that are available in our professional kit of tools—the strategies of “do nothing” and “do more of the same” being distinctly among the alternatives. Instead, the author further specifies the model by emphasizing as key parameters the size of the country and the degree of “openness” of the economy; and he proposes as unique development strategy the expansion of exports of manufactures. He advocates the same strategy for all small underdeveloped countries. This reviewer, while willing to go along with the export expansion strategy in the special case of the Caribbean, fails to see why such a policy has universal validity for all small underdeveloped countries. It is the special institutional openness of the economy that recommends this strategy for the Caribbean—not the size of the country.

Small countries are likely to have highly skewed resources in comparison to the large ones. Their domestic interindustry transactions are more limited and they leave more gaps unfilled—which limits both the volume and the variety of production for the local market. Due to relatively greater specialization in a small country the domestic structure of production differs quite sharply from the domestic composition of final demand. Furthermore, economies of scale cannot be fully achieved by producing for the local market only. From this analysis the author draws two conclusions: smaller countries have greater “dependence” on foreign trade; and in the attempt to take advantage of economies of scale, export of manufactures becomes the leading sector of the economy (pp. 47-48).

Recent empirical work (e.g., Hollis Chenery, who is recognized by the author) attributes special emphasis to the role of import substitution in the process of industrialization of developing countries. It is true that small countries, because of the limited size of their market, might have to forego the benefits of economies of scale in their import-substituting industries. On the other hand, there is no reason why such industries could not become viable through tariff protection. Invisibles (e.g., tourism) is another export industry that might lend propulsion to economic development—and is immune to any effects related to the size of the country. And so is agriculture, the role of which as an “engine of growth” has been recently emphasized by many writers (e.g., T. W. Schultz, S. Kuznets, W. H. Nicholls, W. F. Owen, etc.). It appears that in most countries there is ample room for agricultural development—and this might hold especially true in the Caribbean. (Agricultural products and raw materials seem to make up from 40 to 60 per cent of the total value of imports in the Caribbean countries. And agricultural employment is 39 per cent and 21 per cent of the total in Jamaica and Trinidad and Tobago, respectively, while the sector contributes only 12 to 13 per cent of the GDP of each country.) The author does not entirely dismiss the possibility that import substitution, export of indivisibles and agriculture might play an important role in the development of smaller countries. Yet he places the main emphasis on exports of manufactures on the grounds that this industry has stronger linkage effects for transforming the whole structure of an underdeveloped economy (pp. 60, 132-33). This may or may not be so. At the present stage of the art it is difficult to identify with finality and to quantify linkage effects.

The development strategy of export creation seems, however, to be quite suitable for the specific case of the Caribbean economies that are characterized by a high degree of "openness." This openness is not so much the result of the significant share of exports in the GDP as it is the outcome of institutional factors which narrowly circumscribe the countries' financial, commercial, and monetary policies. The monetary system of the Caribbean is the characteristic colonial Currency Board system. In essence, it is a rigid form of the Gold Standard with 100 per cent backing of local currencies by sterling assets. The local currency is automatically convertible into sterling at fixed rates of exchange. No exchange controls against the sterling can be imposed. Import licensing is not widely used to protect local industry. The consequences of this dependent monetary system are that the money supply depends on the balance of payments; that deficit financing is ruled out (which explains the absence of domestic inflation); and that the rate of growth is tied to the rate of increase in exports. Under such circumstances it is only natural that there are limited possibilities of import substitution and that the export sector becomes the leading sector in the development strategy of the Caribbean countries.

In the last chapter Demas provides a brief but authoritative and insightful discussion of the rationale, the objectives, the techniques, the limitations and the problems of implementation of planning, drawn from his experience as head of the Economic Planning Division of the government of Trinidad and Tobago. As the reader may have suspected, the role of planning in small open economies like the Caribbean is rather limited. The automatic currency system guarantees that there will always be a balance in external payments. Planning then focuses on the elimination of sectoral imbalances. Yet the lack of control over the monetary and financial systems severely restricts the implementation of the plan. So does the small role played by the public sector—the share of the public sector in the total capital formation is 10 and 22 per cent for Jamaica and Trinidad and Tobago respectively. As a result, planning in the Caribbean is more indicative than it is in other underdeveloped countries. Yet it seems that even under such severe limitations a National Plan is still a valuable educative document for both business and government.

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Statistical Methods; Econometrics; Social Accounting

The Brookings Quarterly Econometric Model of the United States. Edited by J. S. DUESENBERY, G. FROMM, L. R. KLEIN AND E. KUH. Chicago: Rand McNally; Amsterdam: North-Holland, 1965. Pp. xiv, 776. \$9.00.

The publication of the Brookings quarterly econometric model of the post-war U.S. economy is a milestone in the development of econometric analysis.

Never before has such a large group of distinguished econometricians worked closely together and produced a coordinated research. The editors and the authors are to be congratulated on this outstanding contribution to the literature.

This project was initiated in early 1959 by the Social Science Research Council and later sponsored by the National Science Foundation. Working papers were prepared and consultation conferences held during 1961 and 1962. Beginning in September 1963, the headquarters of the project was established at the Brookings Institution. A unique feature of this project is that it will be a continuing program, which is exceedingly important. Econometrics is still a young discipline. As more and better data and new techniques of estimation become available, refinements and even basic changes in certain parts of the model may be called for. As the editors mentioned in the introduction to the volume (p. VII), the model presented "will be superseded shortly after it is published." Were it not for this reason, a review article of this monumental work, instead of the brief space allocated to this review, would be justified.

This model was constructed in the Tinbergen-Klein tradition, using simultaneous equation estimating techniques developed at the Cowles Foundation and the Netherlands School of Economics. However, the comprehensive scope, the high degree of disaggregation, and the vigor of the theoretical analysis underlying certain parts of the model (especially the investment functions) are unprecedented. All the contributors to this volume are leading experts in their respective fields, and have enriched the volume with the knowledge and experience accumulated long before the inception of this project.

A bird's-eye view of the model can be provided through a brief summary of its separate parts. J. S. Duesenberry and L. R. Klein prepared a lucid presentation of the basic research strategy. D. W. Jorgenson contributed a quite original theoretical analysis of investment anticipation and distributed lags, together with some empirical estimates on the data provided by the OBE-SEC Investment Survey. This is followed by an elaborate effort by Robert Eisner to transfer intentions into predictors of actual investment through the construction of realization equations. Eisner's work is typical of almost all the other chapters in the volume in that it is in fact a continuation and refinement of his well-known contribution in this area. Inventory equations were estimated by P. G. Darling and M. C. Lovell, much along the approach made familiar by Lovell's earlier work. Since unfilled orders played an important part in the inventory equations, an attempt was made by Manoranjan Dutta to explain the determination of new orders. S. J. Maisel's study of residential construction painstakingly began with an explanation of housing starts in terms of such unfamiliar variables as total available housing units, vacancies and net removals of these units, in addition to the often used income and demographic variables. D. B. Suits and G. R. Sparks estimated consumption functions on the stock adjustment approach, with a number of price elasticities of demand evaluated.

Previous econometric models had generally been elaborate in their treatment of the various components of effective demand, but often weak on the side of production, employment, and income distribution. This deficiency was

made up by Edwin Kuh's estimates of man-hour and employment equations both in the aggregate and for different industries and sectors, with dynamic adjustments and with production and nonproduction workers differentiated. Functions for the different components of income have also been estimated by Kuh. Another common weakness of previous econometric models is the lack of a satisfactory explanation of changes in price levels. C. L. Schultze and J. L. Tryon brought a new approach to bear on this problem by explaining price movements with markup equations at the industrial origin of production. Not since the time of the classical economists have demographic variables received as much attention as in this volume. Stanley Lebergott formulated and estimated equations which determine the labor force and marriages as endogenous variables in the model. R. R. Rhomberg and Lorette Boissonneault estimated disaggregated import and export functions and found reasonable price elasticities. In contrast to the crude manner in which the farm sector was dealt with in previous aggregate models, K. A. Fox derived functions which explained the fluctuations in farm income, prices, and inventory.

An elaborate submodel of the financial sector was developed by Frank De Leeuw, consisting of demand and supply equations, estimated on the stock adjustment approach, for seven categories of financial variables, including bank reserves, currency, demand deposits, time deposits, U.S. securities, "savings and insurance" and "private securities." An even more exhaustive system of equations was constructed by Albert Ando, E. C. Brown, and E. W. Adams to determine, as endogenous variables, most government receipts, transfers, and nondefense expenditures.

An econometric model should not be constructed merely for the satisfaction of having a self-contained system of equations. Its usefulness lies in the prospects that it will yield a satisfactory description and explanation of the historical development during the sample period, that it will serve as a forecasting scheme, and that it will contribute to intelligent policy formulation. All these require a computer program for solving a system of nonlinear difference equations. C. C. Holt developed the required program for the Brookings model and also made certain test runs.

From the point of view of econometric methodology, the chapter by F. M. Fisher on dynamic structure and estimation is the most interesting. Fisher made a compact but lucid survey of the assumptions and properties of ordinary least squares and the full information and limited information estimators. The full information method did not stand out in the comparison with as much distinction as might have been expected, not because of computational difficulties but for its relatively high sensitiveness to multicollinearity (originally pointed out by Klein and Nakamura) and specification errors. The limited information maximum likelihood estimator also suffers a higher degree of sensitiveness to multicollinearity compared with the other limited information estimators. The present popularity of the two-stage least squares method (also a member of the limited information group in dealing with simultaneous equation estimation), therefore, has some theoretical justification, unless the system is recursive, in which case the time-honored ordinary least squares method enjoys many comparative advantages. The assumptions underlying the recursive sys-

terms, however, can hardly be fulfilled in a simultaneous equation model, especially when the period of the sample observation is as long as a quarter. The use of a limited information estimator, therefore, seems unavoidable in estimating quarterly models, and the selection of proper instrumental variables becomes exceedingly important in large-size models. Fisher's contribution to the block-recursive systems and the criteria for choosing the instrumental variables greatly simplified this difficult task.

The final demand components (e.g., consumption and investment) do not coincide with the usual industrial classification of production sectors (e.g., agriculture and manufacturing). Since both schemes of disaggregation are included in the model, a bridge must be built to close the model. The output absorbed by the final demand sectors must be distributed over the production sectors, and the prices generated by the industrial markup equations must be converted to the various final demand deflators. This task was accomplished through the input-output and final-demand relationships developed by Fisher, Klein, and Yoichi Shinpai, on the groundwork done by Fox and J. Babcock.

In the final chapter, a complete model was outlined by Gary Fromm and Klein on the basis of the equations obtained for the different sectors, as summarized above, with some modification and simplification. Approximately 150 equations are genuinely estimated; others are definitions and identities. While the system is apparently not yet completely closed, the gaps do not appear to represent basic obstacles. The size of the volume already approaches 800 pages. Given the truly staggering magnitude of the work completed within a relatively short period of time, one feels more than satisfied, even though the model has not yet been applied to forecasting and policy formulation in this volume.

A rather comprehensive flow chart accompanies the volume. Before one tackles this complicated diagram, it will be helpful to study first the two illustrative equational systems developed by Fromm and Klein (this *Review*, Papers and Proceedings, May 1965, pp. 348-61) which contain the basic features of the flows and feedbacks in the complete model.

Space does not permit detailed critical comments on the various chapters separately. A few questions, however, may be raised with respect to certain broad aspects of the model.

First, perhaps the most important unsolved problem in econometric methodology is how to decide among alternative specifications. There are usually many plausible theoretical explanations of the movement of a given endogenous variable; and very often, many of them fit the sample data just about equally well. It would be ideal if it were possible to incorporate all the plausible theories into the function and let the data assign the proper weight each theory is to carry in the explanation. However, this approach has almost never worked, mostly on account of multicollinearity and the limited number of observations. The usual way out of this difficulty is to try experimental fittings on the alternative theories and then more or less arbitrarily select the result most appealing to the author concerned. Practically, all the equations presented in this volume were obtained in this manner. In a few instances, the rejected versions are presented together with the accepted ones; in the great

majority of cases, however, only those accepted are given.¹ Now that a procedure for the selection of instrumental variables has been formulated, the time has arrived to formulate a general procedure for making and rejecting specifications. The Bayes approach may or may not be a solution of this problem, but it deserves a tryout.

Second, econometric models of the Tinbergen-Klein tradition are business-cycle oriented. In a comprehensive nation-wide empirical model, it seems questionable whether it is rewarding or even legitimate to study growth and fluctuations separately. A case in point is public investment. This variable is completely absent from the model except from the point of view of government expenditure. In a sample period as long as a decade and a half, it does not appear reasonable to assume that the development of the highway systems, for instance, had no effect on production in the private sectors. The same question is relevant for all kinds of investment in social overhead, including education and public health. Public investment should at least be brought into the trend lines around which fluctuations are measured.

Practically all the equations were estimated on seasonally adjusted data. Nerlove has raised the question whether the original unadjusted data should not be used instead, so that the estimate of seasonality would become a part of model construction itself. In this way, the seasonals of the endogenous variables may be explained to an important extent by those of the exogenous variables. (See Marc Nerlove, "A Quarterly Econometric Model for the United Kingdom: A Review Article," this *Review*, March 1962, 52, 154-76.) In fact, this approach was used in the U.K. model constructed by Klein and his associates and reviewed by Nerlove in the article cited above. There are of course difficulties involved in using the seasonally unadjusted data. It would be interesting, however, to see a comparison of the results obtained by using the adjusted and the unadjusted data respectively.

The Brookings model deserves to be studied, not only by econometricians, but also by economists in general. Some of the equations developed for the various sectors of the economy may even be presented in the introductory course in economics, together with an explanation of *both the value and the limitations* of the quantitative approach. If this perhaps novel feature is added properly, the beginning course might become less remote from, and irrelevant to, the reality of economic life.

TA-CHUNG LIU

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Domestic Capital Formation in the United Kingdom 1920-1938. By C. H. FEINSTEIN. Cambridge: Cambridge University Press, 1965. Pp. xii, 270. \$17.50.

This book presents the reviewer not only with the usual problem of the trade, but also with that of two layers of authorship. The estimates were originally prepared by Mr. Maywald, and a brief summary was published by him in *The London and Cambridge Economic Bulletin* in 1960. The estimates and

¹ A mimeographed list of all tried but rejected equations would be a most valuable reference to professional econometricians.

text were then revised and the text rewritten by Mr. Feinstein. The Department of Applied Economics in Cambridge, which had initiated the project as far back as 1950, did not accept Mr. Maywald's draft. Mr. Maywald, in turn, disavows the published version. In this situation it is difficult to decide how the estimates should in the future be referred to—and referred to they will be; maybe the fairest thing would be to call them the Feinstein-Maywald estimates. Whatever the exact division of authorship, the result is undoubtedly the most thorough estimate of capital formation in the United Kingdom during the interwar period and a book that in many respects can serve in execution and presentation as a model for similar enterprises, e.g., in the "Synopsis of methods of estimation for main fixed assets."

The book consists of two chapters on concepts and methods of estimation and three chapters appraising results by comparing the new estimates with figures derived by the commodity flow method and with less complete estimates made by others (not including Maywald) and by evaluating their reliability. These are separated by a chapter briefly summarizing the results and by nine chapters describing in detail the estimates for each of the main sectors.

One of the strong points of the book is the description of sources and procedures which is done with considerably more care and in greater detail than in many publications of this type. The weaknesses of many of the estimates, unavoidable in view of the rather poor quality of the basic statistics, are frankly acknowledged. Very great ingenuity is displayed in using every available scrap of information. Some important components of the estimates, particularly those derived from corporate accounts represent new primary statistical data. It is not likely that anyone will want, or will need, to make another attempt at measuring capital formation in the United Kingdom in the 1920s and 1930s.

Interpretation of the data is another matter. Here the author limits himself to short summaries of the figures and refrains from investigating the relationship of the estimates of capital formation to other relevant economic statistics, except for the calculation of a few simple ratios, such as the average capital output ratio and the capital formation ratio. Similarly there is only a very brief discussion of problems of definitions and meaning of capital formation and stock of reproducible tangible wealth, but a clear operational description is furnished of the procedures actually adopted.

The results of this great effort are annual estimates of gross capital formation, depreciation, retirements and net capital formation and of gross and net stock of reproducible tangible assets on three price bases, namely original (first) cost, 1930 prices, and current prices. While only a few types of durable goods are distinguished—usually only structures, equipment, and inventories—the number of sectors and subsectors for which full information is given separately is large: 8 main sectors and nearly 50 subsectors of which 22 groups are within manufacturing.

The method and sources of the estimates vary greatly. The resulting estimates thus are a mosaic put together with great skill out of heterogeneous material. One main characteristic of the estimates is that greatest reliance is put

on data from the accounts of the enterprises undertaking capital expenditures rather than on figures derived by the commodity flow method, a prerequisite for the great sectoral detail in which the figures are presented. The main original contribution, and one that must have taken a very great amount of time and effort, is the compilation described in Appendix 2.1 of income accounts and balance sheets of companies listed on the London Stock Exchange (all companies with issued capital greater than £500,000 and one-third of smaller companies), which provides the basis for the estimates of capital expenditures in many subsectors, particularly in manufacturing. One would hope that this rich and otherwise inaccessible material will be made available elsewhere as fully as possible for use by other students. As usual, the figures for capital expenditures in constant prices are much more doubtful, as they had in many cases to be derived with the help of specially constructed price indices, the basis of which is often unavoidably weak.

Another characteristic of the estimates is that figures for stocks of capital are not derived by the perpetual inventory method, but start from stock estimates, usually of gross stock, for one date obtained by very different methods, which are reduced to estimates of net stock by a rather rough standard method of estimation, and are then carried forward or backward for the other years of the period on the basis of annual capital formation data.

Some of the main findings of the study, none of which is unexpected, may be summarized as follows: (1) Total capital formation in current prices averaged a little over 11 per cent of gross national product for the period 1920-38. This is by present standards a very low level, even for the United Kingdom where the ratio averaged 18 per cent in the 1950s and 13 per cent in the first decade of this century. (2) Fully one-fourth of total capital formation (1930 prices) consisted of dwellings, about 30 per cent of other structures, and an equal amount of equipment, while 8 per cent was used to increase inventories and 6 per cent to add to net foreign investment. (3) Less than about one-third of total capital formation was accounted for by industry (manufacturing, mining and power) and less than one-fifth by transportation and communication. (4) Of fixed investment, 40 per cent was public or semipublic and 60 per cent private.

From a comparison of Mr. Maywald's original estimates with the published figures it appears that the difference is not too serious for the national aggregates of capital formation. For the period as a whole Feinstein's estimates of gross fixed capital formation are 14 per cent above Maywald's on a gross, but only by 6 per cent on a net basis. Feinstein's estimates of fixed capital stock, however, are continuously and substantially below Maywald's; in 1938 e.g., by 18 per cent for the gross stock in 1930 prices. This is due primarily to the much higher estimates of Maywald for the stock of dwellings—in 1938 £7.48 billion against Feinstein's £5.47 billion, reflecting a similarly sharp difference in their benchmark estimates.

With its ample size (310 by 245 mm), large fount, beautiful typography, wide margins, good paper, and solid cloth binding, this volume, like its companions in the series, is the closest approach to a "coffee table book" (*honn*

soit qui mal y pense) that we have in the field of economics. (The first edition of the *Wealth of Nations* measures 272 by 210 mm and, of course, is printed on rag paper.) High quality is never cheap. It is therefore not astonishing that this volume sells for \$17.50 or 6.2¢ for each of its 270 pages, compared e.g., to 2 to 2½¢ for the much less sumptuous and smaller (227 by 157 mm) recent publications of the NBER. This is not meant as a criticism since the price elasticity of demand for the series probably is quite low—it will get on few individual economists' shelves, and most larger libraries will have to buy it without regard to price given the series' importance and quality.

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Maxima and Minima: Theory and Economic Applications. By RAGNAR FRISCH, IN COLLABORATION WITH A. NATAF. Chicago: Rand McNally, 1966. Pp. xii, 176. \$8.50.

Economics students with limited background in mathematics are frequently puzzled—if not terrified—to find things like Lagrange multipliers, quadratic forms, and eigenvalues slipping into otherwise innocent discussions of profit maximization. Since it is not easy even yet to find a leisurely discussion of these topics which is nevertheless thorough and careful, such students should be grateful to Professor Frisch; his book goes some way toward meeting this need.

The book is an English translation of a 1960 French edition. Its 175 pages are directed largely toward determining the conditions necessary or sufficient for an extremum, possibly subject to equality constraints, of a function having continuous and bounded derivatives of any necessary order. The approach is well described by the author in his preface to the English edition:

When I have had to approach a problem requiring the use of ideas of a more advanced level than that of college mathematics, I have started with a detailed discussion of a few particularly simple special cases. But . . . I have applied to these simple cases the more elaborate method suitable for dealing with the general case. In this way, the reader is brought—in a quite intuitive manner, usually without the aid of any proof—to understand the steps leading to the generalisation. . . . Experience has shown that proceeding in this way one is able to get astonishingly far with a very moderate mathematical apparatus, and to reach an understanding of the situation which is sufficient for practical needs.

The dust jacket adds that "The book is intended first of all for economists and students of political economy without mathematical training . . .," but this has to be interpreted somewhat generously. Acquaintance with the calculus of several variables, including at least a grasp of Taylor's theorem, is a prerequisite to the book.

Chapter 1 contains a few observations on the nature of mathematical economics and on the distinction between necessary and sufficient conditions. Chapter 2 works out the maximum of a binomial expression, as an example of

extremum problems with discrete variables, and formulates a linear programming problem in standard form. Neither topic is developed further in the book. Chapter 3 discusses a few graphical examples "to show that the method of putting the derivatives equal to zero must not be applied mechanically, without circumspection" (p. 20).

The main topic is entered in Chapter 4, which employs Taylor's formula to develop necessary conditions and sufficient conditions for an extremum of a function of a single variable. (The suggestion [on p. 22] that Taylor's theorem can be fully developed merely by applying the Mean Value Theorem to successively higher derivatives seems misleadingly simple, but probably does no harm in its context.) Chapter 5 uses Taylor's expansion for several variables in the same way to obtain necessary conditions for an extremum for a function of more than one variable. The introduction of a single constraint is handled in Chapter 6 by observing that intuitively one expects that at an extreme point the normal to the constraint set should be proportional to the gradient of the function whose extremum is sought. It is then shown that this condition can be rewritten in terms of an unknown constant of proportionality whose value is determined so as to ensure satisfaction of the constraint. The case of several constraints is studied by using the constraints to eliminate some variables, and then showing that the determinantal conditions necessary for an extremum of the resulting function imply the existence of the constants which are the Lagrange multipliers. The Lagrangean function is thus a purely formal construct enabling one to write down the correct necessary conditions. A short, rather awkward "supplement" considers a direct introduction of the Lagrange multipliers, but no economic interpretation is attempted.

Chapter 7 digresses to discuss the concept of Pareto-optimality by means of an example based on an Edgeworth-Bowley box for individuals with identical utility functions $U(x,y) = xy$.

Chapters 8 and 9 deal with linear equations, homogeneous systems, and linear dependence in a thoroughly conventional way. (It is, however, surely misleading to claim that "There exist various very effective methods of calculating numerically the right hand side of (8.19) [an equation using Cramer's rule for calculating the solution of a linear system as a ratio of two determinants] even when the number of unknowns is very considerable, and may, perhaps, be counted in hundreds" (p. 68).

With this background on linear systems at hand, the text returns to the question of extrema. In Chapters 10 and 11 the dominance, in the neighborhood of a critical point, of the quadratic term of the Taylor expansion is demonstrated, and sufficient conditions based on properties of this quadratic form are developed for functions of two or several variables without constraints. The question then being to determine the properties of the quadratic form, the largest and smallest characteristic roots of the Hessian matrix are shown to provide bounds on the values assumed by the quadratic form. In Chapter 12 equality constraints are introduced, and the parallel development based on the bordered Hessian matrix is obtained by a natural extension of the preceding results. The graphical interpretation of properties of the quadratic form, and the extended discussion of the significance of the characteristic

roots of the matrix of second partial derivatives, should prove particularly helpful to students.

The book concludes with a brief account of the theory of matrices and determinants, and a three-page appendix on complex numbers. Together with Chapters 8 and 9, this material on matrices and linear systems makes up just over one-half the book.

One disadvantage of the book as a text might be well illustrated by this last chapter. Taking the position that "A matrix is simply a table of numbers arranged in rows and columns" (p. 144), the chapter proceeds to develop various relations, such as that for the determinant of a product of matrices, essentially as remarkable arithmetic facts. Though the concept of a matrix as a transformation is mentioned in an example, it is nowhere used to give intuitive substance to concepts introduced in the text. Thus the sections on matrices and linear systems tend to develop somewhat by rule, rather than by reason, even though in many cases fully adequate algebraic proofs are provided. Some insight is sacrificed by this approach.

In summary, then, the book has a highly readable account of the general features of classical extremum problems, it has a brief introduction to the elements of linear systems, and it has some illuminating examples carefully worked. As a supplementary text for the student who is taking the first course in mathematics for economists (though his heart might lie elsewhere), it should prove very appropriate. Such a student would find the material accessible, the emphasis on precise statement agreeable, the economic examples helpful, and the whole less formidable than other works (such as Harris Hancock's standard work or recent accounts such as Chapter 3 of Hadley's *Non-Linear Programming*) on the same subject. Nor would he be troubled by the fact that more general mathematical constructs, perhaps yielding more insight, are lacking in this introductory work. Most regrettable for his purposes would be the omission of any discussion of inequality constraints and the lack of any economic interpretation of Lagrange multipliers.

Though the "economic applications" are limited to a very few examples, the present book does seem still to be one of the few adequate introductions to the theory of maxima and minima accessible to the student of economics who is not skilled in mathematics.

A. R. DOBELL

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Economic Systems; Planning and Reform; Cooperation

Modern Capitalism—The Changing Balance of Public and Private Power. By ANDREW SHONFIELD. London and New York: Oxford University Press, for the Royal Institute of International Affairs, 1965. Pp. xvi, 456. \$10.50.

Since World War II the performance of capitalism has greatly improved. Output grows more rapidly, income is better distributed, and the economy is much more stable. In part this book is an attempt to explain how this

happened and why modern capitalism has succeeded so well. Shonfield discards the idea that the improvement is of a temporary nature, or that we are simply experiencing a long upswing of a kind seen before due to special factors promoting trade and building. "The central thesis of this book is that there is no reason to suppose that the patterns of the past . . . will reassert themselves in the future."

He argues that the improvement is not due simply to the adoption of Keynesian policies of demand management. There are new institutional features of capitalism. The public authorities have vastly increased powers, and competition in the private sector has been increasingly regulated and controlled. "Governments in their anxiety to increase the area of the predictable for purposes of economic planning have encouraged firms within an industry to evolve agreed policies on the basis of their common long-range interests." Economic decision making has become coherent and the area of the unpredictable has been reduced.

The bulk of the book does not deal with the factors affecting the differential performance of various economies, or those affecting their generally improved supply potential, in the postwar period—though this is attributed largely to accelerated technological progress. Its scope is better suggested by the subtitle. It is mainly concerned with the relation between the public and the private sectors, and in analyzing whether the techniques by which modern governments manage the economy are compatible with the ideas and practice of traditional parliamentary democracy.

Most of the book is based on the assumption that economic planning is the major institutional characteristic of modern capitalism. Not all countries have accepted this in the same degree, least of all the United States, but they are all moving inexorably in this direction. At times it seems to be implied that differences in performance are due to differences in the degree to which planning has been espoused. Shonfield's prototype of modern capitalism is therefore France, where he finds plenty of evidence of planning. In other countries—such as Italy, Germany, Sweden or Austria—it requires more ingenuity to show that planning has been important.

This hypothesis can only be sustained by interpreting many different things as planning. This is done by arguing that the varying national approaches to planning derive from separate historical traditions which differ in form but not in substance—the French *étatist* tradition, Italian corporatism, and German sense of discipline, power of cartels and banks. However, the extensive historical analysis of the different approaches to "planning" emphasizes the traditional elements in policy rather than bringing out any specifically new features of modern capitalism. The argument is particularly unconvincing in the case of Germany. It is argued that coherent long-term plans for the future exist because business and banks are closely interlocked and long-term planning is done by firms themselves. It is difficult to accept that this has much in common with what happens in France, or that these features of German capitalism are new. It is also far from clear that the move towards planning is an inexorable one. In France, for instance, the opening of the economy to international trade, and the growth of a more active capital market could well reduce the existing degree of *dirigisme*.

Shonfield stresses the incoherence of the U.S. administrative tradition, which makes planning difficult, although here too he finds an "intellectual underworld of American business" who are really planners. This overemphasis on planning weakens his analysis of the reasons why the United States was so reluctant to embark on efficient Keynesian policies of demand management.

In my view Shonfield does not succeed in demonstrating that long-term planning has played as big a role in the performance of modern capitalism as he claims. Planning is something which has only recently become generally popular, and can have had little influence in the postwar growth of most countries. This is not to deny that it is an important or useful additional means to foster rapid growth. But he has mixed up his own recommendations with his analysis of what actually happened. His postulate about the decline of competition also seems exaggerated. The book largely ignores postwar developments in international trade and payments policies, and hence does not have any discussion of increased competition on this account.

In spite of its weakness as a characterization of modern capitalism, the book is excellent as an analysis of the political problems created by the large scale of government intervention in the economy. It gives a scholarly account of the administrative and parliamentary traditions in economic policy questions and a vivid picture of the working atmosphere of the economic bureaucracy in the different countries. He feels that the degree of intervention and administrative discretion which takes place in France is an inevitable feature of modern development, that this should be explicitly accepted and that civil servants in other countries should have greater liberty to express their judgments openly and be subjected to greater public scrutiny. He feels that the French type of civil service as checked by the Conseil d'Etat and Cour des Comptes is the one appropriate for our age. He also feels that government-business deals of a nonmarket character are bound to be extensive, and that the best form of parliamentary surveillance is that developed by the U.S. Congress—i.e., specialized committees to cross-examine both the executive power and business, with professional staff and public hearings.

He also expresses the need for some type of public authority which recognizes the need for active cooperation with private business, which is actively "wheeling and dealing," "seeking out allies, probing and manoeuvring for the active consensus." Here he does not prescribe any particular prototype, but one feels that the U.K. Department of Economic Affairs may be what he has in mind.

The argument on the disappearance of competition is underpinned at several key points by illustrations from defense industries. In these cases, governments must often conclude deals with a large private firm without any possibility of competition. In most European countries, there is often no means at all by which the public or the economist can make a judgment on the rationality of such government transactions. This is a field of government operations which is in urgent need of more public scrutiny and to which Shonfield's general recommendations obviously apply. However, this field of government activity raises rather special problems to which it might have been worth devoting a separate chapter.

The book is a discursive essay, which ranges over a vast array of economic and political issues. It is an illuminating and scholarly work, which does not quite live up to its ambitious and exciting title, but it provides a fascinating account of the modern balance between private and public power and of the policy issues which this raises.

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Planning in the Soviet Union. By PHILIPPE J. BERNARD. New York and London: Pergamon Press, 1966. Pp. xxv, 309. \$9.50.

This translation of a book originally published in French (Paris 1963) reports Dr. Bernard's impressions, musings, and interpretive scrabbings growing out of a two-month visit to the USSR and some scattered reading in the literature on the Soviet economy. They are pretty shallow and unrewarding. The hope that an experienced western planner like Dr. Bernard, given the opportunity to talk with a large number of responsible Soviet planners and economists, might have some solid observations and evaluations concerning Soviet planning is unfulfilled. He was struck by the differences that impress any outsider when he first contemplates the USSR and the conception of detailed centralized planning, but he offers no new information or insights. The book reeks of standard official commentary on any given problem, unilluminated by analytical guidance or by detailed factual knowledge of realities. For example, what he says about inflation is silly, i.e., that increases in bank credit to enterprises cannot lead to inflation in the consumer goods market since credit and currency move in different circuits. The discussion of the capital allocation problem as an aspect of efficiency calculations faithfully reflects the chaos of Soviet thought on the matter. As an introduction to Soviet planning, the best use to which the book could properly be put would be to implant red herrings and useless clichés in the mind of the student, against which the brilliant analysis of the instructor could then contend. Its translation and publication must indicate surplus capacity.

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Business Fluctuations

How Business Economists Forecast. Edited by WILLIAM F. BUTLER AND ROBERT A. KAVESH. Englewood Cliffs, N. J.: Prentice Hall, 1966. Pp. xiv, 540. \$15.95.

Applied Economic Forecasting. By HENRI THEIL, WITH G. A. C. BEERENS, C. G. DE LEEUW AND C. B. TILANUS. Chicago: Rand; Amsterdam: North-Holland, 1966. Pp. xxv, 474. \$14.00.

Although these two books both deal with the general subject of forecasting, they are distinctly different in content, technique, and overall approach. They have been written for different audiences and may accurately

represent the difference between the current frontiers of knowledge about forecasting methodology and its practice in the business and financial communities. The collection of 26 essays which were edited by W. F. Butler and R. A. Kavesh was designed to be of practical value for both economists and businessmen. The business economists were supposed to be able to improve their forecasting accuracy by applying the techniques which were contained in the collected essays, while businessmen after reading the book, were to gain insights about the manner in which the American economy functions. The primary intent of the book, however, is to demonstrate how forecasters approach the problem of predicting Gross National Product, its components, and industry and company sales. The particular emphasis is on making GNP predictions, by utilizing intuitive or judgmental techniques.

On the other hand, Henri Theil's presentation and analysis is at least one and perhaps two stages more advanced than the aforementioned approach. Theil does not attempt to outline a procedure for making forecasts, but rather presents a theoretical and empirical analysis of the accuracy of previously generated economic forecasts. This type of analysis would enable a forecaster to identify the sources of his previous errors and to undertake the necessary corrections. Theil's analysis of the predictions utilizes existing econometric and statistical techniques, but new methods for converting nonquantitative forecasts into estimates which may be judged by quantitative tools are also developed.

The macroeconomic forecasts which Theil analyzed had been generated by econometric rather than by intuitive techniques. However, it is not possible to compare the forecasting accuracy of the intuitive and econometric approaches because Theil uses European data while the collection of essays is devoted to an analysis of the American economy. In addition, the Butler and Kavesh collection does not contain an historical record of the forecasts generated by intuitive techniques which would have permitted such an analysis.

The Butler-Kavesh collection of essays is divided into five parts: (1) general approaches and techniques of forecasting, (2) procedures for forecasting GNP, (3) methods for preparing firm and industry forecasts, (4) forecasting in the financial area, and (5) the use and evaluation of forecasts. The seven chapters of Part 2 and several of the chapters of Part 3 form the core of the book. These chapters present the techniques utilized in the intuitive approach to forecasting Gross National Product and its components; however, in these chapters the difficulties associated with using this approach also become visible.

One of the difficulties of forecasting is to determine when estimates based on past relationships are no longer valid. It is, therefore, important to retain flexibility and to take fundamental changes into account when past relationships are utilized in forecasting. However, it is difficult to convert some of Butler and Kavesh's statements such as, "study inventory-sales ratios in an attempt to anticipate gluts or shortages," "further insight into capital spending may be derived from studying the reports of the National Industrial Conference Board," and "examine . . . the trend of transfers, interest,

and subsidies," into *quantitative* corrections which may be applied to estimates of inventory investment, plant and equipment expenditures, or government purchases of goods and services. Such vague procedures prevent an observer from determining whether an analyst has incorporated correct relationships into his forecasts. Moreover, evaluations of such forecasts and the development of concomitant corrective procedures cannot be undertaken. Finally, a decision maker may not know how much confidence to place in such forecasts, and it is virtually impossible for a novice to learn how to predict in this manner.

Moreover, as Butler and Kavesh indicate, after the individual components and GNP have been estimated, it is necessary to determine whether the separate estimates are consistent with each other. If not, a series of successive approximations is required to obtain consistencies which correspond to past relationships, but the authors do not want to depend upon past interrelations, for they argue that "past experience cannot be wholly relied upon," and "Judgment, consequently, must take over . . ." (p. 157). Consequently, the fundamental question is whether forecasts generated by a method in which consistency is automatically assured, even though derived from the past, might not be superior to the estimates obtained from repeated intuitive juggling. To obtain this answer, further investigations are required, for none of the authors analyze the accuracy of their respective estimates.

The chapters devoted to the individual components contain similarly vague statements stating that it is necessary to make adjustments for a variety of reasons without indicating the weight that is to be given to each factor. Nevertheless, each chapter contains some particularly useful information. M. L. Weidenbaum's description of the various government budgets can be of value to the noneconomist. Greenwald shows how the plant and equipment data and the gross private domestic investment series might be reconciled. Robinson Newcomb analyzes the demographic factors which influence long-run residential construction and indicates that the short-run movements involve deviations from the long-run trend.¹ R. J. Eggert and P. W. McCracken take the same approach in forecasting automobile consumption. They first attempt to estimate the long-run market and only then the short-run yearly deviations from this trend. They present various "rules of thumb" which might yield rough estimates of the long-run demand, but there is also some theoretical analysis which might be utilized to obtain better specifications of this demand. However, the short-run forecasting problem remains unsolved.

L. J. Paradiso's chapter on inventory investment is particularly useful. He indicates how changes in the stock of inventories are estimated and explains the difference between the changes which are derived from the book

¹ A similar but more advanced approach to forecasting residential construction was presented by S. J. Maisel, "A Theory of Fluctuations in Residential Construction Starts," *Am. Econ. Rev.*, June 1963, 53, pp. 359-83. This approach is more advanced than Newcomb's for it not only considers the factors which determine deviations from a long-run trend, but also attaches weights to each of the variables.

value of inventories and those which are obtained on a GNP basis. In addition, the relevance of inventory-sales ratios and the anticipations surveys are mentioned. Paradiso's conclusion that the anticipation data systematically understated actual inventory movements is of interest, for Theil, in his book, showed that the European survey data also yielded consistent underestimates. Since Theil has presented techniques for analyzing the predictive value of anticipatory data, an application of this technique to the U. S. data might improve the accuracy of our inventory forecasts.

R. D. Woodward presents a survey of forecasting techniques applicable to the steel industry. He indicates that an appropriate procedure to obtain steel demand would be to project the level of activity in steel-consuming industries and then to multiply these activity levels by appropriate weights. This discussion would suggest evaluating the applicability of input-output analysis for forecasting purposes, another procedure which Theil's book analyzes, but Woodward does not even consider this approach.

In other essays within the Butler-Kavesh book, F. G. Adams and P. E. de Janosi, for instance, examine the statistics and econometrics of forecasting. They note that extrapolation techniques are simple and often useful, but they do not indicate that these techniques are "naive." The discussion of econometric models would leave a layman with a false impression about their value for forecasting, for they are supposed to yield *mechanical* results. There is also no discussion about the flexibility that may be incorporated into econometric models, but a model's ability to provide consistent estimates of interrelationships is noted.

L. H. Lempert and A. T. Sommers discuss the leading series and diffusion index approaches to forecasting, but neither evaluates their record in predicting past cycles. Morris Cohen's treatment of survey data does include such *qualitative* evaluation, indicating that the data are useful as supplements to other methods and for cross checking. J. F. Rodriguez's chapter "Sources of Statistics" would be helpful to those unfamiliar with the data. It would be especially useful to economists who were attempting to reconcile figures which presented conflicting trend-cycle patterns, an occurrence which frequently confronts forecasters.

W. H. Chartener's essay on evaluating forecasts indicates the difficulty encountered in applying a standard against which historical forecasts may be judged. While mentioning naive and consensus forecasts as appropriate standards for comparisons, a statistical test, such as Theil's U coefficient which has been available since the late 1950s, is not presented. Nor does Chartener indicate that data revisions create problems in evaluating forecasts, for it is not always clear which set of figures should have been predicted.

On the other hand, Theil's book assumes that an appropriate predictive methodology exists. The book evaluates the accuracy of forecasts generated by these techniques while also advancing the frontiers of knowledge concerning forecasting techniques. This book is also divided into five parts: (1) the methodology for forecasting and evaluating predictions, (2) an analysis of past macroeconomic forecasts and estimates, (3) the application of input-

output models to forecasting, (4) the use of information theory for evaluating forecasts, and (5) an analysis of survey data.

After listing the types of predictions which may be made and discussing the functions of forecasts, Theil concludes that both the actual forecasts and the prediction procedures must be verifiable. This conclusion is at variance with the intuitive approach, for not all of the procedures of the judgmental forecasters are verifiable. If forecasting is to become less of an "art" and more of a science, it must, in this observer's view, opt for the verifiable approach. Only in this manner is it possible to examine the causes of inaccurate forecasts and undertake corrective procedures. This does not imply that all forecasts must be based upon econometric models, but if judgmental techniques are utilized, the weights and impact which are assigned to specific variables must be clearly stated. Then the accuracy of these forecasts may also be evaluated.

The value of a forecast, according to Theil, is based upon its use in making decisions. Theil shows that the loss between the decision which is made with an imperfect forecast and the decision which would have been made had the prediction been perfect is a function of the size of the prediction error. This analysis leads to the development of prediction-realization relationships in terms of predicted and actual *changes*. The prediction error can thus be measured by the inequality coefficient,

$$U = \frac{\sqrt{\sum (P_i - A_i)^2/n}}{\sqrt{\sum A_i^2/n}}.$$

The form of this particular statistic differs from Theil's previous U coefficient, for the new coefficient has no upper limit. The earlier version had an upper limit of 1, the value of the measure when no change predictions are made. Since it is conceivable that particular forecasts might be inferior to no change extrapolations, the newer statistic is a more desirable measure. (In addition, the distribution of this statistic has been derived.) This coefficient like its earlier version can be broken down to indicate whether the errors were attributable to biases or variance and covariance relationships.

In the second part of Theil's book, the inequality coefficient and other measures are applied to particular Dutch macroeconomic series, and an evaluation of the accuracy of both the forecasts and the preliminary published estimates of changes is undertaken. The preliminary estimates are examined, for these data are also subject to errors and their usefulness in interpreting economic movements is related to their accuracy. The analysis of both the estimates and the forecasts is based upon the *changes* in the series. These movements can, therefore, all be measured in logarithmic terms. Two hundred and ten observations (forecasts and estimates) on total GNP and specific individual components were available. In addition, some of the unconditional forecasts were corrected for policy changes which were induced by the forecasts.²

² The forecasts were prepared by the Central Planning Bureau of the Dutch government and thus could affect policy planning.

The characteristics of the econometric model from which the Dutch forecasts were generated include the fact that it is estimated in first differences and contains features which are particularly relevant for an open economy, but several important variables such as wages and exports were not included in early versions of the model. Theil evaluates the unconditional (*ex ante*) forecasts with the conditional (*ex post*) forecasts that would have been obtained if the predetermined (and in some cases two endogenous variables) had been predicted correctly and also develops a very important econometric technique. He shows how an equation can be added to a given model, thus replacing an exogenous variable and converting it into an endogenous one. The technique is also applicable if an endogenous variable were to be replaced with a priori information. These procedures again demonstrate the flexibility of econometric models. The actual comparison of the *ex ante* and *ex post* forecasts yields conflicting results but the former are not appreciably poorer than the latter. It is thus possible that in some cases a forecaster's judgment may modify the results generated by a model and improve their quality.

The analysis of the *ex ante* forecasts indicated that underestimates of the change accounted for 59 per cent of the observed errors. The results also showed that in the postwar period with its upward trend, a different type of error was observed when declines occurred. There were more failures to predict the declines than the advances, thus yielding more turning point errors and a smaller percentage of underestimates. The overall median inequality (U) coefficient was .56, exceeding 1.0, the value associated with no change extrapolations, for only one series. A more severe standard, based upon the error that would have resulted if each series' average yearly change had been utilized instead of the yearly predictions, was also used for evaluating the forecasts. The predictions for one-third of the series failed this stricter standard. Although Theil did not do so, it would have been interesting had the author compared the actual predictions with the forecasts generated by other standards such as the second naive model or some autoregressive structure.

Theil also notes that the pattern of economic movements is not known with precision until some time has elapsed. He, therefore, analyzes the accuracy of the earliest published estimates and determines the extent to which successive data revisions have been successful. The results show that the preliminary estimates, like the forecasts, understated the changes which occurred. In addition, only 63 per cent of the revisions were successful in reducing the absolute size of the error between the previous estimate and the change which was finally observed. Nevertheless, both the predictions and estimates improve with time and converge upon the true value.

Theil also shows how input-output tables may be used in forecasting intermediate demand, primary inputs, and sector inputs if final demand were known. Using the input-output coefficients of past tables, it is possible both to forecast the future and backcast for previous years. The older the table which is used, the larger are the errors which are observed in both the forecasts and backcasts. However, the input-output projections were superior

to extrapolations originating from final-demand-blowups when the coefficients were less than three years old. Theil also presents procedures which can be utilized to correct past tables in order to predict the future, thus providing a foundation for the methods which must be followed in order to incorporate input-output forecasts into macroeconomic predictions.

In Part IV, Theil shows how forecasts may be evaluated by that part of probability theory known as information theory. This method may be utilized whenever economic variables assume values that are all positive fractions summing to one³ i.e., the components of GNP expressed as a percentage of GNP. This approach cannot be utilized in evaluating forecasts of *changes*, which are often negative, and this procedure obviously runs counter to Theil's previously stated approach of only forecasting changes. Nevertheless, this method may be utilized to evaluate input-output and survey forecasts.

The information content of a reliable message stating that an event has occurred (i.e., the actual outcome) is related to the probability, p , with which the event was expected (i.e., forecast). The information content⁴ of a message is expressed as $\log 1/p$, and the information content of a forecast is high, i.e., accurate, "when the information obtained from the actual outcomes, given the forecasts, is small." When this method is applied to the input-output forecasts, the results indicate that the information content of the actual outcomes increases with the age of the table used. This result coincides with the previous analysis and reveals that the older the input-output table the less accurate are the forecasts.

In the final section, Theil analyzes the results of several European anticipations surveys. One of the problems involved in evaluating these surveys stems from the seminumerical form—*increase, decrease, or no change*—in which the anticipations are often expressed. In addition, it is difficult to quantify the meaning of "no change," for such a statement does not necessarily imply a zero absolute change, but rather a change within some specified small but not necessarily symmetric interval.

A retrospective analysis of German and Dutch anticipations data reveals a bias towards pessimism, which is consistent with analyses of the U. S. data. In the process of performing this evaluation, several techniques for estimating the limits of the "no change" interval were illustrated. These techniques were required to quantify the qualitative seminumerical data. On the other hand, a diffusion index type analysis of these survey data eliminates the need for quantifying no change statements, and furthermore it also has predictive value. The expected percentage change in a variable is a function of the difference between the number of survey participants who expect increases and the number who expect a decline:

$$\text{Log} \frac{X_t}{X_{t-1}} = a(X_t^1 - X_t^3),$$

³ It can be shown that the components need not sum to one, but they must still be nonnegative.

⁴ A related concept is the information inaccuracy of a forecast.

where X_t^1 and X_t^2 represent the proportion of individuals expecting increases and decreases respectively. Finally, Theil develops a method for approximating a survey realization-prediction table by a single number, which is then used to evaluate the results. Since a particular variable is often predicted inaccurately due to misestimates of some other variable, a statistical procedure for correcting the forecasts, by taking the other variables into account, is presented.

Theil has been able to develop the methodology of information theory and demonstrate its usefulness for evaluating input-output and survey forecasts. In both cases, the required statistical procedures for correcting the data may be formidable. Before this approach becomes generally acceptable, it is, therefore, likely that further methodological refinements are required. Whether information theory can be used for evaluating all economic forecasts or if its use will remain limited to specific types of predictions can only be determined in the future.

In summary, the collection of essays which Butler and Kavesh have edited has done little to lift the "aura of mystery" which has surrounded forecasting procedures. Theil, on the other hand, has demonstrated that predictions may be tied to scientific techniques. Theoretical advances in the field of forecasting will surely stem from the latter approach. Practical forecasters would clearly benefit and gain more influence if they, too, adopted this view, for they could provide more accurate predictions which would become more acceptable to decision makers. In addition, Theil's empirical results are highly relevant for the entire forecasting profession. It might, therefore, be appropriate for other economists to compare his results with forecasts of other Planning Bureaus or econometric models to determine their universality.

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the Federal Reserve System*

A Trade Cycle Analysis—Extensions of the Goodwin Model. By BJORN THALBERG. Lund Economic Studies No. 2. Lund: Studentlitteratur, 1966. Pp. 126.

As the subtitle indicates, this book presents some extensions of the business cycle model suggested by Richard Goodwin in 1951. In both models, the basic features are (1) a simple consumption function in which consumption depends linearly on income, with a possible lag, (2) a flexible accelerator type of investment function with a fixed capital coefficient, (3) the production capacity limit which depends solely on the existing capital stock. Prices, labor force limitations, monetary considerations, government fiscal activities, international trade and other matters are completely abstracted from these models.

The model considered in this book is an extension of Goodwin's model in two respects. First, while in the Goodwin model, the demand for addition to capital stock is equated to investment, in this model, the demand for addition to capital takes the form of addition to backlog of orders, and the backlog of orders become investment with a fixed time-lag. Secondly, there is only one

good in Goodwin's model, while in this book there are consumption goods and one or more capital goods. The author finds it impossible to describe the characteristics of his model by analytical methods, and he reports a few simulation results of his model as an alternative. The major finding of the book is that, when these additional complications are introduced, the Goodwin model still generates what appears to be a limit cycle, but its time path is rather irregular and depends on the numerical values of the parameters.

This reviewer regrets that he cannot recommend this book to either specialists or students. A model in economic analysis is useful either because it provides an insight into some basic feature of the economy in the simplest possible context, as did the original Goodwin model, or because it presents the best possible approximation to the reality, as many recent econometric models attempt to do. Thalberg's model does not meet these requirements. The model is too complex, and the author himself has abandoned any hope of dealing with it by analytical methods. The existence of econometric models makes impossible the claim of any model like the one in this book as the useful approximation to reality.

Even under these conditions, if the book contained an exposition of some new technique for dealing with some common problems in economics, it might be rewarding reading. Unfortunately, it does not contain any such expositions.

It is hoped that future volumes of the Lund Economic Studies will be selected with somewhat higher standards.

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Liquidity Preferences of Commercial Banks. By GEORGE R. MORRISON. Chicago: The University of Chicago Press, 1966. Pp. xi, 163. \$7.50.

Professor Morrison's monograph is a good empirical study of commercial banks' demand for cash assets and should prove to be stimulating reading for students of monetary theory. While the author's empirical tests cover the period from 1872 to 1955, the major emphasis of the monograph is on explaining the extremely large accumulation of excess reserves during the 1930s. Morrison examines several special hypotheses about the banking system during the 1930s and concludes that there is very little evidence for the widely stated view that the banking system was in a liquidity trap. Instead, he argues that the evidence supports the hypothesis that the bank's liquidity preferences shifted (i.e., the banks demanded more cash assets at any given level of interest rates) in response to the banking crisis.

Morrison develops a static, one period inventory model to describe a bank's demand for cash assets. For simplicity, the bank is assumed to be faced with a uniform distribution of cash flows. Given the random flow of cash, the bank can meet its cash requirements by holding excess cash as a buffer against ad-

verse cash flows and/or by borrowing at short term or selling earning assets. The model implies that the cash ratio varies directly with the penalty cost of meeting cash deficiencies and inversely with the expected return on earning assets and the expected cash flow.

Two special hypotheses are presented to explain the large accumulation of excess reserves following the banking crisis of 1933. They both relate to the bank's determination of the expected cash flow. The shock effect hypothesis states that "in a crisis or panic, banks immediately want to hold greatly increased amounts of cash because they have been shocked into expecting very large cash drains." There are two long adjustment lags in this model. First, the bank slowly adjusts its actual level of cash to the new higher desired level and, second, the bank slowly adjusts its expectation of a large cash outflow to correspond to the observed cash flows so that after a period of time the desired level of cash returns to a precrisis level.

The second hypothesis, and the one favored by Morrison, is the inertia effect hypothesis. In Morrison's basic model, expected cash flows are assumed to vary inversely with the difference between current and permanent deposit potential. According to the inertia effect variant of the shift hypothesis, banks revise their expectations about cash flows after a crisis by increasing the time period over which they determine their permanent deposit potential. This lengthening of the banks' horizon "tends to break the normally close link between current deposit potential and actual bank deposits" and replace it with a "more or less close link between movements of deposit potential and the excess reserve ratio." Since large inflows of reserves are expected to be only temporary, the banks hold large portions of these inflows in the form of cash. The inertia effect and the shock effect hypotheses both imply that monetary policy will be effective during periods when the banks hold large amounts of excess cash, but that it will operate with a considerable lag.

Morrison presents several pieces of evidence that contradict the idea that the banking system was in a liquidity trap during the 1930s. The most striking of these is his comparison of the Canadian and the United States banking experiences. Despite the fact that the economic environment in the two countries was very similar, both having suffered major economic depressions, the cash ratio at the Canadian banks remained approximately stable while the cash ratio at the New York banks increased rapidly. He argues that this difference in behavior can be explained by either the shock effect or the inertia effect hypothesis, since Canada did not experience any bank failures or even any significant runs on banks as the United States did. It behooves the liquidity trap supporters to explain why the cash ratio did not increase sharply in Canada as it did in the United States since fluctuations in interest rates and business activity paralleled those of the United States.

Morrison tests the two shift hypotheses by running multiple regressions on annual data from 1872 to 1914 for the New York Clearing House Banks and on monthly data from 1874 to 1913 and from 1921 to 1955 for New York banks. By including the earlier period, Morrison was able to test the hypotheses over five banking crises. The annual data show little support for the shock effect hypothesis. The call loan rate and the business failure rate are the

most important explanatory variables, but they fail to explain the magnitude of the cash ratio following the crises. The shock effect hypothesis performs better with monthly data where a more refined test can be made. It explains about one-half of the variance in the excess reserve ratio after crises. However, it performs very poorly after the 1907 crisis and also in 1933 and from 1939 to 1941. The inertia effect hypothesis explains the post crisis periods much better than the shock effect hypothesis. However, it does not do as well during normal periods. This is attributed to using a constant weight in calculating permanent deposit potential when the theory and the evidence both suggest that it should vary. Unfortunately, Morrison does not test for the statistical significance of the changes in the weight. As in the annual tests, the short term interest rate is highly significant, but it does not explain the magnitude of the cash ratio in the post crisis periods.

While Morrison's tests show that changes in the excess reserve ratio are highly correlated with changes in deposit potential following each of the banking crises, this reviewer is not convinced that this high correlation is due to a radical shift in bankers' memory spans. It seems very tenuous to base the explanation of the large accumulation of excess reserves during the end of the decade on the premise that bankers expected an outflow of reserves. After all, the three-year period from 1938 to 1940 was marked by an unprecedentedly large and continuous inflow of reserves. However, whether one agrees with the inertia effect hypotheses or not, Morrison's study is an important contribution to monetary theory. It presents a large amount of evidence and raises several interesting questions that will certainly stimulate further research about this important part of the money supply mechanism.

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Monetary and Fiscal Policy. By HAROLD A. WOLF. Columbus, Ohio: Charles E. Merrill Books, Inc., 1966. Pp. xviii, 478. \$9.25.

One of the advantages of decentralized decision-making is faculty freedom to experiment with course content. Those who are uneasy about the usual compartmentalization of monetary and fiscal policy—the first tacked on the Money and banking, the second on to Public Finance—will welcome this intermediate text by Professor Wolf.

The first of six parts serves to introduce the distinction between money and capital markets and to identify several sources of market demands and supplies. With the exception of one short chapter on the Quantity Theory, Part 2 covers the banking side of Money and Banking: money; Federal Reserve and commercial bank organization; mechanics of bank credit creation; money, reserve, and credit supplies; and creation of member bank reserves. Part 3 takes up nonbank financial institutions. Three of the five chapters of Part 4 cover the money side of Money and Banking: types of securities and the family of interest rates; neoclassical, Keynesian, and loanable funds interest theories; and the impact of rate changes on borrowers and lenders. A fourth chapter reviews the Fed's performance over the years and a fifth summarizes the Treasury's impact on financial markets.

Part 5 covers a variety of topics related to fiscal theory and operation. There is a chapter on national income accounting and determination; one titled, "The Budget," which explores, among other things, tax shifting, burdens, and superiority of income over excise taxation; one on fiscal policy which explores the impact of government taxes and expenditures, functional finance, and Musgrave's multiple budget; one on the burden of the debt and debt management; and a summary chapter on monetary, fiscal, and debt policy coordination, its practical difficulties, and a record of Fed and Treasury cooperation.

The three chapters of the final section attempt to deepen understanding of the meaning of inflation and deflation, of the meaning of economic growth and stability, and to introduce the world of international trade and finance.

Rare indeed is the intermediate text which spans money and banking, monetary policy, fiscal policy, and debt management. One can only admire the vision which inspired such an undertaking. One can only stand in awe of the extent to which Wolf succeeds in bringing it off. Considering that an introductory text usually devotes some 300-400 pages to the same topics, this is no mean accomplishment.

To be sure, under pressure to be inclusive and yet remain within the relatively limited confines of one text, there is danger that some propositions may be gutted of their vitality or spirit. As a case in point, behind Friedman's argument for a steady-growth money supply is the notion of a more, rather than less, predictable environment. This is at once a fundamental and powerful notion. Yet on the page devoted to developing Friedman's position, "removing uncertainty" is mentioned in only one sentence (p. 389). Here and elsewhere, individual instructors will want to bring the text to life in their classroom discussions.

To those who would welcome the spanning of monetary and fiscal theory, the text as it now stands may be something of a disappointment. On the one hand, Wolf is careful to underscore the desirability of monetary-fiscal-debt policy coordination, but he does little in the way of suggesting that something deeper is involved than mere coordination. On the other hand, Wolf repeatedly faults *monetary* policy and controls as being only weakly or modestly effective when he means (or should mean) *present Federal Reserve* powers (e.g., pp. 217, 272). Taken together, there is danger that students will arrive at the end of their reading with a distorted picture of the issues involved.

One thinks of "monetary" theory as having to do with money—in particular, the quantity of money at work in an economy. For some purposes, *MV* may be an adequate expression; for others, some less aggregative formulation may be necessary. "Monetary" policy presumably refers to policy with respect to the determinants of the monetary variables. If it is clear that, say, central bank policy operates on *MV*, in a money economy it should be made equally clear that fiscal policy effects its results by operating on *MV*. The need for coordination arises precisely because fiscal actions to raise or lower *M* or *V* may be nullified by actions of the central bank—and vice versa. Both policies, properly, are subsets of the possibility set of monetary policies. At some point,

students of economic policy should come to understand that, in a fundamental sense, one does not choose between monetary and fiscal policy, but from among sets of actions by monetary and fiscal authorities.

Limiting "monetary policy" to those actions undertaken by monetary authorities should neither conceal the full range of possibilities nor the essential similarities of many alternatives. On stabilization grounds, there is little difference between the Fed directing commercial banks to increase depositor accounts by some percentage (taking care to provide the needed additional reserves) and the Treasury mailing out checks to depositors in the same amount. Similarly, there is little to choose between the Fed levying a supplementary "stabilization income tax" and the Treasury imposing higher rates to combat inflation. Blanket condemnation of monetary policy suggests that the Fed's actions would be less effective than analogous actions undertaken by the Treasury. (That neither at present has such powers in no way obviates the need for continuing clarity.)

The prolonged controversy between "monetary" and "fiscal" theorists has been sharp enough without raising another generation in its stead. To a greater extent than Wolf conveys, the controversies turn less on whether the actions of one or the other authorities more effectively promote full employment or price stability than on the implications for income distribution, for resource allocations between so-called public and private uses, for capital formation and economic growth, and on *where* authority to initiate policy should lie.

As a text, perhaps one should note also that adopters may experience some assignment problems. For students innocent beyond the introductory level, one is awed by the thought of adequately covering money and banking, monetary policy, and fiscal policy in one semester. Yet for a two-semester sequence, adequate depth and detail will require extensive supplementary reading. If students have behind them an intermediate Money and Banking course, the attention accorded the subject here is rather too extensive for review purposes. One hopes that when Wolf undertakes a revised edition, he can be persuaded to eschew the mechanics of banking in favor of a much expanded treatment of the interplay between monetary and fiscal theory and policy. If so, both colleagues and future students will owe him a debt of gratitude.

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Public Finance; Fiscal Policy

Federal Budget Projections. By GERHARD COLM AND PETER WAGNER. Washington: Brookings Institution, 1966. Pp. xx, 194. \$2.50.

Here is a book that, it is hoped, will make a difference in economic policy. Following the after-glow of the needed and wholly desirable 1964 tax reduction, we have begun uneasily to realize that a lot of problems remain in the operation of a rational and orderly fiscal policy, particularly once the narrower tolerances of a fully employed economy are regained. Indeed, prospects for the

budget have become themselves one of the major uncertainties in the economy (a fact well illustrated by events in 1966). The federal budget now embraces transactions of a magnitude equal to about 20 per cent of GNP, but we seem to stumble from one annual budget battle to the next with no real information about where the operation is going in the longer run.

We need a longer-run horizon here also if the citizenry and the Congress are to make intelligent decisions about federal programs. Should the federal government undertake a new program? Is one program more important than some other candidate? Obviously these questions cannot really be well answered in the absence of information about the rapidity and the magnitude of the budget's build-up for several years ahead. We need, in short, a longer budgetary horizon both for stabilization policy and also for the orderly operations of the public sector itself.

Can meaningful projections of the federal budget be made for several years ahead? In this study the authors endeavor to answer that question, and they have firmly established an affirmative answer. Their objective was not to provide an estimate of the budget picture for five to ten years ahead that they personally would prefer, which would be a relatively easy thing to do. Rather they have attempted to see if, through a close study of pertinent evidence, it is possible to project the future build-up of existing programs and to make some guesses about the impact of new programs during this span of years. In this effort they recognize that budget making must proceed simultaneously in two directions. It must proceed from an evaluation of individual programs to the total whose aggregate they represent. And a provisional decision must also be made about the total, the shares of which will then be allocated to the individual program claimants.

The results of Colm and Wagner's work clearly demonstrate that useful projections can be made showing where the budget is going for a period of years ahead. With this information, projections and forecasts for the whole economy could be more certain. The Congress certainly needs careful estimates of the implications for the longer run of near-term expenditure decisions. And it would be good for the Administration to lay out its longer-range strategy for the budget.

PAUL W. McCRACKEN

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Public Finance and Welfare—Essays in Honor of C. Ward Macy. Edited by PAUL L. KLEINSORGE. Eugene: University of Oregon Books, 1966. Pp. vii, 305. \$7.50.

Collections of essays like this one are always more attractive to the reader than to the reviewer. The latter searches for a unifying theme to agree with or dissent from, to provide a point of view for the review. But the reader is interested in the essays, or some of them, for themselves: never mind the fact that they are only loosely related.

So it is with this volume. Despite the title (and the publisher's book jacket claim), only a dozen of the 17 essays have anything to do with public finance

and, for two of them, the word finance is entirely inappropriate. Most of the essays deal with fairly topical policy problems; a few are rather more abstract. The level of discourse includes serious workman-like contributions to the professional literature, graduate-student-type essays, and journalistic pieces. There is similar unevenness evident in quality control (or rather, the lack of it) and in the strange arrangement of the essays (e.g., an essay on the property tax is presented under "National Tax Problems").

Of the essays on public finance, three are theoretical in nature. The best of these is Barry N. Siegel's essay, "On the Positive Theory of State and Local Expenditures." The literature contains valuable work on the normative theory, cast in terms of social and merits goods and wants, and also a good deal of empirical work on the determinants of state-local expenditures. Most of this lacks any hypothesizing or theoretical framework and consequently the search for variables often appears rather casual. Siegel attempts to outline a positive theory to explain expenditures. It is a bare outline, but an interesting addition to thinking in the field. The essay includes a good summary of previous empirical work, which is, if anything, too kind to the more recent imitators of the pioneering work.

George Babilot presents a useful summary of views and positions on some of the basic theoretical problems in welfare theory concerning taxation. These include: indirect versus direct taxes; the difficulty of discovering truly second-best solutions in situations in which "ideal" assumptions do not obtain and taxes are to be used as correctives; and the havoc caused by considerations of time and uncertainty. His message, stated explicitly, is that welfare theory has few answers for the policy problems of the type which must be resolved continually. Paul B. Simpson has an essay in which the tax rate schedule is treated as a democratically arrived at compromise between community desires for greater equality and for more aggregate real income. It is a showy but sterile job.

On the more topical level, the best essays include John F. Due's very good review—the only available brief review of this subject—of the form and context of value added tax proposals in the United States from the 1920s on, and Robert W. Harbeson's first-rate piece on "Some Unsettled Issues in Highway-Cost Allocation." The latter is essentially a critical review of the Bureau of Public Roads highway-cost allocation study. As Harbeson shows, the BPR study does not face up to the basic question of the scope of the commercial principle in highway finance and consequently neglects the basic conceptual issues. He is less critical, in fact, than he could be, since he places the blame on the Congressional directive for the study, not on the BPR's slavishly narrow interpretation of the directive. He calls for a study of the theoretical issues. But, as he shows, they are not really unsettled; the problem is that most people (including many economists) do not accept the obvious implications of economic analysis.

In a long essay, Lester B. McAllister, Jr., lucidly examines alternative national defense strategies (ranging from the "massive retaliation" of the late Eisenhower years to all-out disarmament) and the actual or hypothetical bud-

gets attached to each of these strategies. He argues strongly for the McNamara policies. In another long essay, Harold M. Somers reviews the arguments for and against the taxation of capital gains at death, as considered by Congress in 1963-64. His review persuaded me that the idea is a good one, but he concludes that the negative arguments prevail; in any event, he sees little hope of congressional acceptance. Robert B. Bangs reviews income tax depreciation policies, especially the reserve ratio test of the reasonableness of depreciation deductions, which went into effect in 1965; he finds the tests too inflexible.

Two essays deal with policy problems on the state-local level. Both are rambling, discursive and generally unexceptionable pieces. One is by C. Lowell Harris, on the property tax, and a second is by Howard R. Bowen, on the financing of higher education. Coverage of public finance is completed with two papers on Latin American public finance. Robert Loring Allen writes on "Regional Development and Public Finance in Venezuela." He argues from Venezuelan experience and from a simple analytical model that "unbalanced" regional growth policies are likely to be best for developing countries. That is, the greatest rate of overall growth will result from a policy of concentrating (governmental) efforts in the already most developed regions, as has been done in Venezuela. His conclusions appear eminently sensible. But one would think that Latin Americans need little inducement to do this. As Raymond F. Mikesell points out (in his essay on "Inflation and Growth") the "system" in Latin America is loaded with advantages for the "haves" and this applies to regions as well as people. Moreover, in countries with centralist traditions, the capital regions are natural favorites. The other piece on Latin America is Milton C. Taylor's essay on tax incentives in Panama, with suggestions for improving—mostly reducing—Panama's rich variety of tax incentive devices.

This is not an outstanding collection. There are relatively few pieces one would want to include in a graduate-level reading list, or read more than once. It raises once more the question of the value of books of this type; a lack-luster collection of essays cannot add to the repute of the person in whose honor they are said to be written.

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International Economics

Trade, Growth, and the Balance of Payments—Essays in Honor of Gottfried Haberler. Edited by R. E. BALDWIN AND OTHERS. Chicago: Rand McNally; Amsterdam: North-Holland, 1965. Pp. viii, 267. \$8.50.

A distinguished group of economists has contributed articles in the field of international economics to commemorate the sixty-fifth birthday of one of the great figures in the field, Professor Gottfried Haberler. Problems of resource allocation and comparative advantage, the interactions between trade and development or growth, and monetary aspects of balance of payments problems all receive attention in this volume as they have in Haberler's own works.

Fourteen candles adorn the cake, and it is perhaps not surprising that some shine more brightly than others.

The two lead articles, contributed by Harry Johnson and Paul Samuelson, will probably receive the most careful attention from graduate students. In a rather lengthy but solid introductory essay Johnson discusses the welfare effects of tariffs, subsidies, and other government policies in the face of an assorted array of domestic distortions such as price rigidities (at both goods and factor levels), factor immobilities, and external economies or diseconomies in consumption and/or production. This is the land of "the second-best," and the terrain is not smooth.

Domestic distortions can either prevent the economy from being at the socially optimal point on the transformation schedule or push it below the schedule, or both. A major question concerns the effect of policies that interfere with international trade—tariffs on imports or subsidies on exports—in counteracting these distortions, especially when compared with other policies that do not discriminate between domestic and foreign consumers or producers. The point is that the optimal tariff argument emerges as the basis for any interference in trade. Domestic distortions are better corrected by other means. The essay relies on verbal and graphical arguments in illustrating the various possibilities.

Samuelson's essay, "Equalization by Trade of the Interest Rate along with the Real Wage," has been in private circulation for a few years. The question (raised by J. Bhagwati in personal correspondence) is whether the "factor price equalization" theorem implies that interest rates are equated internationally. The general answer, even with the assumptions employed in the standard Heckscher-Ohlin models is "not necessarily." The point is that by the standard theorem trade equalizes the *rentals* on capital goods (or the rents on land, or the wages of workers)—the price of capital goods needs also to be equated if the interest rate is to be equalized. In Samuelson's model this is achieved by introducing a (new) price equals cost of production equation for the capital good, with the same technology as between countries. His formal analysis includes a compact, and relatively simple, treatment of the case with any number of heterogeneous capital goods, as well as intermediate goods.

Purely theoretical essays are also contributed by Bhagwati and P. Kenen. Bhagwati examines the proposition that quotas and tariffs have equivalent effects on prices when they have the same impact in controlling imports. He argues that the equivalence depends on an assumption of competition at home and abroad and among quota holders. For example, a domestic monopoly equates marginal revenue and marginal cost, and the former will be higher with a tariff than with a quota (since *foreign* production, and through this the net demand facing the domestic monopoly, is sensitive to a price change under a tariff). Kenen presents a detailed unified treatment of the activities of an export-import firm engaged in foreign exchange speculation and forward exchange transactions. The typical firm is assumed to have a set of indifference curves relating expected net worth and (with a negative sign) risk, as expressed by his net long or short position in foreign exchange. Imports must be financed with bank credit, with interest charges rising (in either country)

with the level of borrowing. In analyzing the optimal decisions made by the firm, and the stability and comparative statics properties of the model, Kenen employs a more mathematical approach than elsewhere in this volume. This, combined with the nature of the topic, may unfortunately restrict the range of readers attracted to the essay.

The contributions of R. Caves and W. Stolper also deal with theoretical topics. Building on the work of D. Bensusan-Butt and others, Caves' essay is a beautiful literary exposition of "Vent for surplus" models of trade and growth. In contrast to the smooth neoclassical growth mechanism, these theories assert the establishment of industries (based, say, on the discovery of special natural resources or raw materials) in response to a *disequilibrium* situation, with rates of growth dictated by the speed of response (e.g., in movements of capital and/or labor) to profitable opportunities. Growth proceeds in "spurts," and those who think the real world is like this will be attracted by the model. Stolper's essay, "The Multiplier if Imports are for Investments," worries about the effect of a change in the terms of trade and challenges the standard Laursen-Metzler argument that a deterioration in the terms of trade serves to raise aggregate spending out of a given level of money income. His argument, unfortunately, is loose and not clearly stated.

The remaining essays are of a different nature. On a strictly historical level, Baldwin's essay provides an interesting discussion of the obstacles encountered by U.S. negotiators in recent tariff bargaining with the EEC. F. Machlup presents a rather taxonomic discussion of the types of action that can affect a balance of payments imbalance. Bertil Ohlin points out that the need for integrating social and economic policies as a precondition for successful free trade among a group of countries has been greatly overstated. In the opposite vein, R. Kamitz, in the most propagandistic essay of the volume, argues strongly for monetary stability as an essential prerequisite for economic integration. Whereas Kamitz would cut down the role played by government, J. Tinbergen is concerned with planning, and the value of making a distinction among international, national, regional, and local industries, based, usually, on considerations of transport cost. At a national level there need be a balance between production and demand for all categories except the first, and this leads him to consider a "semi-input-output" method. The volume concludes with three essays discussing recent monetary developments in particular countries. J. Niehans finds some interesting relationships (and a high correlation) between the import surplus and interest rates in Switzerland. And, to illustrate that international trade economists study *other* countries, C. Kindleberger reviews nine possible explanations for the persistent German surplus, while E. Sohmen concentrates on the one underlying influence affecting the U.S. balance of payments—the degree of competition in U.S. industry. Not surprisingly, he picks on steel.

There is much here to sample. In reading these contributions one can legitimately ask whether Professor Haberler has had something interesting to say about the same issues. The answer, usually, is in the affirmative.

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Synthetic Materials and the Theory of International Trade. By G. C. HUFBAUER. Cambridge: Harvard University Press, 1966. Pp. 165. \$6.50.

Kravis, in his remarkable article on "availability," pointed to the importance of trade originating not from close cost and price comparisons, *a la* Heckscher-Ohlin, but from the temporary monopolies that producers of some nations obtain by developing new products or differentiating older ones. Others, notably Hoffmeyer, Kindleberger, Burenstam-Linder, Posner, and now Hufbauer, have accompanied Kravis down this byway of trade theory with sufficient frequency that the path begins to resemble a respectable if still secondary route towards understanding recent patterns of manufactured goods trade. It has turnpike potential.

Hufbauer begins this short monograph with a terse recapitulation of three nature-of-trade theories which he wishes to test for relevancy against the experience of "footloose" industries, particularly the synthetic materials industries of plastics, synthetic rubbers, and man-made fibers. He makes short work of the factor proportions account of trade by noting that its assumptions of equal technology and constant returns to scale are inconsistent with known conditions in the synthetic materials industries. He holds open the possibility that the scale economy account, encompassing *both* static and dynamic economies, may help to explain synthetic materials trade, and properly shows that artificial trade barriers and variations in home market size, as well as significant scale economies, must exist for this to be so. But Hufbauer's principal exposition of trade theories is reserved for the technological gap account which clearly he regards as the main explanation of synthetic materials trade.

In his mind the technological gap account of trade involves far more than the temporary exports arising initially from, say, country A's industrial breakthrough. Even as country B learns how to imitate A's new development and builds its own plant, country A may reinforce its lead by harvesting static and dynamic economies, that is to say, by being first to erect an optimally sized plant and by learning-by-doing. The completion of production capacity in B, on the other hand, starts B along the learning curve and may eventually result in the replacement of B's technological gap imports with domestic output. Hufbauer's technological gap account need not end here, however, especially if wage costs are lower in B than in A. In that case country B may begin exporting "low-wage goods" to A which may be simply aging versions of the same industrial products previously exported by country A. Eventually this trade too will cease as wage disparities between countries A and B shrink or as country A develops still newer products superseding B's more traditional goods. For Hufbauer then the full technological gap account consists sequentially of technological gap trade followed by low wage trade.

This suggests, he concludes, that technological gap trade and low-wage trade usually flow in opposite directions within a trading partnership. Even so, a given country in a multicountry world may export technological goods to one set of trading partners and low-wage goods to another; the pattern depends upon the relative length of all partners' "imitation lags." Hufbauer's technological gap account therefore implies a "pecking order" of trade, populated at one end of the array with nations enjoying relatively short imitation

lags who export newer technological goods, and at the other end with poor nations whose lengthy imitation lags force them to specialize in more traditional low-wage goods.

In succeeding chapters Hufbauer tests the factor proportions, scale economies, and technological gap hypotheses for relevance to known patterns of synthetic materials trade. Yet again it is the technological gap hypothesis which receives the lion's share of his attention. From the development histories of synthetic materials like galalith, celluloid, polyethylene, and polyvinyl chloride, for example, he gleans several illustrations of trade patterns which fit nicely the expectations of his hypothesis. Another test which compares product ages with the direction of trade flows shows that for a number of advanced countries, and especially for the United States, synthetic materials exports are heavily biased towards newer products compared to those of other countries.

With yet another family of statistical tests, Hufbauer attempts to explain national shares of world synthetic materials exports with variations among countries in the aggregate imitation lag (for which each product's imitation lag is weighted by that product's share of current world exports), wage levels, the size of home markets (G. D. P.), and the accumulated volume of past production. He finds that the imitation lag helped considerably to explain export shares but that trade in only one of the product groups, man-made fibers, was responsive to the level of wage rates; this and other findings seem to suggest that low-wage trade has not been a very significant part of synthetic materials commerce. Home market size helped to explain trade shares for plastics and synthetic rubber products but not for man-made fibers, and accumulated past production proved somewhat helpful for all product groups. These four variables typically explained about ninety per cent of the variance in national export shares.

Hufbauer concludes that the technological gap theory and the scale economy theory together explain most of the trade in synthetic materials. Since some of the explanatory variables are themselves highly correlated, e.g., the imitation lag and home market size, Hufbauer's multiple regression equations, however, do not yield an approximate division between the two accounts of trade.

Anyone who has tried to understand the shifting currents of international trade over the last twenty years, and has felt the frustrations of working with elegantly polished but outdated analytic tools, should welcome and applaud this book. It brings innovations and scale economies to center stage in the theory, where they rightfully belong, and supplies the profession with the richest evidence yet of the relevance of the technological gap account to contemporary world trading patterns. The book does not reject comparative cost doctrine; rather it digs more deeply into the nature of international trade and comes up with a plausible account focusing less on static conditions external to individual industries and more on dynamic developments within each industry.

Hufbauer's technological gap account generally improves on the theory's earlier versions. In one respect, however, it regresses: it scarcely mentions the possibility of *mutual* technological gap trade being carried on *among* the richer

nations heading the trade pecking order. Yet by latest count approximately 68 per cent of world manufactured goods trade is carried on between industrial countries with increasingly similar technological skills. Is it not probable that a rising proportion, perhaps even a majority, of these nation's intragroup exports *and* imports is based on product differentiation or technological head starts? And does this not suggest that trade theorists should be as much concerned in their new quests for understanding with the sources of invention and innovation as with the processes of imitation?

Hufbauer also ignores all demand lags and measures imitation lags as the time between first commercial production in the innovating and imitating countries. Both procedures are potential sources of bias in his results. Sometimes it takes years for new goods to penetrate foreign markets, e.g., the Volkswagen in America; demand lags of this length influence the length of the trading period, making the imitation lag alone a poor measure of trading potential. My own research also suggests that a better way to measure imitation lags is to calculate some index of the rate of diffusion of an innovation within the industries of the innovating and imitating countries (*a la* Mansfield), and compare them; this requires information on more than the premier firm in each country, but it produces a much more accurate measure of national technological differences.

The book is cogent, well written, and to the point. It does not suffer as many published theses do from cumbersomeness, excessive jargon, and vast forests of charts, tables, equations, and footnotes. Most of the technical data have been pushed to the rear in orderly appendices. Apart from specialists in international trade who *must* read the book, students at the upper division as well as graduate levels would do well to study it, for the book is a model of methodological crispness and imaginative empiricism.

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Gold and World Power: The Dollar, the Pound, and the Plans for Reform.

By SIDNEY E. ROLFE, WITH THE ASSISTANCE OF ROBERT G. HAWKINS.
New York: Harper & Row, 1966. Pp. viii, 276. \$6.95.

Gold and World Power consists really of three short monographic essays: (1) an elementary but thoughtful and balanced elaboration of what might be called the monetary facets of international economics; (2) a critique of the various categories of proposals to alter the international adjustment mechanism or, alternatively, to augment international liquidity; and (3) a reasoned discussion of the recent strategies developed by the United Kingdom and the United States to cope with their balance of payments deficits. In each of these areas the book must be judged a success. Essential ideas and concepts are adequately covered, while peripheral and trivial issues are given proper perspective or ignored altogether. The exposition is lively and provocative. There is a felicitous blend of the theoretical, institutional, and historical approaches, with the significant consequence that abstraction is little in evidence. In many instances, rudimentary empirical evidence is imaginatively brought in to support argument and hypotheses, but it would be inaccurate to imply that the

messages of the book are adduced from quantitative materials. All this is accomplished at a level of difficulty tolerable to any conscientious undergraduate economics major and to thinking men involved in practical economic affairs. Many professional economists also will be instructed by the volume, and even specialists in international economics will find challenge and stimulation in the two chapters on U.K. and U.S. balance of payments policies.

Beyond these comments there is little to be gained by reviewing the first and second topics, or "essays," mentioned above. Professor Rolfe takes a standard position favoring improvements in the efficacy of the adjustment mechanism as a longer run target, with interim measures to enhance the liquidity of the international monetary system. Less conventionally, he seems to show a slight, although not unequivocal, preference for greater flexibility of exchange rates—even for reserve currency countries—than is currently permitted by institutional conditions and conventions. In this regard it should be noted that the author too facily glosses over the internal disruptions that may attend exchange rate flexibility. Exchange rate adjustments are a more painful remedy to balance of payments difficulties than they are here made out to be. Reallocative rigidities are not unimaginable, and some attention ought surely to be given to potential reallocative problems precipitated by exchange rate movements. Also, in his analysis of the international monetary system Rolfe persists, like nearly all economists from the North Atlantic nexus countries, in discounting the real needs and demands of the developing nations, even in the face of mounting evidence that these can no longer be realistically subordinated. I would not, however, want unduly to stress these criticisms because, on the whole and given the primer-like quality of the discussion, the two topics are competently treated.

The chapter on U.K. balance of payments tactics undoubtedly will be the most fascinating to U.S. readers. Rolfe points out that the British effort is proceeding at three levels. In the short run, the current account position is to be improved by the well-known "stop-go" aggregate demand policies. He holds reservations, however, arguing that the "stop-go" measures have been inappropriately timed since they have been tied to changes in industrial output rather than to the lagging balance of payments changes. He suggests that controlled devaluation (during periods of unused aggregate capacity) would have been more effective. In the long run, a comprehensive plan for accelerating overall productivity improvement is being implemented, although doubts remain that existing government powers are up to the task. In the intermediate-run, an incomes policy has been pursued, so as not to dissipate any gains achieved via the short-run measures. Again, Rolfe finds fault with an incomes policy, preferring rather that a controlled gap between expanding aggregate supply and demand be the instrumental variable, as has been propounded by Dow and Paish. I intend by these comments to suggest that Rolfe presents an analytical treatment of recent U.K. balance of payments policy; I recommend it highly to those not already familiar with the debate among our British colleagues.

The chapter discussing the U.S. response to its payments deficits contains less of interest, possibly because it covers more familiar ground. However, it

gives welcomed underlining to an important segment of U.S. policy that is too easily overlooked. The early differentiation in the focus of monetary policy, on the one hand, and fiscal measures on the other is, of course, mentioned. The former was redirected to induce retardation and reversal of international capital movements while the latter was designed to achieve purely domestic goals. But Rolfe stresses that these conventional measures have been augmented by reviving a host of direct controls, primarily on capital movements, that were initiated some thirty-odd years ago. These controls were, it was thought, outmoded by the world trading and payments system built up in the postwar era. Their revival in the United States is quite possibly indicative that the payments and trading system itself is dated.

M. O. CLEMENT

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The Ohlin-Heckscher Theory of the Basis and Effects of Commodity Trade.

By J. L. FORD. New York: Asia Publishing House, 1965. Pp. viii, 88. \$5.75.

This brief book is a summary of the literature on the factor endowment basis for international trade, together with a discussion of the factor price equalization theorem, and a critique of Leontief's input-output evidence on the factor composition of U.S. foreign trade. Chapters 2 and 3 provide a careful restatement of Ohlin's contribution, while Chapter 4 offers a critique based mainly on the underlying assumptions. Chapter 5 is an exposé of complete factor price equalization with two factors and two commodities; while Chapter 6 is devoted entirely to Leontief's empirical findings relative to the Ohlin-Heckscher proposition that factor supplies are the principal determinant of commodity trade.

The reader who is acquainted with this literature can without loss start on Chapter 3, which discusses Ohlin's modifications to the standard assumptions basic to the theory of international specialization. These include perfect competition, constant returns to scale, qualitative uniformity of factors between countries, and fixed factor supplies. The absence of the first of these, according to Ohlin, left room for unused capacity. This meant that selling price might well be below marginal cost. In addition, the presence of political and economic instability of differing degree among trading countries, meant that prices would depart by differing percentages from marginal cost. Without pricing in proportion to marginal cost one could not say precisely how far the equalization of prices through trade would also pull factor rentals into alignment.

The presence of increasing returns when countries specialize on fewer lines of production complicates the picture further. By chance, a country might end up producing at low cost an item for which it did not have a plentiful endowment of cheap factors. Consequently trade might cause high-priced factors to become still scarcer, and move factor prices further apart, between the trading countries.

The problem of factor qualities, too, received its due from Ohlin. The now familiar point is made that higher wages can be outweighed by higher quality

of labor, so that a high-wage country may be an exporter of labor-intensive goods. A further complication is on the commodity side where the same goods may have a number of type and quality variations, such that trade in one class of goods can go in both directions at once. One can then not say clearly that relative factor supplies determine the flow of trade.

Finally, there is the influence of prices of factors on supply of labor and capital. The supply of either factor tends, in Ohlin's view, to increase with its price. The consequence of this is that trade, in making an abundant factor dearer, will also stimulate an increase in its supply. Thus unevenness of factor endowments tends to be increased by the forces of trade. While all of these points are now familiar and some have been improved on, it is interesting to remember that they were considered some thirty-five years ago by Ohlin. Ford's summary may induce some readers to reread Ohlin's book.

Chapter 4 is mainly a restatement and elaboration of the points above, together with a discussion of differences in methods of producing the same goods. Under this heading are two distinct cases. The factor-intensity reversal case emphasized by Pearce and others; and differences in technology. Here Ford points out correctly (pp. 27-29) that it is difficult to predict what the expansion of trade will do to affect factor scarcities. In the two-country, two-factor, two-good case, one can show that when country *A* produces good *X* in a labor-intensive way compared to *Y*, and *B* makes the same good in a capital-intensive way, either country may become the exporter of *X* to the other. Suppose it is *A*. If *B* is well endowed with capital and *A* is labor-plentiful, then in *A* the plentiful factor would properly be used more intensively than before. But in *B* there is also a shift to the labor-using good. Thus we can not say that trade generally makes abundant factors scarcer, since it does the opposite of this in *B*.

The author provides a useful discussion of the Leontief paradox in Chapter 6. Leontief, using an input-output matrix has calculated the labor and capital coefficients for export and for import-competing goods. The finding was that import-competing goods were capital intensive, compared to the exporting sector. The conclusion drawn was that U.S. labor efficiency is so great as to make this country in fact labor plentiful compared to the rest of the world. Ford's objections to this are: (a) productive practice abroad may be markedly different from U.S. practice, so that goods which here are capital intensive may be labor intensive abroad. This may be particularly true of some import-competing extractive industries. (b) related to (a), if we remove from the comparison those sectors where natural resources play a large role, the empirical finding is reversed, conforming to expectations that our exports ought to be capital-intensive. The conclusion drawn is that, if (b) were not there to bail us out, we still have (a) to fall back on, so that the Ohlin-Heckscher proposition is not seriously threatened.

The discussion on empirical studies relating to factor intensities would have gained in completeness by considering the work of B. S. Minhas (*Jour. Pol. Econ.*, Apr. 1962). Minhas ranked 20 comparable industries involved in international trade for the United States and Japan, as to labor-capital ratios. He found that there were numerous changes in rank ordering between the two

countries, so that the Spearman coefficient of rank correlation was rather low. Taken on its face, this would tend to weaken further the expectation that countries tend generally to export those goods which embody their plentiful factor. By the argument used earlier, one of the countries may well be pushed through trade into the "wrong" specialization.

In this reviewer's opinion, however, Minhas provided a test which was inappropriate to the question above. The breakdown of factors and commodities is to a large extent an arbitrary matter: it can be made as fine or as coarse as is appropriate to the question at hand, subject to limitations imposed by the data. For the proposition that trade between the United States and Japan tends to cause capital-intensive goods to move west and labor-intensive goods to go east, and its corollary that the two factor-price pairs move toward equality, we need only consider two industry groupings. A short-cut, using Minhas' 20 industries (see his Table V) is to see how many change position from the upper half to the lower half of his table (and vice versa). Just four out of ten switch membership from the capital-intensive to the labor-intensive camp. Consequently, if we take the 20 industries as having equal importance (in the lack of more complete information) we conclude tentatively that factor intensities in Japan are by-and-large the same for these 20 foreign-trading industries as for the United States. Contrary to Minhas' own conclusions, his data tend to support mildly the Ohlin-Heckscher proposition.

To sum up, this book, while not remarkable for originality, is a useful introduction to an important topic in the theory of international trade.

FRANZ GEHRELS

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An Econometric Study of International Trade Flows. By H. LINNEMANN. Amsterdam: North Holland, 1966. Pp. xiv, 234. \$7.80.

Is the classical approach to econometric research in international trade correct? That is the central question left in the reader's mind after digesting the results of the very different approach used in this study.

Classically, research has been concerned with evaluating elasticities of demand and supply for exports and imports, based on data for various countries and for various commodity groups. It was hoped that such estimates would throw light on the stability of the international exchanges, to allow predictions of trends in international trade, and the like. I think it is now common ground among all students of the subject that the results of the past 30 years' work have been disappointing; and one can hardly put the blame on a shortage of numerical data. Time series on foreign trade in immense detail have been available for many decades, and in some countries even for centuries.

Without going too closely into what has so far been achieved (or not achieved!) in this field, Dr. Linnemann in this study simply decides to examine the matrix of trade flows among 80 countries in 1959; treating exports and imports separately, there are thus some 6000 flows. He tries to fit these observations into a mathematical scheme with the help of a couple of electronic computers (situated, in the way research goes these days, in Rotterdam and Cairo respectively).

To the classically trained economist, this may seem another dull cross-sectional exercise in statics and, though he would concede the possible existence of some fascinating numerical problems, he might easily be tempted—having appreciated the point of the exercise—to put down the book. He would be wrong to do so. There are indeed some tedious chapters going through three sets of laborious calculations on virtually the same model, but leading eventually to Chapter 7, on the implications of the empirical findings, which justify (at least to the reviewer's mind) the immense labor put into this project.

The basic model used throughout the analysis posits simply that the imports by *A* from *B* are greater, the greater is the national income of *A*—due to demand factors—and the greater is the national income of *B*—due to supply factors. However, as countries grow in size there is a tendency to greater self-sufficiency, and for this offsetting factor Linnemann introduces as additional explanatory variables the populations of *A* and *B*, to which the regressions in practice always attach a small negative elasticity (about -0.2). It is not very clear from the discussion why population is the correct variable for this purpose (rather than, say, national product), and whether the separate positive and negative effects of a country's size can be isolated in this way. The reader may therefore prefer to think of national-income-per-head and population size as the two equivalent variables (i.e., mathematically equivalent to national income and population size used by Linnemann) which enter into a country's demand and supply of foreign-traded goods, and as having somewhat different quantitative effects, but being positive in both cases. On the latter interpretation, Linnemann's results indicate that a rise in national-income-per-head has a greater proportional influence (elasticity about unity) on trade than does a rise in population (elasticity about 0.8).

The geographical distance between *A* and *B* is introduced into the analysis as a further variable to represent the natural resistances to trade, whether due to difficulties of commercial contact or costs of transporting the goods. In addition, dummy variables are introduced to measure the average effects of trade preference within certain trading blocs (British Commonwealth, France, etc.); and, as an afterthought, there is introduced an index measuring the degree of matching of the commodity composition of *A*'s total imports and *B*'s total exports.

Thanks to the large number of observations and their wide range of variation, the parameters of the regressions are all estimated with a fair degree of precision (as far as sampling errors are concerned). The interpretation of these estimates in terms of the underlying demand-supply structures is not gone into by the author, so little can be said as to the plausibility of their size in most cases. Nevertheless, the measure of the effects of trade preference arrangements is clear: the coefficient shows how many times trade within the bloc is greater than among countries outside the bloc that are similar in size, distance apart, etc.; the multiple is found to be surprisingly large—a factor of 8 for the British Commonwealth and 25 for the French trading bloc.

Among the topics discussed in the section on "implications," two may be mentioned here: the expected changes in trade resulting from the formation of

the Common Market; and the effect of the average distance of a country from its foreign markets on its foreign trade ratio. Whilst the analysis in neither case is definitive, it marks a distinct advance in our understanding of these complex matters.

The major limitation of the basic model is the unsatisfactory treatment of nonlinearity. A constant elasticity formulation (linear in logarithms) is adopted, but it is unsatisfactory in view of the wide range of variation; many of the trade flows among the smaller countries are zero, and the author runs into trouble from the start since he cannot incorporate these observations (minus infinity, after taking logarithms) into the analysis. Without diagrams, and there are none in this book, it is not really possible to suggest improvements; the palliatives adopted by the author (assigning arbitrary small positive values to replace the zeros, and linearizing by segments) are not found satisfactory.

The exposition is clear, precise, and unpretentious. It is a serious piece of research, and its publication will no doubt act as a healthy stimulant to further work along the new lines marked out by the author.

S. J. PRAIS

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U.S. Production Abroad and the Balance of Payments: A Survey of Corporate Investment Experience. By JUDD POLK, IRENE W. MEISTER, AND LAWRENCE A. VEIT. New York: National Industrial Conference Board, Inc., 1966. Pp. 200. \$17.50; special prices for NICB associates, government agencies, students and teachers.

The investment behavior of U.S. manufacturing companies with operations abroad, its effect on the U.S. balance of payments, and its implications for policy are described in this NICB report. Such behavior of these companies, the authors believe, primarily concerns maintaining market positions, which typically have been established by exporting from the United States but which are inevitably eroded by foreign competition. It is much less influenced by relative rates of return. Furthermore, the costs of various types of financial capital only influence how foreign investments are financed, not whether they should be financed.

Moreover, the authors argue, since exports inevitably lose out to foreign competition, U.S. operations abroad that maintain a company's presence do not replace U.S. exports. Rather they promote U.S. exports of capital goods and intermediate products. They also create foreign interest in other U.S. products that, initially at least, are exported from the United States. Lastly they stimulate foreign economies and, hence, foreign demand for U.S. exports. All these exports would be lost, according to the authors, if investment were cut back.

But the most important result of curtailing U.S. investment abroad would be the sharp decline in earnings from abroad. Continuing earnings from manufacturing facilities "normally entail continuing investment" (p. 22). This necessity for continuing investment brings out the essential, "organic" nature of foreign operations, which reflects the need for a growing operation to maintain

earnings. The authors reject an "incremental" view of foreign investments, which relates new investments to additions to an earnings flow.

The description of corporate behavior, summarized above, was based on questionnaires and interviews with corporate executives. The results of the questionnaires were mainly useful in suggesting questions for the interview; for the authors decided very early that data from corporate accounts, which are designed to measure profits, could not be used to indicate effects of foreign operations on the U.S. balance of payments. Accordingly, in order to highlight these effects, they turned to a "qualitative" study, focusing on corporate attitudes, motives, and methods of operations. Because the qualitative approach was not supplemented with quantitative research, this study, although suggesting interesting hypotheses about corporate behavior, adds little to theoretical analysis or policy deliberations. A major conclusion, for example, that earnings will decline if the voluntary restraint program of the Commerce Department continues for a long time, is neither new nor specific enough to be helpful. How sharply and how soon earnings will decline, both critical questions, were not satisfactorily examined, although on the basis of the "organic" approach the authors presumably expected a sharp decline almost immediately.

This expectation would be realized, however, only if productive lifetimes of foreign investments are unrealistically short and unrealistically productive. Otherwise, investments could not make a profit, let alone recover their initial cost. (The question arises here whether domestic U.S. investments are as "organic" as foreign investments.) Moreover, even when using the "organic" approach to analyze earning potential, it does not necessarily follow that the current restraint program will lead to a decline in total foreign earnings. Investment on the scale of recent years can be financed by borrowings abroad and depreciation and depletion allowances, which are not covered by the program, and by capital outflows and retained earnings, which are only discouraged from rising by the program.

Other problems arise because the authors appear to have overlooked theoretical implications of what businessmen told them. For instance, the importance of maintaining market positions should have been examined critically. Specifically, the economic rationality of basing investment decisions primarily on a market-position criterion should have been explained, and it should have been made clear why prospective relative rates of return have only limited, instead of primary, bearing on investment decisions. Similarly, if "very few decisions to invest are reached by *comparing* different investment opportunities, including opportunities in the United States," the authors should not have stressed the welfare aspects of free capital movements (pp. 73, 22, 156; my emphasis).

There are additional problems. For example, by using multiplier analysis without considering the source of financing, the authors concluded that U.S. investment abroad will stimulate *both* foreign and U.S. economies at the same time. But, on the one hand, if financing is from foreign sources, the assumed purchase of equipment produced in the United States for installation abroad will be a leakage from foreign expenditure flows; whereas, on the other hand,

if financing is from U.S. sources, export of financial capital will be a leakage from U.S. expenditure flows, a leakage that is only partially offset by the equipment exports (pp. 116, 146-48). Finally, the authors referred inconsistently and inaccurately to the general intent and major details of the Commerce program (e.g., pp. 152-53).

In brief, this study of investment experience adds little to our knowledge of the balance-of-payments effects of American manufacturing investments abroad. This is due both to the failure to work through all the implications of the organic approach, including the market-position criterion, and to the absence of any quantitative verification of what businessmen said. The main benefits of the report are the numerous hypotheses for explaining business behavior that it suggests and the sample of executive thinking that it provides.

FRED B. RUCKDESCHEL

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International Trade and Finance: Theory, Policy, Practice. By FRANKLIN R. ROOT, ROLAND L. KRAMER, AND MAURICE Y. D'ARLIN. 2nd edition. Cincinnati: South-Western Publishing Co., 1966. Pp. x, 638. \$8.75.

A good textbook in international economics at the undergraduate level is hard to find these days. Perhaps more than any other course in economics at that level, with the exception of Introductory Economics, the selection of the materials for the course requires a proper blend of theory, policy, institution, and historical perspective. Students taking the course are apt to come from diverse fields of undergraduate majors, and the one centripetal force that binds them together is likely to be their common interest in current international economic issues. Ideally, therefore, a textbook in this field should make clear the logical structure of international trade theory, demonstrate its relevance to real-world problems by applying it to the analysis of major international economic issues, and provide the institutional and historical background essential for the understanding of these issues. From this viewpoint, the book being reviewed here is at once a source of felicitation and of disappointment.

Following two introductory chapters, in which some statistics on the structure of world trade and the role of the United States in that trade are laid out, the main body of the book is divided into three parts: on the theory, policy, and practice of international trade and finance, respectively.

In Part I, the theory of international trade and finance is presented in a decidedly low key. More advanced topics, such as the factor-price equalization theorem, the Leontief Paradox, the stability condition for the foreign-exchange market, the income and price effects of exchange devaluation, are all touched upon, but only lightly so as not to get involved in complex theoretical issues which at present seem to yield relatively meager tangible returns. The approach seems to be on the whole a sensible one, and its execution generally satisfactory.

Occasionally, however, the attempt to present a simple version of the theory verges dangerously on providing glib answers to what are in fact complex questions. In most of these cases, the basic problem lies perhaps not so much in the author's presentation as in the unsatisfactory state of the theory itself. On sub-

jects such as imperfect competition in international trade, and the relationship between technological progress and the structure of international trade, perhaps not much can be meaningfully said at this stage of the theory. In a few other cases, however, the answers given by the authors are oversimplified to the extent of being of dubious theoretical validity. For instance, the source of internal economies of scale is categorically attributed to the indivisibility of certain factors of production (p. 77); it is asserted that short-term capital movements are equilibrating when exchange rates are fixed, but cannot be expected to be so when exchange rates are freely fluctuating (p. 214). Such unqualified statements are either tautological, hence unhelpful, or invalid and hence misleading.

Part II on international trade and financial policies is perhaps the strongest of the three parts of the book. It starts with a brief discussion of the aims of international economic policy, then proceeds to examine various types of policy instruments for the restriction of international trade and payments. The truly commendable chapters are those dealing with topics of a historical and institutional nature. For instance, the authors' treatment of international cartels, citing case histories, makes fascinating reading of a subject students usually find dull and far removed from their concerns. The discussions of such important topics as the General Agreement on Tariffs and Trade, the International Monetary Fund, the European Economic Community, and the developments in U.S. commercial policy are the best that can be found in any of the undergraduate textbooks in international economics now on the market. A chapter on the U.S. balance of payments and international monetary reform draws material from the recent Bernstein Report on U.S. balance-of-payments statistics to elucidate the various approaches to the measurement of payments imbalance; in addition, it provides a comprehensive, up-to-date review of the policy measures adopted by the United States for correcting its persistent balance-of-payments deficits, as well as of the various proposals for the reform of the international monetary system. On all these topics, the authors have combined judicious selection of material with a lucid, lively style of writing. The results are truly gratifying.

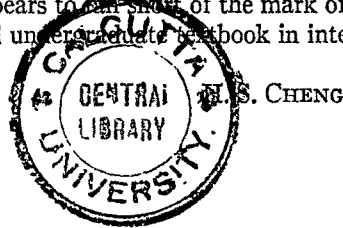
The usefulness of this part of the book, however, is marred by an unduly narrow view of what constitutes international economic policy. The authors explain that, "for reasons of space and analytical convenience," they decided to limit international economic policy to government actions that seek *directly* to affect the conduct of international trade and payments (p. 235), thus excluding from consideration fiscal and monetary policy measures that have for many decades figured importantly in governmental programs for adjusting external imbalances. This narrow view is not only unrealistic and unhelpful, but also precludes any application of the balance-of-payments theory presented in Part I to the balance-of-payments problems and policies discussed in Part II of the book.

Part III on the practice of international trade and finance, presumably, is mainly for students with international business orientation. How this part fits in with the rest of the book is difficult to perceive. For, whereas the first two parts deal with problems of national and international resource allocation and payments adjustments, the last part is concerned solely with the operation of international business from the standpoint of individual firms. The topics

covered in Part III include such matters as the sources of information for foreign market surveys, the organization of international business firms, the incentives and obstacles to foreign business operations, and the financing of international business. As a layman to international business, this reviewer finds the treatment of these subjects exceedingly dull, and can only hope it is not typical of the materials a student has to wade through in the nation's business schools.

In conclusion, despite its many shortcomings, this book is definitely a welcome addition to the small collection of undergraduate textbooks in international economics now available on the market. In the hands of a knowledgeable and skillful instructor, Parts I and II of the book could be quite suitable for a one-semester or one-quarter course in international economics. However, in view of the misgivings stated above, it appears to fall short of the mark of a first-rate textbook. The need for a truly good undergraduate textbook in international economics remains unfilled.

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Business Finance; Investment and Security Markets; Insurance

A Theory of Financial Analysis. By EUGENE M. LERNER AND WILLARD T. CARLETON. New York: Harcourt, Brace & World, 1966. Pp. xii, 281. \$7.95.

The purpose of this text, in the authors' words, is "to make explicit the inter-relationships that we believe exist between Security Analysis and Corporation Finance. . . . *A Theory of Financial Analysis* is designed to provide an analytical frame of reference for the formal study of finance . . . setting forth the basic material for advanced courses in both Corporation Finance and Investments." In considerable measure, these purposes are achieved.

The student using this book should have a basic familiarity with algebra and the calculus, including the use of lagrangian multipliers. The most frequently used mathematical techniques involve optimization by means of derivatives and partial derivatives. One section in the final chapter utilizes matrix algebra; however, it may readily be omitted if desired.

Lerner and Carleton begin with two chapters on the subject of financial analysis. The first is historical, and the second introduces such concepts as the rate of return on assets and the flow of funds through the firm. The next two chapters are concerned with the capitalization of an income stream, the calculation of the internal rate of discount (time-adjusted rate of return), and the treatment of taxes. Considerable emphasis is devoted in Chapter 4 to distinguishing between accounting depreciation and economic depreciation. The authors propose this distinction as the solution to the problem of multiple rates of return.

Chapters 5 to 10 comprise the heart of the book. Fundamentally, Lerner and Carleton develop a share-price valuation model in which the several vari-

ables are subject to two constraints. The first of these constraints they call the LC function (for reasons which should be apparent). This function relates the firm's level of investment to its rate of return on assets. It reflects the conditions prevailing in the firm's product and factor markets (i.e., its demand and cost schedules). The normal assumption is made that the firm's rate of return on assets will decline as its level of investment increases. The second constraint (called the FC function) is that imposed by conditions prevailing in the financial markets. Specifically, it defines the rate of interest at which lenders will advance funds to the firm. This rate is constrained by the debt-equity ratio of the borrower. The assumption is made that the interest rate on debt available to the firm rises with its debt-equity ratio.

In Chapter 10, Lerner and Carleton proceed to develop their complete share-price valuation model and to demonstrate how share prices may be maximized subject to the two constraints mentioned above. Included are a graphic solution to the problem as well as an illustration employing specific assumed values for the parameters involved. In this chapter, the authors argue that the value of the firm is not independent of its capital structure proportions and that the position to the contrary espoused by Franco Modigliani and M. H. Miller rests upon the assumption that equity will always sell at book value.

Chapter 11 seeks to relate elements of national income economics to financial analysis. It develops equations for aggregate expenditures, an aggregate financial function, and an aggregate valuation model. The final chapter is concerned with fitting the problem of portfolio selection into the system of financial analysis developed in earlier chapters.

The book is clearly written and felicitous of style. The development of the final valuation model is logical and easy to follow, both verbally and mathematically. Each chapter is followed by a brief verbal summary of its main points and by exercises (to which answers are provided in the back of the book).

The book has substantive as well as expository strength. It provides a basic framework by means of which various factors such as the demand and cost schedules facing the firm, its level of investment, retention rate, capital structure, the interest rate on borrowable funds, and the attitudes of its shareholders toward risk may be taken into account in making the financial decisions of the firm in such a way as to maximize the price of its shares. Thus, one of the book's contributions is to integrate these several variables into a single model in such a way as to provide a logical approach to financial decision-making. At the same time, certain limitations are apparent. While the work does a good job in specifying the important variables and constraints and relating them to each other and to the valuation model, the important task of determining the appropriate values to be assigned to these variables and constraints in specific cases is, perhaps of necessity, given fairly cursory treatment. The task of putting additional meat on the model's bones remains.

This reviewer would have welcomed a fuller and more explicit treatment of the dividend decision of the firm, perhaps taking recognition of the dividend-irrelevance position and presenting the author's arguments against it, in a manner similar to that in which the capital structure problem is treated. The rela-

tionship between the capital-structure and dividend decisions might have been developed. At occasional points the assumption is implicitly made that the firm is in a condition of capital rationing. This follows from Lerner and Carleton's assumption that the firm's rate of return on assets (a measure of its level of investment) is a function of its retention rate. Under the assumption of capital rationing, the dividend decision of the firm must, of course, affect shareholders' wealth since it affects the level of investment of the firm. This assumption might have been made explicit, and, further, the implications for the model of a noncapital-rationing situation might have been explored. Also desirable would have been a more thorough treatment of the impact of uncertainty on the financial decision-making of the firm. In this connection, the assumption that investors' attitudes toward risk are fully described by the variance (or its square root) of the return is more questionable than the authors suggest.

Withal, *A Theory of Financial Analysis* is a combination of a useful textbook and a monograph that, because of its original approach to a number of problems, will interest the professional student of capital theory. As such, its appearance is welcome.

JAMES T. S. PORTERFIELD

Stanford University

The Securities Markets: Operations and Issues. By SIDNEY ROBBINS. New York: The Free Press; London: Collier-Macmillan, 1966. Pp. xvi, 303. \$7.95.

The book opens with a brief overview of the money and the capital markets and their interaction. Subsequent chapters discuss the economic basis of the securities markets, the origin and work of the Securities and Exchange Commission, the administration of the Securities Acts as an exercise in self-regulation with government and industry cooperation, criteria of efficient securities markets, the major regulatory problems that have caused controversy, and the rise of the institutional investor in the stock market with its effects. A concluding chapter on market interrelationships deals with the several trading markets for stock—both the various organized exchanges and the over-the-counter markets.

Readers will be pleased with the enlivening use of illustrations, pertinent historical material, and the selected research data. The chief research is the *Special Study of Securities Markets*, for which Robbins served as chief economist, submitted to Congress by the Securities and Exchange Commission in 1963. The recommendations in this Study led to the Securities Acts amendments of 1964 and various SEC regulations. Other studies, such as those of the Wharton group, and a variety of journal articles are also cited. This material has been organized and enriched with historical and operational background to make a most readable volume for economists, professionals in finance, and advanced students interested in the regulatory aspects of the securities markets.

Robbins stresses the need for fostering "efficient markets," defined as a pricing mechanism that "permits the maintenance of a logical relationship be-

tween a company's financial position and the price of its shares." Some of the suggestions are likely to meet with stiff criticism in business circles: a waiting period and a filing for presently unregistered secondary stock distributions (p. 90); a broader definition of "insiders" that would include anyone in possession of special material information before its public release (p. 97); extension of Federal Reserve Board control of margin requirements to collateral loans made for the purpose other than the purchase of securities (p. 105); and possibly a limitation on price swings of more than a certain magnitude (p. 150).

Probably economists will find their deepest interest in the pervasive theme that "fairer" prices can be achieved by stiffer regulation. In spite of references to Stigler, Friend, *et al.* (p. 31) and to Robinson and Bartell (p. 161), one senses a failure to distinguish between the price continuity generally accepted as desirable for the short term and the need for price to change, sometimes substantially, to reflect large changes in outlook. There also seems insufficient stress upon the difficulty, if not impossibility, of determining fair price with certainty when value depends upon future events. The past record can supply the only data that can be fed into either a computer or the mind of the expert security analyst.

Another basic question raised is how far the stock market influences the course of the economy and how far it merely mirrors expectations. The point recurs in the suggestion that institutional buying and selling is influential for the prices of particular stocks. The correlation could reflect the correct anticipation of earnings developments rather than the independent influence of institutional activity.

The fair price question rears its head again in the discussion of proper commission rates for brokerage in exchange transactions. One wonders what kind of cost data would be conclusive for fixing a fair rate schedule in a competitive personal service business such as stock brokerage. Admittedly, the business is highly cyclical and subject to over- and under-capacity even in the short run, has profits which are a mixture of capital and labor return, and provides service to small investors on what may represent a loss leader basis. How much good economics is there in the idea of basing the prices of such service upon "long-term volume expectations" (p. 187) or suggesting a flexible (compensatory) commission system that provides some assurance of a more uniform rate of return" (p. 140)?

The concluding chapter tells of the rise of certain competitive pressures for the New York Stock Exchange. The "third market" is made by a few non-member houses who offer to buy and sell certain active listed stocks as dealers, performing a function akin to that of the exchange specialist when the latter deals for his own account. The regional exchanges, notably the Midwest and Pacific exchanges, have made innovations that result in concessions from standard commissions. Such growing competition may even serve the New York Stock Exchange by reducing the threat of antitrust action.

This sampling of the book is inadequate because it centers attention on the more debatable material. It fails to give an idea of its wide scope and scholarly fairness in presenting pros and cons. Robbins does, however, take an activ-

ist's approach to regulation. If used with students, they should be acquainted with the literature of those skeptical of what securities regulation can achieve when it goes much beyond the basic areas of full disclosure and the prevention of fraud and manipulation. It is in these areas that the SEC, in cooperation with the industry, has done such an excellent job.

HARRY G. GUTHMANN

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The Economics of Corporate Finance. By SEYMOUR FRIEDLAND. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xii, 418. \$11.95

The past five years have seen the emergence of a plethora of new books, as well as revised editions of older books, on the subject of basic business finance. The trend common to most of these books has been a shift away from the descriptive-institutional approach of the 1950s to one which incorporates analytic techniques in various degrees. Professor Friedland's objection to most of these books is the "omission of a framework for analysis on which can be hung the various analytic approaches *and* the more traditional institutional materials." His objective in this book is to develop such a framework. The framework which he selects as the central theme for the portion of the book dealing with corporate finance is "treating the firm as if it were trying to choose an optimal balance sheet." Specifically, this framework involves the selection of assets and capital sources through the application of the well-known net present value (NPV) criterion to the problem of choice among alternatives.

Part I of the book (144 pages), develops the investment decision rules for the major categories of assets—fixed assets (Ch. 2), inventory (Ch. 3), accounts receivable (Ch. 4), cash and marketable securities (Ch. 5), and the asset portfolio (Ch. 6). The approach in this part of the book can be described as an application of basic operations research techniques and some microeconomic theory to the problem of asset investment. Even though the author does not resort to direct use of calculus in the book, he does a commendable job of conveying to the reader the concepts embodied in differential calculus and probability theory and their implications for finance. Most of the subject matter is presented in a lucid and readable manner. Unfortunately, there are several areas where the quality of the results does not match the intent.

The rationale for the asserted universal applicability of the NPV criterion is developed in Chapter 2. Quite correctly the author identifies the cost of capital as the appropriate discount rate in the net present value formula and defines it as "the minimum acceptable rate of return at which the interests of the owners will not be damaged" (p. 27). I suspect, however, that despite statements such as, "Fortunately, there is a criterion of fixed asset investment which . . . provides an unambiguous and accurate means of ranking investments . . ." (p. 27), the uninitiated reader is likely to have difficulty understanding just why this criterion is so superior. The necessary tie-in between NPV and shareholder wealth is never clearly established. For example, in a subsequent chapter, the author states: "We have assumed throughout this book that the firm is trying to achieve some optimal mixture of the expected

value of net worth, which is more nearly market value than book value or any other observable valuation *and* [italics added] risk" (p. 177). Thus, the author fails to point out (or realize) that the correct use of the NPV criterion (as he himself defines it) already takes account of the element of risk. That is, the risk embodied in the returns of the firm as a whole, or in any specific project, affects the firm's cost of capital (this point is brought out very nicely by the author himself in Part II); the "value of net worth" is then merely a function of this risk-adjusted cost of capital.

In Chapter 6 the author applies an expectation/variance model to the asset portfolio decision of the firm. While his ideas are sound, his explanation is not. Again, he falls into a trap similar to the one described above. He fails to treat correctly the relationship between net expected value, expected returns, risk, and the cost of capital. Also, there are several minor but basic errors in the treatment of scrap value (pp. 34-36) and the credit selection process under uncertainty (p. 88).

Part II, which covers 98 pages, deals with the firm's capital structure decision. Following a general description of the characteristics of financial instruments and certain environmental factors in Chapters 7 and 8, the author devotes Chapter 9 to the problem of selecting an optimal capital structure. He does an excellent job of illustrating the effects of debt on the riskiness of the returns to common shareholders and discussing the dynamic nature of the capital structure decision. His conclusion, however, still evidences the confusion between expected value of net worth, expected returns, and risk . . . " . . . the firm will not necessarily attempt to achieve the highest level of expected value of net worth, but will choose a capital structure providing the most desirable mixture of return and risk available to it" (p. 194).

Chapter 10 is concerned with the determinants of the cost of capital. While certain parts of this chapter are excellent—for instance, the author's handling of the "changing capitalization rate" case (pp. 202-7)—the clarity of exposition which characterized the preceding chapters is less in evidence here. For example, the discussion of the "cost of retained earnings" (pp. 199-200) is especially ineffective. Also, throughout Part II, the author assumes what is essentially a constant expected earnings model, and fails to discuss the complications which would be introduced under other assumptions as to earnings streams. The author's conclusions on the effects of market price movements on the cost of capital are unwarranted—"Permanent changes (in market price) will increase the cost of capital when the change is downward, and lower the cost of capital when the change is upward" (p. 214). That is, he fails to consider that, even in the simple earnings capitalization model which he uses, market price is a function of *two* variables: expected earnings and the cost of capital. To attribute the cause of the change in price to only one of these variables—the cost of capital—is inappropriate. Part II concludes with two excellent appendices. The first appendix deals with the problem of leasing; the second one with mergers and business failures.

Part III is a departure from materials traditionally encountered in corporate finance texts. The six chapters in this part, covering 165 pages, provide a description in depth of the flow-of-funds social accounting system (Chs. 11

and 12), the financial sector and commercial banking (Chs. 13 and 14), non-bank financial institutions (Ch. 15), and the corporate nonfinancial sector (Ch. 16). The author justifies the inclusion of this material on the grounds that "more and more students come into the first corporate finance course with virtually no knowledge of either the money or capital markets . . ." (p. ix). This reviewer found the presentation in this part considerably less imaginative and more tedious than the material preceding it.

In summary, despite the fact that the author is often definitive where at best he should be suggestive, the book is well worth reading. Its primary contributions lie in presenting a unifying framework and in bringing together basic concepts of economics, operations research, and finance. While Part I has been subjected to considerable criticism, it is there that the author makes his most significant contributions. As a basic textbook, the book has some inadequacies: (1) there is virtually no discussion of short-term finance; (2) no problems are available in the book; (3) dividend policy is accorded a total of two pages; and (4) the discussion of the determinants of market value is perfunctory. This book's primary value as a text is likely to be in introductory finance courses within departments of economics and possibly in some intermediate level courses in business schools.

ALEXANDER A. ROBICHEK

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Business Finance: Theory and Management. By STEPHEN H. ARCHER AND CHARLES A. D'AMBROSIO. New York: Macmillan, 1966. Pp. xvi, 556. \$8.50.

Writing a textbook in the area of business finance is not an easy task. The area is in a constant state of flux and in a perennial condition of controversy. The scope of the area ranges from the immediate appearance of the hustle and bustle of the corporate office to the longer range theoretical perspective of the ivory tower, from the inside view of one financial institution to the general outlook for a whole economy. Writing a text in this more than usually extensive and unsettled area requires a multitude of difficult decisions about the inclusion or noninclusion of topics and about the degree of finality with which to present uncertain truths. The authors have chosen not to follow blindly in the footsteps of their predecessors. They have attempted in some of the sub-areas to break their own paths, tread on new ground, and incur risks associated with new ventures.

The contents of the book, its organization, and its emphasis indicate a conscious decision on the part of Archer and D'Ambrosio to give the book a heavy dose of macroeconomic and capital-markets flavor. Of the 22 chapters in the book, Chapters 2, 3, portions of Chapter 5, Chapters 6, 7, 8 and segments of other chapters treat the macroeconomic overall view. For business (industrial) administration programs without a good required course in macroeconomics (or variations thereof: "Capital Markets," or "General Monetary Conditions," etc.) the Archer-D'Ambrosio emphasis on the general view is an extremely useful addition to a business finance course.

The emphasis of Chapter 5 is new for a business finance book. Written in

collaboration with W. F. Sharpe, it explores with the help of indifference curves the process of choice between investments of differing risks and of differing expected rates of return. It sets an economics-theoretical tone for subsequent discussions of risk. There is little doubt that potential users will find this chapter an especially beneficial and helpful feature of the book.

The book highlights and emphasizes risk. Interest rates (rates of return) are not only differentiated by their levels but also by the risks accompanying these levels. The student is constantly made cognizant of the need to take into account, simultaneously, both the yield and risk dimensions of investments. The emphasis on risk is a useful one. It repeatedly communicates the inadvisability of abstraction from the risk dimension and constantly conveys the basic notion that risk is a part and parcel of the "commodity" called investment.

Some omissions in the book are regrettable. Notably absent are a discussion of risk preference, an acknowledgement of limitations of profit definitions including the one favored by the authors, and the evaluation of difficulties inherent in the empirical estimation of rates of return. (1) The attribution of risk aversion to all segments of the capital market and to all managerial capital budgeters, no more than a convenient assumption at the outset, remains to influence the entire book. It is never seriously questioned or critically evaluated. Risk preference, not at all unlikely to be the dominant characteristic of some segments of the market, remains an important unexplored possibility. (2) The discussion of findings relating to random walks of stock prices could have been included advantageously either at the cost of an addition to the book or at the expense of another topic. One of the major results of the investigation of the random walk of a stock price is the lack of correlation between consecutive changes in stock prices (ownership values). This result suggests that a profit definition which relies heavily on changes in the value of ownership, as does the profit definition the authors favor, may not be useful for the formation of expectations about future profits. (3) The effect of lack of information and of errors of measurement on the actual empirical evaluation of rates of return on equity and assets as well as on estimates of future yields on investment projects have not been sufficiently treated.

An emphasis on the total macroeconomic picture and a heavy concentration on the implications of risk for investment behavior constitute the bulk of the first part of the book, Chapters 1-8, and a framework within which the rest of the book is written and developed. The second part of the book, Chapters 9-13, is what might be most aptly described as an economic theoretical descriptive discussion of profits and the factors affecting the demand for and supply of capital (loanable funds) as viewed and experienced by an individual firm and by its suppliers of capital. The third part, Chapters 14-22, treats topics which are customarily included in a text on business finance: management of the investment and capital budgeting processes, financing and refinancing decisions, and ratio analysis. Problems at the end of chapters, designed to help the student acquire a concrete understanding of the conceptual material in the text, are a useful pedagogical addition.

Despite the omissions mentioned earlier the text is a satisfactory teaching

instrument. It will be especially welcomed by instructors who choose to use their own supplementary materials for some of the details of financial management and for topics of special interest and who prefer an emphasis on general market forces.

HASKEL BENISHAY

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Business Organization; Managerial Economics; Marketing; Accounting

Accounting, Evaluation and Economic Behavior. By RAYMOND J. CHAMBERS. Englewood Cliffs: Prentice-Hall, 1966. Pp. xii, 388. \$7.50.

There are few basic works in the literature of accounting that undertake a thoroughly theoretical approach. Indeed, most writing by accountants or about accounting is concerned with applications of existing rules of practice to specific situations, the tracing of precedents for or against some procedure, or with teaching the methods and techniques currently used by professional accountants. Scattered throughout the articles and textbooks, one finds protests by a few academicians and practitioners against the inconsistent body of rules that are known as "generally accepted principles of accounting." These protests have sometimes contrasted accounting with economics in order to develop a structured system of accounting definitions and rules (e.g., J. G. Canning, *The Economics of Accounting*, 1929); have been concerned about the effects of price level changes and called for and described a system of constant dollar accounting (e.g., H. W. Sweeney, *Stabilized Accounting*, 1936); have presented a plea for meaningful accounting reports (e.g., K. MacNeal, *Truth in Accounting*, 1939); have presented a system of accounting based on "practical" considerations, such as cost-based measurements that can be readily audited (e.g., W. A. Paton and A. C. Littleton, *An Introduction to Corporate Accounting Standards*, 1940); and have sought to develop and describe a system in which trading income and capital (or holding) gains are reported separately (e.g., E. O. Edwards and P. W. Bell, *The Theory and Measurement of Business Income*, 1961). Professor Chambers' book is one of the most recent of this genre.

On the jacket Chambers' book is described as "A thorough and literate foundation for a system of accounting, developing the many facets of the function of accounting as an aid to the efficient management and development of the economic institutions of an industrial society." This foundation is built of propositions that are ". . . admitted only because their premises are believed to be acceptable or demonstrable and because they comprise a consistent system" (p. 10). The propositions are drawn from the theory of signs and language, measurements, logic, social structures, communication, etc. The belief in their acceptability is based on copious references to authorities in these fields. (The economist quoted most often is von Mises.)

The system of argument is predominantly deductive. Statements about

human behavior, perception, needs, institutions, etc. are made and supported by many references to the assertions and conclusions of authoritative writers in many fields. Each chapter is followed by an outline, in flow-chart form, and a list of numbered definitions and assumptions, from which inferences and conclusions are drawn and also numbered. After each conclusion, one or more numbers are given that refer back to a previously drawn conclusion or assumption on which it is based or related in some way. The net result of this form of argument is a carefully built up, apparently logically consistent system, upon which a theory of accounting is to rest. (I use the word "apparently" because, despite the heroic efforts of the author to provide schematics and cross-referenced statements, I find it extremely difficult to keep track of the basis for the multitude of assertive statements made. Perhaps a reason for my problem is that Chambers uses philosophical terminology that prevented my fully understanding the points being made.)

The first three chapters (about a fifth of the book) construct, from basic assumptions, the milieu in which an accounting system functions. Chambers begins with statements and definitions about the behavior of people: their capacity for perception, motivation for action, reaction to stimuli, and capacity for and method of reasoning, learning and belief. His following discussion on the achievement of desired ends is based on these statements. "Actors" are said to choose among alternative means to the desired ends by discovery and evaluation of the consequences and marginal utilities of their actions. The "environment of action"—natural and social, legal, and economic—is considered next. The conclusion drawn from these introductory chapters is that an entity must be able to choose among alternative causes of action and adapt to changing conditions in order to survive.

Chapter 4, "Monetary Calculation," contains Chamber's conclusions about the function and method of accounting. He argues that the ability of an entity to adapt by engaging in market exchanges is measured by ". . . the simple financial property which is uniformly relevant at a point of time for all possible future actions in markets . . . the market selling price or realizable price of any or all goods held" (p. 92). "We therefore define accounting functionally, as a method of retrospective and contemporary monetary calculation the purpose of which is to provide a continuous source of financial information as a guide to future action in markets" (p. 99).

The remaining two-thirds of the book presents useful elaborations of the conclusion—an illustration of the effects of not making price level adjustments, a discussion of the application of current market prices to different types of assets, a consideration of accounting for trading ventures, corporations, and nonprofit organizations, as well as additional statements about behavioral problems and some basic and often irrelevant observations about such subjects as double entry bookkeeping and epistemology.

Chamber's key conclusion, that assets should be measured at contemporary market prices and income measured as the change in these asset amounts (adjusted for changes in general price levels and investments in the entity), differs from the present value approach favored by most economists. He rejects present value because the calculations are not based on current market

"facts," but rather on expectations about the future. Considering that Chambers believes that financial statements should show the short-term ability of the entity to adjust by selling or buying individual assets, his rejection of present values may be justified. However, his valuation rule requires that the assets of an entity always be stated at the lower of exchange or use values. It is difficult, for me at least, to conclude (even granting Chambers' premises) that this is or might be the sole function of accounting data. To give just one example, consider that the cost of a specialized machine would be shown as an expense in the period of its purchase, if it has no resale value (net of transaction costs), even though it has high use value. If managers and/or owners view the accounting statements seriously, what might the effect of this procedure be on their decisions to acquire assets?

A different type of problem that one encounters with this book, perhaps because of the theoretical approach that Chambers takes, is that he does not consider adequately the practical problems of measuring asset values in terms of current market prices. Chambers asserts that "... whether or not [a person] chooses to discover his financial position, that position is discoverable" (p. 81), in terms of the current cash equivalent of his assets. But the amounts that may be received for assets differ when they are sold singly or in groups, even when unambiguous market prices exist for specific assets. (This fact violates the principle of additivity about which Chambers is very concerned). Nor is the market price of manufactured goods known until they are sold. (Interestingly, in the brief paragraph or two in which he considers the problem of manufactured goods, Chambers seems to suggest that traditional cost allocations be made, with the exception that original costs be restated at current market prices.) Indeed, the essential problem for accountants, in my opinion, is that of measurement. I believe that it behooves the proponent of a theory of accounting to spell out, in detail, how his concepts can be made operational.

It would seem, given the as yet unsolved problems of valuing assets, either in terms of current market price, present value or in some other way, that Chambers would favor accounting statements in which assets are valued on several bases. But he rejects this notion emphatically, on the grounds that only one method can be correct.

At the conclusion of his book, Chambers likens his work to that of Copernicus; he envisions a similar Copernican revolution in accounting thought. As he notes, others have complained about the inconsistencies and meaninglessness of much accounting practice. Considering the pragmatic orientation of accountants, it is doubtful if Chambers' formal axiomatic presentation, with its limited consideration of methods of measurement and recording, will be as effective as he hopes. Nevertheless, Chambers has provided us with a rigorously constructed system that attempts to integrate accounting theory with other disciplines. In so doing, he has prepared a strong foundation upon which additional research into the measurement and reporting techniques of accounting can proceed.

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Industrial Organization; Government and Business; Industry Studies

International Differences in Industrial Structure. By JOE S. BAIN. New Haven and London: Yale University Press, 1966. Pp. xv, 209. \$5.00; paper, \$1.45.

Professor Bain's contributions to the study of industrial structure and performance are well known and highly regarded. His latest monograph, which has the great merit of brevity, compares certain aspects of the structure of a number of manufacturing industries in eight countries: the United States, Britain, Canada, France, Italy, Sweden, India, and Japan. The quantitative comparisons deal with plant size, plant concentration, firm concentration, and the relation between the two. Conclusions regarding efficiency are drawn from comparisons based on plant size. Comparisons of firm concentration are supplemented by brief discussions of "supercontrol groups," cartelization, government regulation of business, government ownership and operation in manufacturing, and import competition. Some explanations for the statistical findings are offered.

The general picture that emerges is that plants in the other countries studied are smaller than in the United States, production is less efficient, concentration both by plants and by firms is, more often than not, higher, and monopolistic practices are more prevalent. However, the handicraft sector, which is important in some of the countries examined, is omitted from the comparisons. There are, of course, numerous exceptions and qualifications to these generalizations, and the author does not state his conclusions so bluntly or so briefly.

The study suffers from weaknesses in workmanship. Available primary and secondary sources are not fully or properly used. Some examples of this deficiency may be given from the Canadian literature, with which the reviewer is acquainted. In 20 out of 34 industries Canadian plant size distributions are listed as "not available" although they are published, for most of the industries concerned, in bulletins listed in the government's catalogue of statistical publications. The comparison of plant and company concentration is confined to 5 industries, although the source used for data on company concentration devotes a chapter to this subject and contains comparisons for 96 industries, which, in most cases, could have been put into the form favored by Bain, with the help of supplementary information from published sources. On the non-quantitative side of the analysis, both the text and the bibliography suggest that no books or articles on Canada published after 1959 were consulted, nor the Annual Reports and other reports issued under the Combines Investigation Act. Even a cursory glance at this material should have prevented the astonishing statements found at pages 105 and 154, to the effect that enforcement of Canada's anti-combine legislation is "vigorous" (this is quoted from a misguided secondary source) and that it is a factor in the low ratio of company to plant concentration (no one has ever been convicted on a merger charge in Canada).

The publisher claims on the jacket flap that "a beginning is made here in

the development of a literature in which the industrial structure of different countries is compared on the basis of facts and statistics rather than impressions and folklore." The book is written as if the author believed this claim to be valid; that is to say, the contributions of earlier studies to the subjects under discussion are ignored. They are significant, though not, perhaps, numerous.

All fruitful empirical research in economics involves questionable practices, such as the drawing of conclusions from small or biased samples, the use of "heroic" assumptions, and *ad hoc* theorizing. The problem is to know when an investigator has gone "too far" and this is, of course, a matter of judgment. In my judgment, Bain has gone "too far" in significant respects. For example, conclusions regarding the general level of concentration in a country are based on non-random samples of 19 industries at the three- or four-digit level for France, 16 for India, 7 for Sweden. Even smaller samples are involved in the study of the relation between plant and company concentration. The comparison of efficiency is based on the assumption that in each U.S. industry 70 per cent of employees are employed in plants of efficient size or larger, combined with the assumption that the minimum size required for efficiency is the same, in absolute terms, in every other country as in the United States. The former assumption is derived from the finding in Bain's earlier study that in each of 20 industries examined between 70 and 90 per cent of output came from reasonably efficient plants. The second assumption is backed up by a brief and very inadequate discussion of the possibility that minimum efficient size might differ in different countries.

The monograph gives the impression of having been prepared in haste, with too much of the work, including the examination of source materials, left to research assistants. Nevertheless, it is useful as a compilation of data from a great variety of international sources, and should stimulate further research.

GIDEON ROSENBLUTH

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R & D: Essays on the Economics of Research and Development. By DANIEL HAMBERG. New York: Random House, 1966. Pp. xiii, 170. \$4.95.

The accelerating growth in research and development (R and D) activity since the end of World War II has, as one might well have anticipated it would, stimulated an accelerating growth in the body of literature relating to the R and D process. Professor Hamberg has been a fairly regular contributor to that segment of the literature which focuses on the economic aspects of the R and D process, and in particular on the interrelationships between market structure and R and D activity. The present volume is a collection of essays which brings together and updates much of Hamberg's previous work in the area, as well as presents some new work relating to the allocation of the R and D budget.

The essays, seven in all, are logically, and in fact, grouped into three distinct parts. The first two essays describe and discuss "the research explosion" and its implications; the next three essays explore the theoretical and empirical arguments that might encourage one to link together R and D activity and

market structure; and, the final two essays present an empirical analysis of the determinants of R and D, and the suggestion of linear programming as a basis for the allocation of the R and D budget.

Much of the material in the book is well known (thanks in part to Hamberg himself) and the virtue in its repetition lies in bringing these materials together in a single volume. Thus, we are once again reminded of the comparatively minor and declining share of R and D effort that is devoted to basic research, the relative preponderance of development activity, the growth in the role of the government as a source of R and D funds, and the growing importance of industrial R and D activity. Hamberg argues that the declining emphasis on basic research, the deterioration in the quality of research, is a potential source of danger to the economy since future applied R and D feeds on present basic research. Yet, the uncertainty of payoff and time lag in payoff will tend to discourage all but a few of the larger diversified firms that can accept the risks, afford to wait for the payoffs, and are capable and interested in exploiting many and varied results in many and varied ways, from undertaking basic research programs. Thus, Hamberg argues, the government may have to play a larger role in the R and D process if basic research is to be supported on a larger scale. This could be done both through the sorts of activities financed by the government as well as through adjustments in the patent system. In particular, there might be a system of awards that would eliminate incentives for minor innovations, or the issuance of dual patents. Finally, government-owned patents might be sold on a nonexclusive basis rather than granted on inventions arising out of government-sponsored research.

Hamberg next discusses the pros (financial strength, ability to diversify efforts, weather hardship, exploit varied opportunities, exploit protected positions) and cons (bureaucratic complexity, large fixed investment in existing operations, inability to enter entrenched markets) of size and market power as influencing research activity. His empirical results lead him to conclude that while size does seem to be a determinant of R and D activity, this varies within individual industries. Through simple correlation analysis Hamberg finds that there is a tendency, albeit an extremely weak one, for R and D activity to be related to industrial concentration, with size playing a slightly larger role. There are, however, wide differences among industries. Hamberg also presents some evidence to support the hypothesis that the large industrial laboratories are likely to be the minor sources of major inventions. He argues that this will be the case because of company quick-profit oriented, low-risk programs, customer- and management-stimulated research ideas, and vested interests. He also argues, not very convincingly, that organized research is handicapped because creative individuals may be unwilling to subjugate their views in favor of a group norm that tends to be most productive of mediocrity through compromise. The potential of interdisciplinary economies and benefits is not considered.

The last part of the book is far and away the weakest. The theoretical arguments and empirical work in attempting to isolate the determinants of R and D (sales, profits, liquidity, government contracts, investment, previous expenditures) tend to be inconclusive and rather unexciting. The use of linear pro-

gramming to allocate and evaluate R and D projects was obvious to others over a decade ago and is no more satisfactory now than it was then, since R and D budget problems still persist in being dynamic problems, under risk, in a nonlinear world.

It seems safe to say that these essays will not set the world on fire nor further disturb the frontiers of knowledge. The book, or at least the first five essays, will be useful to the student of the R and D process in that Hamberg's past work is brought together and coordinated, and some attention (although not enough) is paid to the very important contributions of others, such as Schmookler and Mansfield. The book will not, however, settle any significant debates.

IRA HOROWITZ

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The Economic Effects of Regulation: The Trunk-line Railroad Cartels and the Interstate Commerce Commission Before 1900. By PAUL W. MACAVOY. Cambridge: Massachusetts Institute of Technology Press, 1965. Pp. ix, 275. \$10.00.

Paul MacAvoy has written another excellent book of that (happily) growing genre which unashamedly uses economic theory to cast light on the real world. It is a book which is an outstanding contribution both to the "New Economic History" and to industrial organization.

MacAvoy sets himself the task of delineating the factors that lead to the establishment of cartels, and that contribute to their stability and their dissolution. A generation earlier, the decision to write such a book would have surely meant that the author had suddenly acquired access to the previously secret minutes of the cartel meetings. Herfindahl has previously demonstrated that considerable insight into cartel pricing can be garnered from economic inference and outside data. Now, MacAvoy has burnished the tools of economic theory, and produced insight not only into prices, but also the timing of price wars, the identification of the nonconformer and even a convincing indication of the profits and losses accruing to cheater and noncheaters alike.

While everyone *knows* that price differentials between the same grade of a commodity at two locations on the same side of a surplus supply area should represent transport costs between the two points, no one before MacAvoy has used that fact to establish effectively a time series of true transport rates. While everyone *knows* that the market value of a company's shares should reflect changes in its profitability, most students of the "real world" are too preoccupied with establishing the "irrationality" of the stock market to use such price data to date real changes in company affairs: fortunately, that is not true of MacAvoy. The result is that fortunate readers will learn much more about the Eastern truck-line railroad cartels than they knew before and in some cases more than many railroad executives knew at the time. MacAvoy's main conclusion is that the cartel arrangements were clearly unstable prior to the establishment of the ICC. When the ICC began to take action to prevent "unreasonable" rates, the main impact of its regulation proved to be the effective enforcement of cartel arrangements, and when the Supreme Court re-

stricted the rate-setting powers of the Commission, price competition returned again.

A book can be excellent without being what every reader would like it to be. It is possible to find some faults in this book as well. MacAvoy has painstakingly attempted to give the reader all of the statistical and econometric evidence that lay behind his own conclusions, to a degree which is both excessive and bewildering. The reader who wants to challenge the conclusions is well-supplied with ammunition; the reader who wants merely to discover them is less fortunate. At a more fundamental level, MacAvoy has not done all the things this insatiable reader would have liked. For example, the hypothesis that cartel success or failure was determined (or affected) by changes in transport costs or in capacity utilization is treated only in two long footnotes on pages 65 and 195, and in the latter case only in support of MacAvoy's conclusion that the ICC was the predominant cause of the cartel stability in later years. In fact, except for his demonstration that cheating was frequently profitable he gives us surprisingly little insight into why cheating occurred at the particular time it did and by whom. For example, he shows that the N.Y. Central suffered least from breakdowns in the cartel, but it was not an outstanding cheater, while the Erie and the B&O which were particularly hard hit by price wars, cheated as much as anyone. The stocks of the latter roads were in a general decline during the period while the Central stockholders prospered. It is almost as if the prosperous railroads were willing to cooperate as long as stockholders were happy while other roads used the temporary profits from cheating to stave off stockholder discontent or increase liquidity in an imperfect capital market.

No one can satisfy every reader, and this reviewer's gratitude clearly outweighs his disappointments. MacAvoy has done an outstanding job of demonstrating the light that can be cast on practical problems by the sensible application of economic principles. We can only hope that he (and others) will shine that light on other problems soon.

PAUL H. COOTNER

Massachusetts Institute of Technology

Defense Purchases and Regional Growth. By ROGER E. BOLTON. Washington: The Brookings Institution, 1966. Pp. xiv, 189. \$2.50.

As the title implies, this study is concerned with the impact of defense expenditures on regional growth. The units under consideration are the states during the period 1951 through 1962. The conclusion, as expected, is that defense expenditures do influence growth and that the impact on total income is more significant than the impact on per capita income.

The author's framework of analysis is an economic base (foreign trade multiplier) model in which exports are the major exogenous activity. The main sources of data are annual Department of Defense contract award data, income estimates by states, and wage and salary information by state and industry groups. Given these data, the share of federal defense spending as a component of export activity levels is taken as a measure of importance. In

turn, changes in the level of total income and per-capita income are examined in terms of changes in defense activity.

The immediate empirical problem is how to estimate the exogenous income of a state and the defense spending in a state. Three estimates have been made for exogenous income. In all three cases property income and transfer payments are included. Manufacturing and agricultural income are assumed as exogenous, *a priori*, in one case. In the other two cases exogenous income is determined on the basis of location quotients. Defense spending consists of three groupings: military payrolls, civilian defense department payrolls, and procurement purchases. The first two are estimated directly for each state, although not without some digging. Procurement spending, which constitutes about two-thirds of all defense spending, is more difficult to estimate by state. Only prime contract award data are available by state and these do not show industry groups. The nasty problems are to account for: (1) the fraction of prime contracts which accrue to the state as income; (2) the income generated by subcontract work in a state, regardless of the location of the prime contractors; and (3) the estimation of non-subcontract interindustry purchases from other manufacturers.

The author's method of estimating procurement spending on a state-by-state basis is only partly satisfactory. On a "share of the nation" basis each state's share of total procurement in any year consists of three components: (1) the share of contracts awarded, lagged to account for delays between contract awards and work done; (2) the share of wages in "defense related" industries; and (3) the share of wages in all "other manufacturing" industries. Each component is weighted by the estimated value added within the state with the weight sum equal to one. The author estimates a value of .5 to a state's share of contract awards, i.e., half of a typical contract represents value added by the prime contractor. For "defense related" industries, three alternative values are calculated .1, .2, and .3. The last term, the share in other manufacturing, takes up the residual .4, .3, or .2, depending on the values used for the "defense related" industries.

The industries in the "defense related" category include: Aircraft and Parts, Ordnance, Ships and Boats, the electronic industries, and other such industries. Yet, these are the same industries that receive the vast majority of prime contracts. This raises two problems. First, prime contract awards are somewhat double counted. Second, since these are essentially the same industries that hold prime contracts, the coefficient for value added also should be .5. Use of this value, however, would leave nothing for the third component, "other manufacturing." Thus, while it seems inconsistent to assign "defense related" industries values of .1, .2, or .3, it does avoid this problem. It means, however, that analytically any distinction between the prime contract component and the defense related component is blurred.

The author's overall findings seem quite reasonable. During the period 1952 to 1962 the Pacific, Mountain, South Atlantic, and New England states have had the highest share of their aggregate income growth dependent upon defense expenditures. The two areas where the impact exerted a negative

influence were the Middle Atlantic and East North Central states. In large part this shift reflects the changing pattern of military requirements towards aircraft and missiles. While this geographic pattern reflects the total impact, prime contracts tend to be even more concentrated in the first four state groups mentioned, while the Middle Atlantic and East North Central gain in the sub-contracts or other input requirements. These same geographic patterns also show up in the special Census of Manufacturers report, *Report on Shipments to or Receipts for Work done for Federal Government Agencies and Suppliers* (1963, MA-175), published after Bolton's work was completed.

Given the growing interest in regional development policies at the federal and state and local level, Bolton's analysis of one of the largest sources of autonomous regional income, military expenditures, is extremely welcome. Further, the author is to be congratulated for ferreting out hard to come by data. Both students of regional economics and defence economics will find this a useful work.

CHARLES M. TIEBOUT

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Economic Report on Mergers & Vertical Integration in the Cement Industry.

Staff Report to the Federal Trade Commission. Washington: Supt. Docs. 1966. Pp. xiii, 123. 45¢.

Up-to-date information on an important industry is always valuable. The Bureau of Economics of the Federal Trade Commission has put together a competent survey of the structure and performance of the cement industry—a survey prompted by concern over the extensive mergers in the industry and possible action against such mergers under Section 7 of the Clayton Act.

There were 48 producers of cement in the United States in 1966, the number having fallen from 72 in 1946, despite the entry of 13 new companies. Mergers account for the reduction in numbers. The relevant market, however, is not national but local or regional. The structure of these markets is oligopolistic; out of 51 identified "regions," only 3 have a largest-four concentration ratio of less than 50 per cent (these account for 11 per cent of total U.S. output), while 34 regions have concentration ratios for the largest four producers of over 75 per cent. Yet concentration in the markets does not seem to have changed very much in recent years, notwithstanding numerous acquisitions. The reason is that mergers have for the most part been "extension" mergers—acquisitions by firms in one market area of plants or firms in another market area. There have been very few "horizontal" mergers within market areas. Producers in one area have also frequently entered other areas by building new plants there.

The report views vertical mergers with alarm. Though several producers of ready-mixed concrete have entered the production of cement in recent years, most vertical integration has come about through forward merger—acquisition of ready-mixed concrete companies by cement producers. In 1966, 19 cement companies had acquired ready-mixed companies, almost all subsequent to 1959. The "tied" concrete producers consumed 7 per cent of all cement pro-

duction, of which 4 per cent came from their affiliated cement companies and 3 per cent from nontied suppliers. These figures may seem trivial, but the report views them as developing trends; in some local markets the percentage was much higher, e.g. in New York where consumption by affiliated concrete producers reached 29 per cent of total cement shipments.

What problems is the trend toward vertical integration between cement and concrete production likely to create? Vertical integration may reduce the competitiveness of markets and lead to a worsening of overall economic performance when it extends a pre-existing monopoly into adjacent markets, when it creates an integration "squeeze" in previously competitive markets, when it forecloses supply or market access to a competitive sector of an affected market, or when it raises additional barriers to entry. There is no clear evidence that any of these effects have actually occurred, though the report discerns the threat that all of them will. So far the effect of acquisition has been redistribution of supply and market outlets rather than foreclosure, and the total percentage involved is too small to permit an overall "squeeze" on non-integrated manufacturers of ready-mixed concrete. One difficulty in assessing the probable consequence of vertical integration is that ready-mixed concrete is itself a new industry; it had scarcely become well established before cement makers began acquiring parts of it. These producers formerly sold cement to wholesalers and building contractors; now the integrated firms will sell concrete to building contractors. Will this enhance their market power? A lot depends on the scale economies and entry conditions at the ready-mixed level, and information on this point is sketchy. The report does quote the opinion of some cement companies that they could not "afford" to enter ready-mixed manufacture except by acquisition, and that it would require "\$3-5 million" to enter by internal expansion, but does not explain how the more than 300 existing producers of ready-mixed concrete managed to "afford" it. Full integration between cement and concrete would no doubt increase the difficulty of entry into the cement industry, but we do not know how much.

The recent merger movements, horizontal and vertical, do not seem to have worsened economic performance in the cement industry as yet. The report shows that capacity has grown apace, keeping well ahead of demand; that profits have declined to quite modest levels in recent years; that prices have declined since 1960 and have shown signs of increased structural flexibility; and that producers have provided "faster and more convenient service to customers" in the postwar years (p. 63). Nor has the reduction in the number of cement producers resulted in markedly different structural conditions within the relevant markets. The case against mergers thus appears to rest on the general proposition that acquisition rather than expansion by internal growth in a context of oligopoly is very likely to strengthen monopoly materially—if it proceeds far enough. This is the view embodied in the "incipiency" provisions of the Clayton Act, which is designed to deal with tendencies and probabilities as well as with established monopoly.

JAMES W. MCKIE

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Utility Regulation: New Directions in Theory and Policy. Edited by WILLIAM G. SHEPHERD AND THOMAS G. GIES. New York: Random House, 1966. Pp. xii, 284. \$2.95.

This collection of nine papers on telephone and electric utilities is significant chiefly for not presenting new things, and overlooking important developments that are new.

Professor W. Haber's sense that inadequate earnings allowed by commissions have discouraged innovation neglects 85 years of remarkable advances. Though "continuous surveillance" of utility rates by commissions is presented as new and desirable, Twentieth Century Fund's *Electric Power and Government Policy*, written in 1940-42, found "something fundamentally wrong" with secret negotiations. The statement that "The era of suspicion, when both the regulated and regulators viewed each other as mortal enemies, is over," is inconsistent with the Dixon-Yates episode of 1953-56, the fierce struggle over generation and transmission cooperatives, and the deep antagonisms uncovered in the hearings of 1965 on the Smathers-Holland Bill (S. 218) to cut Federal Power Commission's jurisdiction over wholesale power rates.

Professor Shepherd apparently concurs with "critics" that the "best" regulation is "slow and ineffective" and "least calamitous." The basis is an unsupported thesis that regulation breeds inefficiency and unnecessary capital intensity. His estimated saving of "at least" \$2 billion by cutting reserves 8 per cent in 1960 is high by a factor of 3 or 4 if applied to the oldest generating plant. The statement that profits regulation succeeds mainly in deflation ignores FPC rate cuts since 1961. The explanation for regional differences in electricity rates neglects government competition.

Professor J. R. Nelson's model for indexed capital overlooks the historical success of utilities in raising capital, and the investor's continuous option to sell securities as price levels fluctuate. The discussion of marginal cost pricing omits the possibility of use patterns not changing correspondingly, or of off-peak becoming peak loads. It should explain why the Ontario Hydroelectric Commission is moving in the opposite direction.

Professor Gies, stipulating cost-plus pricing, proposes incentives and penalties to achieve "the proper allocation of resources among alternative uses, rather than mere profit limitation." Cost-plus does not explain the spread of rates of return from under 5 per cent to above 10 per cent within and among States. The need for incentives is contradicted by history. Improper resource allocation is unproven and inconsistent with the regulatory theory of the competitive analogue.

Professor C. E. Troxel, discussing telephone regulation in Michigan, inconclusively questions metropolitan rate schedule uniformity. He characterizes the Michigan commission as "tough" for reducing company requests, and cannot believe that there is tactical overasking. But his own statistic that allowed profit rose during 1948-61 from 6.0 per cent to 8.0 per cent indicates little toughness. Professor Ben Lewis provides a wise sermon urging companies to be philosopher-kings by surpassing commission minima, but it could as well have been written in 1940.

Professor G. J. Stigler's title is, "What can regulators regulate?" in electric-

ity. He is introduced as "a veteran tweaker of noses and ruthless enemy of bad logic." His initial error is the dictum that regulatory literature "touches seldom and lightly on the most basic question one can ask about regulation: does it make a difference in the behavior of an industry?"—for this question has dominated the literature for 60 years. The proposition that "innumerable" regulatory actions are "conclusive" of the desire to regulate is a non sequitur—a major criticism is that commissions don't want to regulate. Several errors of fact are contained in the statement that "professional" writers "hail" the beginnings of "effective" regulation from 1907, footnoted by the 1940 Brookings study, *Government and Economic Life*, and Twentieth Century Fund's study (*supra*). Only State commission regulation of electricity dates from 1907, with the cited volumes demonstrating this regulation to be largely ineffective. An oddity is data no more recent than 1937, despite much better data available since. Regulation is incorrectly said not to be intended to protect commercial customers.

Stigler's experimental design compares gross revenue per kilowatt-hour, with and without State commission regulation. A crucial flaw results from thus disregarding differences in proportions of industrial, farm, and residential loads and such elements as electric cooking, water, and space heating. Use of typical bills would have been sounder. The design error is itself due to a more basic defect, the absence of qualitative investigation and analysis. Twentieth Century Fund's study, for example, had already performed, with comparative thoroughness and wisdom, the comparisons attempted by Stigler, and had found significant differences where he could not. He might thus not have written that his confreres have "assuredly" not faced up to the problem of measuring the effects of regulation, that their judgments are "suspect," and that "They never question the importance of (effective!) regulation."

Utility Regulation omits mention of government competition as a regulatory device, although this arena is where comparative effectiveness of commission regulation is best measured. Though 40 years of regulatory frustration with reproduction cost was largely relieved by the *Hope* case (1944), there is little analysis of subsequent influence on regulation. There is nothing on the vital *Colton* decision of 1964 (*FPC v. Southern California Edison Co.*, 376 U.S. 205) which affirmed FPC's view that the interconnected engineering network of utilities is under its wholesale power jurisdiction, despite intrastate company boundaries. The industry's reaction at the 1965 hearings on S. 218—the most significant insight into the regulatory process since the 1930's—is overlooked. The postwar competitive role of rural generation cooperatives is unnoted.

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Transport and the Economic Integration of South America. By ROBERT T. BROWN. Washington, D.C.: The Brookings Institution, 1966. Pp. xiii, 288. \$6.00.

This book is concerned with what might be done to improve intercountry transport in South America in order to further the aims of the Latin American

Free Trade Area (LAFTA). In other words the author accepts the intercontinent common market approach as his basic assumption and his treatment of transport problems is conditioned by this assumption. A second assumption, which he also favors as a goal, is balanced urban development and the avoidance of urban gigantism.

Very briefly, the argument is that although South America may be a continent (almost no consideration is given to Mexico which is also a member of LAFTA, or to Central America) topographical features make road and railroad interconnections very costly so that to secure integration, reliance must chiefly be had on ocean and air transport. This broad generalization is not so valid for Region I, the industrial section of Brazil and Argentina (in rather old fashioned geophysical terms referred to as the "Industrial Heartland" of South America) and Region II the "Supporting Hinterland," which embraces the whole of Paraguay and Uruguay, that part of Argentina not included in Region I, the Brazilian Federal "Distrital" and provinces of Mato Grosso, Goiás and Rio Grande de Sol and the Bolivian department of Santa Cruz, where roads and internal water ways would serve. This leaves the other seven regions of South America more or less isolated from Regions I and II and from each other, although it is stated that the main problem resides in the lack of sufficient and low cost ocean liner service for general cargo from Region VI (Colombia and Venezuela) and Mexico to and from the other countries. Main attention, therefore, is directed to the desirability of securing such service. While such service would aid in generating more trade, it is recognized that the prior existence of a considerable volume of such traffic (at least potentially) is probably a condition for the provision of this service without heavy subsidies, and that the movement of general cargo is today very small. An alternative—air transport—it is argued, favors excessively large cities, though this has not been true in Colombia.

The main practical recommendations are the creation of new LAFTA maritime transport agencies—a shipping conference composed of shipping companies of LAFTA and of foreign countries serving this area and a LAFTA regulatory maritime commission. Traffic between the LAFTA countries would then be reserved to the conference members. The adequacy of such measures to achieve the purpose in mind appears to be doubtful, even to the author. He recognizes that to date "the maritime transport problem has been looked upon as one of protecting LAFTA ships rather than as one of improving maritime service" and that "inefficient ports and undisciplined port workers represent, at present, the single most important obstacle in this transport sector to economic integration in South America," and it is questionable whether these new administrative agencies can resolve these problems.

Finally, the completion of the Pan American Highway is listed as a prerequisite, though it is admitted that even this will fail to secure integration unless existing "bureaucratic controls and restrictions" on trade are removed. Curiously enough, the highly ambitious and costly proposed road from Venezuela along the eastern slopes of the Andes to Brazil is not mentioned except by implication as it is stated, quite correctly, that road transport along this route would present overwhelming problems.

Transport is evidently only one element, and not the most important one, in increasing trade between Latin American countries. Perhaps a more fruitful approach would be to follow up the clue contained in the statement "economic integration in South America will be significant solely if it is successful between Brazil and Argentina," though one might question the word "solely." This bilateral approach could be applied to other groups of countries, such as Colombia and Venezuela, instead of assigning to the latter grouping the rather startling role of the "Texas and Louisiana" of South America (because of their oil production), and the transport problems that Dr. Brown is struggling with would be enormously eased. The Declaration of Bogotá subscribed to by Chile, Peru, Ecuador, Colombia, and Venezuela in 1966 at least opens the door to the study of the desirability of smaller regional groupings. However, it would not be fair to tax Brown for failing to discuss possibilities he expressly excludes from his frame of reference. Within the limitations the author sets himself, the treatment is provocative and the collection and presentation of the statistical material is skillfully done. What is clearly brought out is that a transport program necessarily depends on what wider economic goals and programs are adopted.

LAUCHLIN CURRIE

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Transportation and Economic Policy. By JOHN B. LANSING. New York: The Free Press; London: Collier-Macmillan, 1966. Pp. xv, 408. \$7.95.

For a relatively short book, as textbooks on transportation go, 400 pages, Professor Lansing provides an excellent and broad view covering both intercity and intracity transportation of people and freight. He undertakes to look at transportation in microanalytical terms, theoretically, in 70 pages, and empirically in many other pages scattered among the sections on rail, highway, and the other modes. More than usual space is devoted to the point of view of economic development, with five principal country cases and several minor ones, containing both historical material and present day situations. There is a reference desirable but too brief to location theory and analysis and to transportation's role as just one of the inputs in production with significant possibilities of substitution of other inputs for transportation and the reverse. There is a concise and surprisingly complete coverage of government regulation, from both historical and analytical viewpoints. Costing is considered in its more general aspects. The book has a solid analytical approach and a good display of the relevant analytical methods, presented almost entirely verbally, there being only seven graphs and five equations.

The author does a good job of noting the limitations placed upon analysis by a dearth of statistics relating to some significant features of transportation in America and by the almost complete absence of meaningful data in underdeveloped countries. There is appropriate recognition of the difficulties of estimating demand elasticities and social benefits or costs, and of choosing the proper interest rates for use in public investment decisions. There might well have been more concerning the uncertainties in dealing with measurement of the capacity of transportation facilities and the quality of transportation ser-

vice. The problems in developing operationally useful bases for allocation of joint and common costs, a matter of universal importance in transportation, likewise need more emphasis.

There is a significant weak spot in coverage with respect to prices. Average revenue per mile figures, even by commodity or other particular groupings, do not provide enough evidence on prices to indicate the nature of market structures, the extent of discriminatory pricing practices, and the nature of carrier responses to changing demand and supply factors. The widespread practice in connection with intercity passenger transportation of providing first class and coach services with differentiated prices is not portrayed and analyzed. In the field of freight transportation, the more complex differentiations between prices for particular commodities, routes, and consignment sizes (less-than-car or truck load, carload and truck load and multiple carload) again are passed over. From the point of view of the economist, some of the recent European developments in revamping freight rate patterns, with greater consideration given to marginal costs, are highly significant. The major restructuring of U.S. railroad freight rates with the introduction of any-commodity carload rates for forwarders, and then piggy-back and multiple-car or train-load rates, are equally interesting. The role of price in allocation of resources to various transportation operations is too important not to be well covered.

KENT T. HEALY

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Land Economics; Agricultural Economics; Economic Geography; Housing

A Production Model for Wheat and Feedgrains in France (1946-1961). By BERNARD OURY. Amsterdam: North-Holland, 1966. Pp. xvii, 306.

The major concern of this book is "supply response" for wheat and feedgrains in France. This is expressed, in the core of the book, through the fitting of regression equations using time series data, with grain acreage, yield, and output, in turn, explained by a variety of explanatory variables. The usage of the term "model" here appears to mean a final version of a single equation regression which the author feels has the best properties of all the alternatives examined for a specific dependent variable. The model is thus a rather elementary construct, and, as the author notes in his preface, "generally this essay does not contribute any new theoretical breakthrough," since it essentially involves straightforward applications of least-squares.

In terms of organization, there are (1) a set of introductory chapters presenting background material on relations to be investigated and variables involved; (2) the regression results, with a chapter, respectively, for wheat and feedgrains, followed by an evaluation chapter; and (3) a forecasting application of the results directed to the consequences of alternative common market grain pricing policies.

Several limitations are noted in the introductory section: Only 16 observa-

tions were available on each grain category (covering the period 1946-1961) and only linear forms were used. Explanatory variables include measures of weather, price, and "technology." A number of weather variables are described, with ultimate reliance placed primarily on an index involving precipitation and temperature and on a measure of the severity of the winter. Similarly, a variety of price variables is introduced, with final choices involving wheat (or feedgrain) price relative to the wholesale price index and wheat price relative to feedgrain price. These price relatives are related to "transitory and permanent components" of production, respectively (pp. 10, 113, and 160); the terminology here is geared to ease of substitution in production. (Such terminology is fashionable, at any rate.) Prices are lagged, affording a rationale for single equation estimation, in addition to the author's appeal to authority (Wold, Waugh). The author attempts to measure technological change by fertilizer use. The notion of technological change shifting a supply curve is reasonable, but the use of fertilizer as a proxy is suspect at best; and the author's explanations—if anything—make matters worse (pp. 44, 50, and 51). Among other difficulties here, there is no distinction made between changes in underlying production functions and changes in input levels and proportions reflecting input price changes; and the final form of equation employed includes a production input and can be viewed as a blend of supply and production function.

The introductory material also includes a number of arguments on farm firm behavior and aggregate supply along the lines of doctrines propounded by Willard Cochrane, and there is a useful summary of French price and income policy for grains.

The regression results, in summary form, include the following points: A great many alternative equations have been fitted and presented, with the author displaying a good deal of persistence and some ingenuity in extracting maximum information from limited data. The marginal return to the reader for many of the alternatives presented, however, is rather low: In many cases, there is little difference in R^2 , and in meaning, among alternatives. The results are usually quite "reasonable." Thus, focusing on output equations with highest R^2 , we find for wheat that the weather indexes (as indicated above) have negative coefficients and the price variables have positive coefficients, as expected; the fertilizer variable, too, has a positive coefficient. For feedgrains, all the variables employed (winter effect, prices, and fertilizer) have coefficients of expected sign and are significantly different from zero.

The reasonableness of results is to some extent imposed by selection. This is defended as a virtue by the author (p. 164): "Regressions have been revised until the signs of the coefficients conform to reality and the main statistical tests are satisfactory." Virtue, if overdone, can be a vice. Certainly, some experimentation is called for in exploring economic relationships; but given enough alternative measures and computer time, perhaps *any* a priori "reasonable" hypothesis can obtain apparent confirmation.

A good deal of effort is devoted to considering direct estimates of output (based on the output regressions) versus indirect estimates (consisting of estimated acreage times estimated yield). The reason for this is not particularly

clear since (as one might expect) the direct estimates are better in terms of explained variance (based on applications of data given in the text). Estimates of price elasticities are obtained, and it is of interest that they are rather high, with both deflated wheat and feedgrain elasticities being approximately unitary and above previous U.S. estimates (p. 178).

In the final section of the text, the author applies the regression results to develop forecasts of the impact of alternative common market price policies for grains. The alternatives may be labeled high, medium, and low, for simplicity; results depend, in effect, on the elasticity estimates obtained. It turns out that the EEC has since selected essentially the medium policy (with a wheat price about 6 per cent above the current French price and 11 per cent below the German price) to go into effect by July, 1967. The author's forecast (for this policy) of a 5 per cent increase in French wheat output and a 12 per cent increase in feedgrain output thus yields some opportunity for checking a forecast against reality.

In retrospect, a major criticism of the book is that there is not enough reliance on economic theory (of the orthodox variety, at any rate). Thus, we are told that if product prices fall, farmers "have to produce more in order to break even" (pp. 17 and 50) and are advised to do so by "drastically increasing fertilizer consumption" (p. 212). This may be in line with some of Professor Cochrane's ideas, but it yields a rather unusual sideways V-shaped supply curve: If prices *change*, output increases. (Perhaps there is implicit here the notion that farmers are not maximizers, and the shock of falling prices forces more effective behavior. As a minimum, this is not particularly orthodox doctrine.)

Again, the author focuses on acreage and yield as *determinants* of output (see, for example, pp. 73 and 175). Of course, output is identical to acreage times yield (or output over acreage). Indeed, paraphrasing Gertrude Stein, an identity is an identity is an identity. Now, yield, in other terms, is the average product of land. Average product may rise because of technological advance, or because of changing relative prices of factors, or because use of land is restricted by government. The results may look the same numerically, but the meanings attached thereto will differ. Parenthetically, as every economist knows, maximizing yield is not particularly economic; assuming two inputs and constant returns, marginal product of the other factor will equal zero. Further, using yield as an indicator of "productivity" is akin to (and as wrong as) using average product of labor as the same sort of indicator. Perhaps an occasional reminder of such things is in order.

IRVING HOCH

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The Supply Responses of African Farmers. By EDWIN DEAN. Amsterdam: North-Holland, 1966. Pp. xiv, 174. \$7.00.

Edwin Dean's short book, in spite of its somewhat grandiose and misleading title, is an interesting attempt to quantify several crucial aspects of African agriculture. It is quite well written and has an important message for specialists concerned with Africa, agriculture, or economic development. In particu-

lar, it sheds some light on the elasticity of supply for cash crops, the characteristics of the labor supply function, and the "social factor" as a determinant of market price in Africa.

The geographic base for Dean's book is Malawi in southeast Africa. Most of the time he is specifically concerned with tobacco and tobacco producers in that country, and Chapter 1 is devoted exclusively to the recent history of Malawi and its economy. However, the significance of the book is not as limited as the one-country, one-commodity empirical base might imply. For in Chapter 2, Dean presents a useful approach for analyzing the largely subsistence agriculture which is typical of much of sub-Saharan Africa. In his framework, a clear distinction is made between the types of output for which growers have revealed a desire, i.e., home goods, cash goods, and leisure, and the types of inputs which are necessary to obtain these outputs, i.e., home-goods production, tobacco production, leisure, and other wage-earning activities in the nonsubsistence sectors. Using these categories, and the Hicksian notion of income and substitution effects, Dean analyzes the direction and magnitude of various types of farmer responses to price. For example, he argues that a rise in wage rates in nonfarm (or plantation) activities will cause a shift in labor and thus a decline in tobacco production. He is clearly not a believer in the concept of zero marginal productivity for labor in agriculture.

A theoretical chapter sets forth in a provocative way the complexities of "rational" economic behavior as seen by the African peasant farmer. It is particularly good in underscoring the employment and other interrelationships between the agricultural and nonagricultural sectors in a labor-scarce economy. Because of this relative scarcity, however, both the theory and its implications are significantly different from much of the recent writing on labor-surplus economies such as India and Pakistan.

While the analytical framework for agriculture presented by Dean has several advantages, the specific conclusions he draws for Malawi are, in part, dependent on some rather special assumptions. For example, the sharp distinction which he makes in the analysis between home-consumed goods and cash crops plays an important role. In assessing the implications of a rise in income from any source, he argues, "Since the income elasticities of leisure and home goods are both positive, by assumption, a net increase in the consumption of each . . . will occur. Since these goods can be consumed only if the grower and his family produce them, more [resources] . . . will be allocated to [their] . . . production. Therefore, the amount of land and labor allocated to . . . tobacco will necessarily decline" (p. 31). But this decline in tobacco supply is "necessarily" so, only if farmers cannot purchase (or barter) "farm-produced" consumption items. Certainly at the margin this trading possibility must exist, and hence one of his distinctions, and possibly one of his conclusions, remains open to question.

In the empirical portion of the study, Dean presents an excellent review and comparison of other estimates of agricultural supply functions from less-developed countries. He then uses a variety of first-difference models to show that lagged prices of the various goods described earlier are indeed an important determinant of fluctuations in Malawi tobacco sales. In these models,

Dean creates estimates of "expected" prices by using a weighted average of tobacco prices for three years; yet, interestingly enough, he avoids the use of a distributed-lag formulation, even though the tobacco price is often the only significant variable besides the weather. In addition, he shows an unusual concern about degrees of freedom, even though the time series used in the study exceed 30 years. Finally, the write-up of the statistical analysis is cumbersome and difficult to follow. While direct comparison with other studies is difficult, his results seem to show a relatively high price response on the part of Malawi tobacco growers, with supply elasticities on the order of 0.5.

In analyzing the trends (as opposed to fluctuations) in tobacco sales in the last 35 years, Dean shuns the use of prices as an explanatory variable. He argues that cost-reducing technical change and incentive effects of imported consumer goods were the prime factors in sustaining the two per cent annual growth. While these arguments are consistent with the data and are presented in a convincing way, several other hypotheses might have served equally well. For example, it would have been useful to have considered increased labor-land ratios as an explanation for increased yields—especially given the concern with the labor input in the monograph.

The final chapter deals with social forces as determinants of market prices in Africa. Three controlled experiments are reported, two from southern Malawi and one from the Congo. While the market discrimination chapter is obviously an appendage to the main portion of the book, the results are interesting and are occasionally humorous. One set of experiments was intended to discover whether members of one tribe sold at lower prices to their own tribesmen than to members of other tribes, while the second tested "whether young women sold at lower prices to young men than did old women" (p. 102). The general conclusion of all these tests was that social groupings were not an important determinant of price.

The most important contribution of Dean's volume is the readable theoretical chapter which provides a much-needed supplement to the recent literature on two-sector economic development models. In addition, the empirical estimates add to the growing evidence that prices and other economic variables are prime determinants of production, even in traditional agricultures.

WALTER P. FALCON

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Labor Economics

Unemployment, Money Wage Rates, and Inflation. By GEORGE L. PERRY.
Cambridge, Mass. and London: Massachusetts Institute of Technology
Press, 1966. Pp. xv, 143. \$6.00.

In recent years, beginning with the pioneering study by Professor A. W. Phillips in 1958, there have appeared a number of empirical studies to explain the behavior of wage rates. The present book by George Perry is a further contribution to our understanding of this phenomenon. The author begins by giving a brief survey of the important past publications which have been concerned mainly with the economies of the United Kingdom and the United

States. He criticizes these studies, especially those by Phillips and Lipsey, on the grounds that they are limited to annual data and thereby probably obscure short lags between the relevant variables. In the case of the United States, on the other hand, Perry points out the contradictory results obtained by W. G. Bowen and the present reviewer regarding the role of unemployment in determining wage changes. Perry believes that in this latter case an excessive concentration on one, rather than more, explanatory variables may be the reason behind this contradiction. Consequently, he advances the hypothesis that the changes in wage rates are determined by more than one variable: he identifies these as the level of unemployment, profits, cost of living, and the state of expectations (i.e., the direction in which the explanatory variables are moving). He sets out to verify this hypothesis by considering the postwar behavior of wages in the United States.

The author uses quarterly data and employs primarily the least squares regression methods. In order to point out the explanatory power of each variable, he builds up his analysis step by step starting with the relationship between wages and unemployment and adding cost of living and profits as explanatory variables. The final equation relates wage changes to the level of unemployment and profit rates and to the changes in the cost of living and in the profit rate. Perry finds enough evidence to support his hypothesis; R^2 for his final equation equals 0.879. The main policy conclusion the author derives from this equation is that there is little hope of combining low unemployment rates with price stability. On the basis of a 3 per cent annual growth in productivity, and profit rates stable at their 1947-60 average level, price stability would imply an unemployment rate of 6.4 per cent. The author discusses finally various policy measures that may be undertaken to change the structural relationship with a view to lowering the rate of unemployment that may be consistent with price stability. These measures include changes in corporation tax rates, accelerated depreciation allowances and other tax incentives, fostering competition, avoiding economic fluctuations, etc. However, these long-term measures would hardly change the basic relationship in the short period or even the medium period.

How far is Perry's extended equation a better explanation for wage changes in the United States than the earlier studies? The R^2 for his equation, with four explanatory variables, at 0.88 is only slightly higher than the corresponding figure of 0.80 obtained by the present reviewer in relating wage changes to only the level and the rate of change of profits. Considering the previous studies, perhaps it would have been better if the regression analysis had started with a single relation between wage changes and profit rates and then extended step by step to include other variables. But Perry appears to have a bias in favor of unemployment rather than the profit variable as the more natural (or the primary) explanatory variable and discusses the policy questions more in the domain of varying the (uncertain) relationship between wages and unemployment than the (less uncertain and more maneuverable) relationship between wages and profit rates. Finally, it would certainly have reassured the readers more if the question of possible correlations between the explanatory variables had been considered explicitly and shown as insignif-

nificant. Notwithstanding the above, Perry's book is a useful contribution on the subject, not only because it contains a valuable review of the past studies, but also because it tries to extend the analysis on a more general basis than hitherto.

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Public Contracts and Private Wages: Experience under the Walsh-Healey Act.

By HERBERT C. MORTON. Washington, D.C.: The Brookings Institution, 1965. Pp. x, 140. \$3.50; paper, \$2.00.

Wage Policy under the Walsh-Healey Public Contracts Act: A Critical Review. By CARROLL L. CHRISTENSON AND RICHARD A. MYREN. Bloomington and London: Indiana University Press, 1966. Pp. xiii, 239.

The time: 1935. The event: the National Industrial Recovery Act has just been declared unconstitutional. The response: Secretary of Labor Perkins tells Roosevelt that the minimum wage and maximum hour features of the NIRA can be partially salvaged by a law applying them solely to government contracts. Result: the Walsh-Healey Act of 1936.

These two excellent books describe the background and effects of this legislation. The Morton book is a model of clarity and organization; the Christenson and Myren book is somewhat less so. However, the latter authors assume a more difficult task in that they attempt a painstaking statistical analysis of the law's wage impact.

Although Walsh-Healey was passed in 1936, it has basically been administered as a supplement to the Fair Labor Standards Act of 1938. Both books suggest that if Perkins and Roosevelt had felt that the FLSA would be held constitutional, the Walsh-Healey Act would never have been proposed. But no matter . . . it has stayed on the books.

Although a supplement to FLSA, WH's legal provisions are significantly different, and herein lies an interesting tale in the history of government intervention in the economy. FLSA minima are legislated by Congress; WH minima are determined by Secretary of Labor fiat (since 1952 subject to judicial review). FLSA minima are nationwide in scope whereas WH's are restricted to a very fuzzy concept of "locality." Also, WH has certain extra legal exemptions (such as the statutory minimum of a \$10,000 contract and the exemption of subcontractors) which complicate implementation.

Morton makes painfully clear the tremendous morass of formalism and legalism that has engulfed WH—a virtual bureaucratic nightmare or delight, depending upon your perspective. Labor and business have fought over every conceivable ambiguity in the law, of which there are several. The Congressional dictate to the Secretary of Labor is a natural for disputation: to determine "the prevailing minimum wage." This is not the European concept of a "living wage" or a "fair wage." As Morton points out, this makes the Secretary "a wage finder, not a wage fixer." But how to define "the prevailing minimum" has, as might be expected, plagued the administrative branch of government. Unions have maintained that it means the union contractual rate; business representatives have insisted that it means the lowest end of the in-

terquartile range in BLS surveys. The Secretary of Labor has tended to compromise on the median rate in BLS surveys, naturally satisfying neither of the parties.

Christenson and Myren attempt a detailed analysis of the law's impact in four industries. Morton's study centers more on the procedural aspects of the law. But both books agree that the wage impact has been slight. They feel that very few workers have been affected and that there have been few indirect effects in terms of intra-firm, intra-industry, or inter-industry adjustments.

Both books raise, but do not sufficiently answer, the question of whether the law should be amended or repealed. Both express a dissatisfaction with the law and call it "ineffective," but neither adequately defines what an "effective" law would be. They both convey the feeling that if there is such a law *it should be doing something*, but that the predominant thing this law is doing is contributing to bureaucratic red tape. Certainly one goes away from these two books with the feeling that the administrative cost to government, business, and labor far exceeds whatever "good" this law may have accomplished for a few workers, or "bad" it may have imposed on a few business firms.

In my opinion, one procedural aspect of the law that merits much more attention than it received in either book is the political and economic consequence, both potential and real, of lodging so much authority in the executive branch of government. *So much* is left to the discretion of the Secretary of Labor. It is not only up to the Secretary to determine "the prevailing minimum wage," but also to decide for which industries and localities such "prevailing minima" should be established. We live in an age in which the power of the executive branch of the federal government is being exercised imaginatively and threateningly towards both business and labor. The power of the Secretary of Labor to issue or change a "prevailing minimum wage" is a natural lever for use by the President in his efforts to exert administrative pressures against individual industries, firms, or unions. The FLSA is a clear infringement on the free enterprise principle of letting market forces be determinate, but at least FLSA simply sets uniform standards for all industries covered, with the wage minima established by Congress. WH goes one step further in relying on flexible executive discretion to be determinate. I shudder at the thought of WH procedures being adopted for FLSA. The implications and propriety of such extreme reliance on executive discretion in setting minimum wages is unfortunately not considered in these otherwise excellent and complementary books.

RALPH C. JAMES

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Women in the Soviet Economy. By NORTON T. DODGE. Baltimore: Johns Hopkins Press, 1966. Pp. xviii, 331. \$10.00.

This thorough book surveys the high level of educational achievement and labor force participation by women in the Soviet economy. The basic explanation for the major role played by Soviet women is the depletion of working-age males due to the wars and political purges of the last half century. At the

same time, rapid industrialization required large numbers of workers, including many newly trained technical personnel. This economic stimulus was reinforced by an ideological belief in the equality of women. Thus, the propaganda organs of the state were used to induce women into the schools, fields, and factories, and legislation prohibited employment discrimination on grounds of sex.

The lack of total control by the regime has been evident, particularly in the Moslem-dominated republics of Central Asia, where women still occupy a noticeably subordinate position. Elsewhere, however, the government efforts have met with impressive success. Women constitute a majority (52 per cent) of the labor force, with participation rates remaining high (near 80 per cent) even in the child-bearing and rearing ages. Thus, the Soviet Union has not yet had to face a problem of increasing concern in the United States—women who leave their jobs in their twenties and thirties and after a prolonged absence, during which their children are growing, would like to reenter the labor force. Also of little import in the U.S.S.R. has been the female demand for part-time rather than full-time employment. One wonders whether these problems will become more pronounced in the future, as the Soviet standard of living rises, relieving the pressure of economic necessity on female workers and making it possible for them to withdraw from the labor force while raising their families. Tapping female resources under these conditions may require more imaginative experimentation with educational and employment patterns—for example, adapting traditional recruitment practices, job training procedures, and work hours to meet the changing needs of women. This is a topic which might also be given greater attention in the United States, particularly in an era where long-run full employment is maintained.

Dodge's most valuable contribution comes in the latter half of his book, where he examines in detail the types of jobs held by women. Women constitute almost half of the industrial work force and over half of all "mental workers," a category which includes professional and scientific personnel. In this respect, the Soviet Union is probably utilizing its female resources better than any other country in the world, developing to a fine point the intellectual abilities and career motivation of its women. The United States might learn from this example.

Yet Dodge claims that women are not proportionately represented in positions of greater leadership and creativity; men still receive most graduate degrees and occupy most managerial and top government and party posts. (In fact, approximately 80 per cent of women workers are doing physical labor, the majority of these in agriculture, where women have historically predominated and continue to do so.) How can we explain this phenomenon? Dodge dismisses (perhaps too readily) the hypothesis that this is due to outright discrimination, although he does suggest that men benefit from the right contacts necessary for promotion to the top echelons. More crucial, he believes, is the handicap women suffer as a result of bearing the dual burden of family and job. This dilemma, apparently, has been resolved no better in the Soviet Union than in the United States, and in some ways less well. While the regime encourages female employment, it has been unwilling to divert

resources for an adequate number of nurseries or kindergartens to care for the young children of working mothers; only 10-20 per cent of children under age seven can be accommodated, and, unlike primary and higher education, this service is not free. Production of consumer durables has also been a low-priority item. Thus, domestic chores demand much of a women's time and energy, diminishing her desire for advanced training and her on-the-job productivity.

Dodge estimates that the lifetime work output of an average professional woman is 14 per cent less than that of a male, because of time lost through family obligations, health problems, and early retirement. He introduces several other measures of female productivity—by studying the sex distribution of memberships in scientific academies, of recipients of the Lenin prize for science or art, and of contributors to scholarly journals—and finds women clearly inferior here too. Some of these appear rather unconvincing as indices of productivity, and a bit of circular reasoning may be involved. Nevertheless, Dodge concludes that this lower female productivity explains the absence of women from the more responsible positions and justifies recent changes in educational admissions policies which favor males.

The book, enriched by extensive tables and graphs, provides an abundance (indeed, perhaps an overabundance) of data on female education and employment in the U.S.S.R. The careful reader may be disturbed by occasional unexplained statistical inconsistencies (as in Tables 21, 23, 24, 27, 29, and 90). Some of the early chapters are relatively limited in analytic content, but this is remedied toward the end. The discussion of the latter chapters is highly relevant to Western experience; at times one only wishes that Dodge had developed this international comparison a bit further.

ESTELLE JAMES

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Labor Market Institutions and Wages in the Lodging Industry. By JOHN P. HENDERSON. East Lansing: Michigan State University, Division of Research, Bureau of Business and Economic Research of Graduate School of Business Administration, 1965. Pp. ix, 227. \$6.50.

The nature of our economy has been changing rather significantly over the past decades. By the end of World War II we had clearly shifted from an agriculture-oriented economy to a manufacturing-oriented one. Only in recent years, however, have we begun to recognize the increasing importance of the service sector of the economy. The shift to service trades and industries is now occurring at a growing rate. But so far, not too many professional economists have considered this shift sufficiently significant to study and analyze the nature of these service industries. We are still putting forth a disproportionate amount of time and effort studying the manufacturing sector of our economy. And when individual industries come under close study, economists still seem to find more glamour in industries such as steel, autos, and rubber. The book here under review takes on the task of studying the labor market aspects of the lodging industry, one of the more important service industries in the United States.

John P. Henderson takes the standard steps necessary to report on and ana-

lyze the labor situation of an industry. He first analyzes the growth of service industries in the United States, and then goes on to describe the economic structure of the lodging industry. The industry incorporates a rather wide variety of economic units and activities: we have the small five-room hotel or motel with no payroll and the large luxury-type hotel with over 100 employees; there is the small road-side motel and the large town-motel with all the luxury services; there is the establishment furnishing lodging only and the one furnishing hotel rooms, plus restaurant, bar, entertainment, and banquet facilities.

The chapter on labor-market institutions is rather thin. References are made to obvious factors, such as unionization, the skill composition and the ethnic composition of the labor force, but none is treated in depth. However, in the next chapter, Henderson comes to grip with the basic problems of the wage structure, and here is the heart of the study. Henderson has done a creditable job of putting together and analyzing the rather scarce wage data available for the lodging industry. The structure is examined from the point of view of geographic differences among the various job classifications, and from the point of view of differences between the tip and the non-tip occupations. Again we have a verification of wide variations in wage structures among cities in different areas, and of differences between tip and non-tip classifications.

The remainder of the book discusses such topics as the minimum wage controversy in the industry, multi-employer bargaining, and the problems of federal jurisdiction over the industry. Some aspects of the topics discussed in the final third of the book appear as appendages to the problems of labor markets and wage structures discussed earlier.

Labor economists, and also the general reader who is interested in learning something about the labor market situation in other than steel or autos, will find this book a contribution to the field. Henderson writes well and presents his material in an orderly fashion. A complete reading of the book will give an understanding of, and a feel for, the lodging industry. While the reviewer would have preferred more emphasis upon labor market institutions and wage problems, this is a matter of taste. One has to admit that the study is a significant contribution in an area where we should encourage more work to be done.

MORRIS A. HOROWITZ

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Occupation and Pay in Great Britain, 1906-60. By GUY ROUTH. New York and Cambridge: Cambridge University Press, 1965. Pp. xi, 154. \$6.50.

After preparing a budget speech a weary Gladstone once wrote: "A load of figures has a suffocating effect upon the brain until they are well drilled and have taken their places." Fortunately for those interested in occupational wage differentials Professor Routh has drilled his figures admirably, and made them take their informative places. His book originated in a charge from the National Institute of Economic and Social Research to investigate "the changing economic fortunes of the principal 'Common interest groups' of which society is composed." It became primarily a presentation of "the changing size

of occupational classes and . . . their concomitant pay structure in Great Britain in the years 1906-1960." If we were fortunate enough to have a similar study of U.S. materials it would be readily recognizable as an NBER report—data having been extensively accumulated, effectively ordered, then altruistically made available to the profession for analysis. However a concluding chapter does offer a mixed bag of perceptive and contentious "interpretations."

Routh converts the established "social classes" of the Registrar-General's reports to "occupational classes," then reviews how numbers in these classes and many individual occupations, changed. His discussion of training time notes "surprisingly small differences" between formal education levels for unskilled and foremen, then emphasizes the penumbra of on-the-job training. No attempt is made to measure the advantage or consequence of such training—whether formal (*à la* Keat, Mincer, or Oi) or informal. Some international comparisons are made, particularly with the United States, leading to the suggestion that "technique and social advance" may override national boundaries to produce "similarities more striking than the differences."

In Chapter 2, on income and pay differentials, Routh finds a compression in the male income tax distribution from 1911 to 1958, but a widening distribution for females. Data added on weekly earnings by manual workers (in 1906, 1938, and 1960) help suggest that compression was achieved by 1938. They imply, however, that not much can be made of the income tax data for females until some adjustment is made for a possible increase in part-time workers. The bulk of this second chapter reviews trends in pay for individual occupations, and affords us a basis for more interesting inferences than can be drawn from net changes in the income distribution.

Chapter 3 offers a bewilderingly rich set of material on the time and circumstances under which the changes for the particular occupations took place. The familiar puzzle of a one-third cut in wage rates from 1921 to 1924, followed by a mere 5 per cent cut from 1924 to 1935 (despite unemployment rates of from 10 per cent to 20 per cent in every year), stares out of one table. Routh's new data accentuate the puzzle: despite the collapse of mining, textiles, and engineering, declines for many of these industries were minor. Actual rises during 1924-35 appear for some industries, for various occupations. Some reference to the extensive study by Janet Chapman would have been welcomed here.

In a concluding chapter on "interpretations" shrewd intuitions jostle explicit reservations about applying supply and demand concepts to the labor market. That the pay of the unskilled rose more than the skilled from 1914 to 1924 Routh finds to be "the reverse of what demand-supply relations would suggest"—apparently because the unemployment rate for the unskilled was greater than that for the skilled by 2 points in 1913 and by 23 points in 1921. Relating 1958-60 averages of vacancies to unemployment counts by occupation Routh finds that "great variations [appear] between occupations, which demand and supply theory postulates should be equalized by changes in relative pay." It is cheering to know that Marshall has not exorcized the ghost of Cliff Leslie, but depressing to find classical theory seen through fun house mir-

rors, demanding that the adjustment to excess supply (or demand) take place within a given time span. (Why should it require the complex impacts of the war rise and postwar catastrophe to cancel neatly? And even if so, why by 1924?) This section concludes that "in general the labour market does not operate in the way conceived by the theory of demand and supply; the price of labour may rise in the face of high unemployment; it may rise at similar pace for occupations showing contrasting demand-supply relationships," etc. Whose theory is being rebutted here is not clear. More important the author would have had a comparative advantage in utilizing the rich array of English data on vacancies, unemployment, and pay rates year by year and occupation by occupation in a model that even roughly, but systematically, allowed for the complex facts of the real and the (usual) theoretical world: job substitution and dilution, adjustments to excess supply completed at no constant rate and varying among industries, shifting demand schedules, the presence of unemployment insurance and employee savings, etc.

A concluding section, "An Explanatory Hypothesis," begins: "The outstanding characteristic of the national pay structure is the rigidity of its relationships," and goes on to note that despite fluctuations these "have the capacity to regain previous shapes, sometimes after lapses of many years." From this the writer eventually concludes that "attempts to formulate a national policy or manipulate a pay structure are misconceived if they are based on a belief in the effectiveness of price in the regulation of supply and demand. Bishops, judges and cabinet ministers have had their real pay drastically reduced in the last fifty years, yet there is still no shortage of candidates for those positions." It is, of course, a welcome pleasure to find the cut direct to both believers in free markets and in incomes policies or central wage fixing. But no blood will flow until, at the very least, it is established that Heath and Brown are equivalent say to Lloyd George, or Profumo to Morley. True, pay incentives probably never elicit a Pitt, or even a Perceval. But will they put a better bobby on the beat or a worker on the line? This is the more immediate concern of wage theory and wage policy. However one may differ on the "explanatory hypothesis" there can be no question that the extensive set of materials developed for this study will be of durable use to those concerned with the study of wage problems.

STANLEY LEBERGOTT

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Wage-Structure in Theory and Practice. Edited by E. M. HUGH-JONES. Amsterdam: North-Holland, 1966. Pp. x, 242. \$7.00.

The editor, E. M. Hugh-Jones of the University of Keele, describes this book as an experiment. Economists in three countries—the United States, Great Britain, and France—were given the title and identical but short instructions and then left to write their essays as they pleased. This turned out to be an experiment not only in labor economics, but also in anthropology. Quite clearly, the U.S. and British contributors share roughly the same cultural heritage which makes them ask similar questions. The French contributors Jacques Lecaillon and Jean Marchal—whose main ideas, incidentally, had

been expressed previously in their monumental three-volume work, *La répartition du revenu national*—have quite different concerns. Mark W. Leiserson (U.S.) and J. R. Crossley (U.K.) inquire into the contribution which economic theory can make to our understanding of wage structures. If their answers differ, their questions and the way they go about answering them are quite similar and betray their common neoclassical Anglo-Saxon intellectual ancestry. The main interest of the French authors lies in identifying socioeconomic groupings among the wage recipients which a theory of distribution could operate with. These groupings are quite different in nature from—say—those an economist may establish by determining the degree of substitutability of one kind of labor for another. The French groupings are derived from such elements as social origin and legal distinctions. Most of them are broad categories such as “non-manual junior employees (clerical workers and civil servants).” Such wide groupings may be indispensable for many purposes, but not necessarily those which Leiserson and Crossley had in mind. The distinction between the general line of investigation of the French authors and that of the other two essays is the more remarkable as Marchal and Lecaillon are admirably well informed about published work on the theory of distribution in England and the United States. Other distinctions in the three essays, e.g. different weights given to such factors as a sense of social responsibility or class solidarity in wage determination, might also be of concern to a student of comparative economics.

For the “pure” economist, the main interest of the essays is likely to be in the authors’ attempts to answer the question of how much economic theory can help in explaining short-term changes in the wage structure. The authors seem to agree that in the long run competitive wage theory has a good deal to say in the analysis of changes in the wage structure. From this point on, however, the authors part company. Leiserson doubts whether conventional theory has much to offer for the explanation of short-term changes of the wage structure. The emphasis on equilibrium relationships in particular prevents “traditional theory” from throwing “much light on the dynamic process of wage setting” and the significance of changing institutions in the economy. No great progress, moreover, has been made in marrying institutional factors with the competitive hypotheses of conventional theory, however modified by assumptions of oligopolistic behavior. Indeed, some of the institutional regularities in an earlier period that appeared to contain the germs of possible generalizations such as the “key bargain” and the “wage pattern” have lost a good deal of their validity either in the light of later events or as a result of closer analysis. Dunlop’s “wage contours” have withstood the passage of time, but have remained so far a significant descriptive characteristic of wage changes in the United States without a theoretical foundation which would permit integrating the concept with other theoretical propositions. Crossley holds the opposite view. Market conditions have their impact on both wage rates and earnings in the short run. The empirical tests for this hypothesis are derived from the following reasoning: Since short-term supply curves of labor to a given industry or firm are less elastic than their long-term counterparts, a relative increase in the demand for labor will call forth an increase in supply

only if there is some increase in the relative wage. With the passage of time this increase may be reduced as long-term supply elasticities are higher. The competitive theory thus leads to two testable propositions: the wage structure changes more over the short run than over the long run and relative wage changes in the short run are positively correlated with relative employment changes. No such implications follow, according to Crossley, from nonmarket theories of the wage structure. Some empirical evidence for the two propositions is found in studies by Phelps Brown and Browne, Reddaway and others, but taken together the evidence can at best be described as suggestive. However, an examination of the evidence in support of the Phillips hypothesis modified by the adoption of three different wage measures (changes in rates, earnings, and wage drift) is taken to indicate that "market conditions in the short run . . . bear on the determination of the earnings level both through their effects on wage rates and on the gap between rates and earnings" (p. 223).

This is highly ingenious reasoning and I hate to question its validity. Yet, not only is the evidence cited by Crossley far from conclusive but also somewhat ambiguous. The main pillar of Crossley's reasoning needs additional support if the structure he wishes to build on it is to be stable. The degree of supply elasticity varies not only with the time span under consideration, but also with other factors including the level of unemployment in the economy, the area, the industry and occupation, as well as the facility of arranging or withdrawing overtime, etc. Changes in the qualifications required for a given job or in vacancy rates tolerated may take the place of wage variations and statistical data may conceal these phenomena. Lags in wage responses to changing demand conditions may also blur the picture. It is thus conceivable that, under certain conditions, supply elasticities may appear to be greater in the short run than in the long run or that relative wage increases or decreases in earnings follow relative employment changes with considerable delay. Crossley himself is disturbed by the high short-run elasticity of labor. The reason for this, he thinks is the reduction of uncertainty about the maintenance of a given wage through collective bargaining. This is unconvincing as far as wage rates are concerned, but may have some relevance for yearly earnings. Since, however, the impact of bargaining on uncertainty as regards lay-offs and short hours appears to be about the same in different industries under contract, it is difficult to see how collective bargaining could affect supply elasticities in a universe of unionized union-influenced firms.

I am not entirely convinced by the evidence regarding changes of the wage structure in the long run. It is conceivable that over long periods supply and demand schedules adjust to autonomous changes of the wage structure rather than the reverse so that the market may "justify" changes *post hoc* originating outside of the market. Far more intricate research methods will thus be required to test alternative explanations and to control for these and many other variables, before Crossley's tests can turn out more convincing results. Yet, his first-rate piece of analysis outlines a promising fresh line of research.

If the French study has been somewhat neglected in this review, this is not intended to be a reflection on its merits. Its most significant contributions,

however, are in areas closer related to sociology. The review of statistical material on French wage differentials, however, is meritorious, though somewhat dated.

ADOLF STURMTHAL

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Population; Welfare Programs; Consumer Economics

The Economics of Education—Proceedings of Conference Held by the International Economic Association. Edited by E. A. G. ROBINSON AND J. E. VAIZEY. New York: St. Martin's Press; London: Macmillan, 1966. Pp. xvii, 781. \$16.00.

This volume contains 24 papers on the economics of education presented at the International Economic Association's conference at Menthon St. Bernard in 1963.

The first section of the book is devoted to "General Problems of Education" and contains an interesting and useful article by F. Edding on education expenditures. Edding presents historical series of expenditure on education in 9 countries, extending in most cases to the pre-World War I period, along with a survey of expenditure in 20 countries over the decade of the 1950s.

The second section, entitled "Education and Economic Progress," contains the first generally available English translation of the Soviet academician S. G. Strumilin's celebrated 1925 paper on "The Economic Significance of National Education." Strumilin estimates the relationship between the level of skill, and age, years of employment, and years of education in a large sample of Leningrad metal workers. The measure of skill is based on official skill gradings and pre-World War I relative wages for each skill grade. The data provide an interesting insight on the skill formation effects of both on-the-job learning and education. A number of problems are not adequately treated in the paper; the chief ones are the failure to correct for the fact that both the duration and the quality of a year of schooling vary over time, and the difficulty of distinguishing between the effects of education on skill and the effects of other attributes, such as measured intelligence and family background, which are closely associated with the number of years of education. Further, in view of the fact that the results of Strumilin's work lead him to suggest very major changes in the amount of resources devoted to education, some recognition of the dependence of the earnings of various types of labor on the supply of each type of labor in the labor force would have been appropriate. Strumilin's estimates of the social rates of return on educational expenditures will be regarded by many contemporary economists as upward biased for they involve no discounting of future benefits, but given the circumstances one is tempted to view this aspect of the analysis as akin to the talking horse that is remarkable not so much for what it says, but because it talks at all.

It is a pleasant irony that the group of economists who set out from Chicago in the late 1950s on their exploratory voyage in the application of capital

concepts and market valuations to educational problems should now find that they had proceeded much of the way in the footsteps of a voyager who had embarked at Leningrad thirty years earlier; this coincidence no doubt constitutes one of the rare contributions to economic analysis originating jointly (if independently) with the Soviet Academy of Sciences and the University of Chicago department of economics.

M. C. Kaser's statistical study of patterns of educational evolution in the process of economic growth in the industrialized non-communist economies is an underanalyzed array of potentially very interesting data.

In Part III of the book, for some reason labeled "Demand and Supply," Raymond Lyons provides a good survey of the OECD's Mediterranean Regional Project, one of the most ambitious educational planning exercises yet attempted.

Part IV is devoted to "The Cost and Financing of Education." Mary Jean Bowman has provided a nicely argued piece on the nature of educational costs and some problems concerning dynamic aspects of training and education. Selma Mushkin raises the much ignored problem of the obsolescence of skills and offers a proposal for the re-education of adults on a mass basis which should attract considerable attention. John Vaizey's paper on expenditure criteria in the field of education surveys and finds considerable fault with a number of alternative approaches and concludes doubting "whether in the long run the concept of education as 'investment' will prove much of a guide on 'what ought to be spent on education'."

Part V on the "Balance Between Different Forms of Education" is seriously weakened by the failure of the authors to cope with the problem of establishing an operational or even conceptually interesting definition of balance. (As far as I could determine, the authors thought that balance was a good thing.) Michel Debeauvais' paper provides an interesting survey and some striking examples of statistical pitfalls in quantitative international comparisons of educational structure. The remaining papers in this section are devoted to problems of vocational and technical education.

In the concluding section on international aid to education H. M. Philips' paper provides a survey of the very sketchy available information on bilateral and multilateral educational assistance programs to the underdeveloped countries. H. B. Ripman's paper recounts the lessons learned by the International Bank's then recent entry into the field of financing educational projects. The absence of any consideration of the reverse flow of human capital from the poor to the rich countries is an unfortunate omission in this section.

The volume contains a number of articles directed to economic problems of education in particular countries: Nigeria, Turkey, Pakistan, the Soviet Union, and the OECD area. While these papers contain a sprinkling of insights they offer little material of general interest. At least three papers in the book (those by E. F. Denison, I. Svernilson, and F. H. Harbison) have been in print elsewhere for some time; their republication here hardly seems necessary.

The contents of this book are convincing evidence that the economics of education field offers both ample scope for continued controversy and a fertile

ground for further research. One of the many persistent differences of opinion among conference participants concerned the use of earnings as a measure of the economic effects of education, and the related problem of the foregone earnings component of educational costs. Much of the work presented at the conference, as well as more recent research, suggests that policy prescriptions based on the discounted future earnings approach differ in a rather consistent way from those generated by the various versions of the manpower requirements approach to resource allocation in education. An exploration of the sources of these differences might shed some light on the range of applicability of the various competing planning methods put forward at the conference.

A number of important problems were not seriously discussed at the conference. Chief among these omissions were the empirical investigation of the very imperfectly understood relation between education and earnings and the absence of any work on educational production functions. Further progress in these areas, and in the economics of education field generally, depends in large degree on careful empirical studies, probably at a fairly high level of disaggregation. Greater emphasis on this type of work at the expense of the wide geographical distribution of case studies would have done much to increase the usefulness of this volume.

The publication of this book by the International Economic Association can be thought of as a milestone in the recent development of the economics of education. With the exception of a very few papers, however, it adds very little to either our conceptual tools or our empirical understanding of the problems in this field. Taken as a whole, this eagerly (and longly) awaited volume will be a disappointment to most readers.

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The Profitability of Investment in Education in Israel. By RUTH KLINOV-MALUL. Jerusalem: The Maurice Falk Institute for Economic Research in Israel, 1966. Pp. xiv, 109. (Published in English and Hebrew.)

This rather short book is primarily empirical, analyzing in great detail the profitability of investment in education in Israel. It provides useful results which can be compared to similar investigations in other countries (thus providing a basis for formulating hypotheses about education as a productive factor) and of course the conclusions will also be useful in guiding decisions for educational policy in Israel. Theoretical issues play two roles: as in any empirical investigation, they are used to indicate the relevant factual evidence; and they are important when the author considers the implication of economic growth upon education's profitability.

The earlier chapters present and analyze the data used to estimate the returns to education. The present value rather than the internal rate of return technique is used throughout, and values (at age 21) of various income streams are provided using alternative discount rates of 6, 8, and 10 per cent. Later chapters consider the cost of education, both direct expenditures and income foregone. The very surprising conclusion is that, after completion of primary education, there is no clear-cut evidence that investments in educa-

tion are profitable, when the opportunity cost of money is 8 per cent or greater. This is true whether social or private returns are considered although social returns appear to be higher, primarily because the loss resulting from increased income taxes more than offsets the gain realized by the subsidized education (tuition less than total cost).

The brevity and the level of the exposition make the book difficult to read. The text consists of only 76 pages and has been gleaned to the bare essentials. Introductory arguments are often sketchy and the reader should be familiar with the more common statistical and theoretical problems of evaluating education's costs and returns. Owing to its brevity, the text contains many statements of the "if-then" variety which are not supported and tax the reader's ability to discern their accuracy. In general they are accurate and support the impression that the author is an extremely competent analyst.

Several short appendices follow the text. They indicate the wide range of phenomena considered in the study as well as suggest important issues to be considered. The appendices are particularly interesting for it is here that some of the rigor and care that characterize the text are relaxed and the author allows herself freedom to speculate.

The description of the empirical techniques deserves praise. The methods are obvious to the reader and one feels that, given the basic data, the results could be recomputed. This is certainly superior to most presentations in which the underlying "adjustments and corrections" are swept under the rug. When an arbitrary decision is made, the reader is informed. However, the numerous assumptions necessary to reconcile data from several sources and to use cross-sectional estimates of age-earnings profiles lead to the authors' admonition against reaching unwarranted conclusions from the results. Indeed she suggests that the principal role of the study is "... to systematize thought in an important field" and to "... serve as a basis for further research." Here, it seems that she worries too much. It is likely that the evidence provided in this book is superior to any other concerning the profitability of educational investments in Israel. Surely many simplifying and perhaps heroic assumptions were required, but complex problems always do! To ignore conclusions based upon such assumptions begs the whole point of the alternative cost theory. For until other data are available what alternatives exist?

The analysis is couched in terms of a three-variable production process in which education complements other factors and tangible capital substitutes for unskilled labor. Thus, when considering prospects for future profitability of education, Miss Klinov-Malul concludes that, if the impetus for economic growth comes from education, its profitability will decline, but if capital accumulation provides the impetus, it will increase. It is also argued that, if growth occurs through technical change that is neutral between education and other factors, then the profitability of educational investments will increase. This proposition appears in both the introductory and concluding sections, but the reasons for such a conclusion are illusive. The argument is simply that neutral growth will increase the wages of skilled and unskilled laborers by the same proportion. Since the return to education is the difference between the skilled and unskilled wages, the return to skill acquisition also increases equi-proportionately. But what about costs? If the marginal products of all factors

increase in proportion to the technical change then costs must have increased in proportion to returns. Investments that were profitable before the change will be more profitable afterwards but marginal investments will remain marginal and those that involved losses will involve even greater losses.

If the technical change increases all wages equiproportionately but does not involve corresponding increase in the marginal products of other factors, then education will become more profitable but not so much as would appear at first blush. The return would obviously increase but so would cost. Education is labor intensive, the two most important elements of cost being income foregone and instructional salaries, each of which will increase in proportion to the return. Too, prices of other factors will increase since labor is used in their production. Thus the net gain in profitability is less than normally assumed.

The book summarizes a difficult piece of research performed by a competent analyst. Most of the tools are not new yet they are not misused. The conclusions are interesting and add significantly to our growing body of knowledge concerning education as an investment alternative and as a factor of production. I would certainly recommend it to anyone formulating educational policy in Israel as well as to the serious student of human capital. The casual reader can do better elsewhere.

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Related Disciplines

Secrecy and the Arms Race. By MARTIN C. MCGUIRE. Cambridge: Harvard University Press, 1965. Pp. x, 249. \$5.95.

As the subtitle indicates, this book elaborates "a theory of the accumulation of strategic weapons and how secrecy affects it." While the debt to previous authors, in particular Lewis F. Richardson, Kenneth Boulding, and Thomas Schelling is clear, the author has succeeded not only in synthesizing their approaches but also in developing new and important aspects of the theory of arms races where an arms race means here an interacting sequence of decisions by two nations to acquire missiles. (McGuire considers only one type of weapon, a missile.)

Perhaps the most important contribution of this book to the scarce literature on the subject of arms races is its use of explicit economic concepts. Conversely, from the viewpoint of economics the book indicates a new field ripe for economic reasoning. The problem of economic allocation considered by McGuire is that of allocating a given (scarce) level of national wealth between the competing ends of missiles, which have no intrinsic value, and alternative uses of national wealth, which do have an intrinsic value. Why a nation should then buy missiles or, equivalently, why missiles should have some indirect value, is seen on the basis of a second problem of economic allocation. The second problem is that of allocating a given (scarce) number of missiles among the competing ends of firing missiles against enemy missiles (a *counterforce* operation), firing the missiles at enemy cities (a *countervalue* opera-

tion), and not firing the missiles (a *holding* operation). McGuire assumes that the solution to this second problem is one for which the attacker fires a certain proportion of his missiles (but not all of them) in a counterforce operation against his opponents' (the defender's) missiles. The number of missiles remaining for the defender after this attack is then a random variate depending on the number, yield, and accuracy of the attacking missiles and the number and vulnerability of the defending missiles. The minimum number of defending missiles that would, with some specified degree of confidence, survive an initial attack by the other side is then the retaliatory capability, a concept which plays a central role in McGuire's analysis.

McGuire considers only two countries and assumes that the national utility functions, summarizing the "tastes" of the problem, depend on the retaliatory capabilities of both sides and the alternative uses of national wealth. If the two countries are X and Y , then the national utility function for X depends on the retaliatory capability of X (assuming Y strikes first), the retaliatory capability of Y (assuming X strikes first), and the alternative uses of national wealth for X (other than for missiles). An increasing retaliatory capability for X increases utility (but at a decreasing rate) since it would, if known by Y , deter Y both from attacking initially and from subsequently attacking X cities for fear of retaliation. To take an extreme case, if X had no retaliatory capability (i.e., Y had a disarming first strike capability), then there would be a great incentive for Y to attack since after a Y attack X would have little choice but to capitulate in the presence of the remaining Y missiles. Thus the desire of X for security leads to the dependence of its utility function on its own retaliatory capability. On the other hand, to the extent that X desires power its utility function depends on its opponent's retaliatory capability, where an increasing retaliatory capability for Y decreases utility (but at an increasing rate) since such a capability could be used for retaliation following an initial attack by X . Finally the utility function depends on alternative uses of national wealth, where increasing alternative uses increase utility (but at a decreasing rate). To simplify, McGuire assumes that the utility function is independent and additive.

Having specified "tastes" the next step is of course to specify "technology." If the missiles are thought of as "costs" (representing a diversion of national wealth) and the retaliatory capabilities are thought of as "benefits" (each advancing the interests of one side, but retarding those of another) then the technology is summarized by cost schedules, showing how much any particular level of retaliatory capability would subtract from national wealth. Combining the "tastes" summarized by national utility functions with the "technology" summarized by missile cost schedules, McGuire obtains an optimum at the point where:

$$\frac{MU \text{ of own retaliatory capability}}{MC \text{ of own retaliatory capability}} + \frac{MU \text{ of enemy retaliatory capability}}{MC \text{ of enemy retaliatory capability}} = MU \text{ of money}$$

Now focussing attention on the interaction process inherent in the arms race, since there are two opponents with conflicting interests (only partially conflicting, i.e., nonzero sum since alternative uses of national wealth enter the utility functions), the arms race leads to a theoretical structure similar to that of duopoly. The various solutions to the duopoly problem have been likened by McGuire to various outcomes in the arms race. For example the naive (Cournot) solution corresponds to an arms race toward the intersection of reaction curves (but not necessarily an "unrelenting" arms race as McGuire states); the contract curve (Pareto) solution corresponds to an arms control agreement; and the leader-follower (Stackelberg) solution corresponds to unilateral disarmament. These analogies might be useful pedagogically in motivating duopoly theory.

The second important contribution of this book to the study of arms races is its discussion of the importance and influence of secrecy. From the viewpoint of economics this book indicates the importance of studying uncertainty in the context of duopoly, where uncertainty is perhaps most important because it can be varied, at a cost, by the actions of the rival firms, for whom secrecy, intelligence operations, and the establishment of open or closed societies are almost as pervasive and important as they are for rival nations.

McGuire treats two types of imperfect information: first, that which can be represented by a probability distribution ("risk" to an economist), for example imperfect information concerning the number, yield, accuracy, and vulnerability of missiles, and second, that which cannot be represented by a probability distribution ("uncertainty" to an economist), for example imperfect information concerning target location, especially for mobile missiles such as Polaris, and enemy intentions. McGuire shows that the effects of imperfect information are manifold, affecting cost curves (as shown via numerical simulations), utility levels, equilibrium points, and hence the rate and direction of the arms race and even the basic character of the rivalry between the two nations. An interesting result is the social value of secrecy resulting from the possibility that it might promote deterrence relative to attack and hence stabilize against the outbreak of war. Thus arms control agreements which partly destroy secrecy might increase the probability of war. McGuire gives the example of an agreement to limit numbers of missiles with no limitation on war-head technology, which would eventually allow both sides to achieve a disarming first strike capability. Another example would be general disarmament, where both sides could, by acquiring a few weapons, achieve dominance.

As has been indicated, this study has made important contributions in elaborating an approach to the study of arms races and in suggesting the importance of imperfect information. Unfortunately, however, the specific results of this approach appear to be weak or obvious. For example: "... our theory illustrates ... the fruitlessness of a search for absolutes in security" (p. 45); "... one central message of this study ... is embraced by the adage 'Know thy enemy'" (p. 46); "... not all arms control schemes merit approval" (p. 26); "... it can pay one to bribe secrets out of a rival." There are also some apparent inconsistencies: "... the theory ... indicates no clear-cut line between

quality and quantities of weapons" (p. 45) vs. ". . . The optimum allocation between quantity and quality depends on the magnitude of the missile force . . . [and] on the size and character of the opponent's forces" (p. 105). We may hope that sharper and more interesting conclusions will come from future studies that are either more specific (e.g., using specific functions instead of functional forms) or more general (e.g., allowing for more than one weapon, leading to tradeoffs between weapons and allowing for more than two countries, leading to the possibility of coalition formation).

To an economist the three interesting aspects of this book, as indicated above, are the opening up of a new field for economic reasoning, the possible use of arms races as a way of illustrating duopoly theory, and the importance of extending the theory of duopoly to allow for imperfect information and its variation as a specific policy instrument.

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NOTES

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NEW JOURNALS

The *Journal of Economic Issues*, a new quarterly co-sponsored by the Association for Evolutionary Economics and the University of Texas, will begin publication in the spring of 1967. This *Journal* will be broadly concerned with major issues of public policy, of economic methodology, and of understanding the processes and problems of economic change. To join the Association and receive its *Journal* write to Professor Ben B. Seligman, 125 Draper Hall, University of Massachusetts, Amherst, Mass. 01003. Annual dues are \$5.00. Communications about library subscriptions (\$6.00 per year, payable to the University of Texas), should be sent to Professor Forest G. Hill, Editor, *Journal of Economic Issues*, University of Texas, Austin, Texas 78712.

A new journal, *The Review of Income and Wealth*, began publication with its March, 1966 issue. The journal is published by the Yale University Press for the International Association for Research in Income and Wealth under the editorship of Mr. Odd Aukrust, Central Bureau of Statistics, Dronningens Gate 16, Oslo, Norway. Communications on subscriptions and other business matters should be addressed to the International Association for Research in Income and Wealth, Box 2020, Yale Station, New Haven, Conn. 06520, U.S.A.

The *Statistical Reporter*, issued by the Bureau of the Budget, reports monthly on current developments in federal statistics. Current developments include descriptions of statistical surveys and programs, major publications, and important organizational changes. Occasional articles appear on international, State, and local statistical activities. The *Statistical Reporter* is oriented primarily to federal personnel working on statistical programs and has been prepared for distribution within the government since 1939. Because of a widening use of and interest in federal statistics, it is now available to the general public. Subscriptions, at \$2.25 per year, are available from the Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. 20402.

Announcements

The Institute of Advanced Projects of the East-West Center in Honolulu, Hawaii, each year invites several groups of eminent scholars and specialists to spend some months at the Center in research and writing, under its Senior Specialists Program. In addition to this program the Institute sponsors the Research Translations Program, which endeavors to increase the flow of translations, particularly from Asian languages into English, in the areas of particular interest to the Center; and the International Development Fellowships Program, which supports advanced study and research in the application of the social sciences to the complex problems of contemporary economic, social, and political change in the Asian and Pacific areas. For more information about these programs, write to the Director, Institute of Advanced Projects, East-West Center, Honolulu, Hawaii 96822.

The Survey Research Center of the University of Michigan will hold its twentieth Summer Institute in Survey Research Techniques in 1967. There will be two four-week sessions, the first from June 28 to July 21 and the second from July 24 to August 18. The Institute is designed for men and women engaged in business and governmental research and other statistical work and for graduate students and university teachers interested in quantitative research in the social sciences. Detailed information may be obtained from the Survey Research Center, University of Michigan, P.O. Box 1248, Ann Arbor, Michigan 48106.

Authors of papers for possible inclusion in the Medical Care Section Program of the October 1967 annual meeting of the American Public Health Association should write to Dr. Donald C. Riedel, Yale University School of Medicine, 60 College Street, New Haven, Connecticut 06504, to obtain standard abstract forms. Two types of papers will be considered: research reports and descriptions of programs or demonstrations. The deadline for submitting abstracts is April 14, 1967. Papers will be selected by early June.

A conference on the centenary of Alexander Bryan Johnson's death will be held in Utica, N.Y., on September 8-9, 1967, under the sponsorship of Hamilton College and the Munson-Williams-Proctor Institute. Besides giving recognition to the importance of Johnson's *Treatise on Language* of 1836, attention will be paid to his close affiliations with John Adams and John Quincy Adams, his works on economic theory, banking, and on American political problems ranging from the Jacksonian era through the Civil War. Anyone having letters or documents relating to A. B. Johnson, or otherwise able to contribute to the conference, is asked to communicate with Professor Charles L. Todd, Hamilton College, Clinton, N.Y. 13323.

Professor Hugh S. Norton, department of economics, University of South Carolina, Columbia, South Carolina, has under way a book dealing with the role of the professional economist as advisor in the federal government since 1920. Professor Norton would appreciate hearing from economists who served in the early period (1920-1940), or who know others who served, with a brief statement as to the character of the service. Records of these years are sketchy, at best, and it is desirable to make the record as complete as possible.

Deaths

Eugene E. Agger, professor emeritus, Rutgers—The State University.

Wesley C. Ballaine, director, Bureau of Business and Economic Research, University of Oregon, September 25, 1966.

Vernon C. Fowke, Britnell professor of economics, University of Saskatchewan, Saskatoon.

J. Edward Hedges, April 27, 1966.

Royal E. Montgomery, professor emeritus, Cornell University, June 13, 1966.

T. Bruce Robb, October 30, 1966.

Retirements

Lester S. Kellogg, corporate economist, Deere & Company, Moline, Illinois.

Margaret Linnan, associate professor of business administration, Washington State University.

Theresa Wolfson, professor of economics, Brooklyn College, February 1967.

Charles F. Ziebarth, associate professor, School of Business Administration, University of Oregon.

Visiting Foreign Scholars

A. W. Coats, University of Nottingham: visiting professor of economics, Stanford University, autumn 1967.

P. Sargent Florence, University of Birmingham: visiting distinguished professor of economics, University of Rhode Island, spring semester.

Francisco Forte, University of Turin: visiting professor of economics, University of Virginia, spring 1967.

Herbert S. Frankel, University of Oxford: visiting professor of economics, University of Virginia, spring 1967.

Willem Grader, Nederlands Opleidings Instituut Voor Het Buitenland, The Netherlands: European exchange professor, University of Oregon, 1966-67.

Michael C. Kaser, Oxford University: visiting professor of economics, University of Michigan, 1966-67.

Alexander M. Kerr, University of Western Australia, Nedlands: visiting professor of economics, University of Texas, spring 1967.

Peter Mathias, University of Cambridge: visiting professor, University of California, Berkeley, spring and summer 1967.

Vladimir Pertot, University of Ljubljana, Yugoslavia: visiting professor of economics, Stanford University, February-April 1967.

Graham L. Reid, University of Glasgow: visiting associate professor, School of Labor and Industrial Relations, Michigan State University, January-June 1967.

Norman Sun, International Christian University, Tokyo: visiting professor of economics, Temple University, 1966-67.

Eugene Zaleski, Centre National de la Recherche Scientifique, Paris: visiting professor of economics, University of Virginia, spring 1967.

Promotions

Booth, E. J. R.: professor of economics, University of Connecticut.

H. E. Bronson: assistant professor of economics, University of Saskatchewan, Saskatoon.

Kalman J. Cohen: professor of economics and industrial administration, Graduate School of Industrial Administration, Carnegie Institute of Technology.

Davd G. Davies: professor of economics and business administration, Duke University.

Ragaei El Mallakh: professor, University of Colorado.

Glenn W. Fisher: professor, Institute of Government and Public Affairs, University of Illinois, Urbana.

M. Blanche Fitzpatrick: assistant professor of economics, Boston University.

William P. Glade: professor of business, University of Wisconsin.

Richard L. Gordon: associate professor, College of Earth and Mineral Sciences, Pennsylvania State University.

M. F. Hassan: associate professor of economics, University of Miami.

Karel Holbik: professor of economics, Boston University.

Samuel Hollander: associate professor of political economy, University of Toronto.

John J. Hughes: professor of economics, Boston University.

William Iulo: economist and professor, College of Economics and Business, Washington State University.

Paul Jonas: associate professor of economics, Brooklyn College.

Kathleen M. Langley: association professor of economics, Boston University.

John H. Lorant: assistant professor of economics, Douglass College.

Michael C. Lovell: professor of economics, Graduate School of Industrial Administration, Carnegie Institute of Technology.

Chris J. Luneski: associate professor, School of Business Administration, University of Oregon.

Sarah Montgomery. associate professor of economics, Mount Holyoke College.

Thomas H. Naylor: associate professor, department of economics and business administration, Duke University.

Charles F. Phillips, Jr.: professor of economics, Washington and Lee University.

Leonard A. Rapping: associate professor of economics, Graduate School of Industrial Administration, Carnegie Institute of Technology.

Stuart U. Rich: professor, School of Business Administration, University of Oregon.

Ronald D. Schoesler: assistant professor, College of Economics and Business, Washington State University.

Norman R. Smith: associate professor, School of Business Administration, University of Oregon.

Paul N. Taylor: professor of economics, University of Connecticut.

Arthur S. Thomas: associate professor, School of Business Administration, University of Oregon.

Jaroslav Vanek: professor of economics, Cornell University.

Ruth E. Warnke: associate professor, College of Economics and Business, Washington State University.

Andrew M. Watson: associate professor, department of political economy, University of Toronto.

Imanuel Wexler: associate professor of economics, University of Connecticut.

William P. Yohe: professor, department of economics and business administration, Duke University.

Administrative Appointments

William F. Beazer: acting chairman, department of economics, University of Virginia, 1966-67.

Charles E. Bishop, North Carolina State University: executive director, President's National Advisory Commission on Rural Poverty, Washington, D.C.

Merwyn G. Bridenstine: acting dean, College of Business Administration, University of Arkansas.

Martin Bronfenbrenner: head, department of economics, Graduate School of Industrial Administration, Carnegie Institute of Technology. On leave at the Center for Advanced Studies in the Behavioral Sciences, Stanford, California, 1966-67.

Oscar S. Dooley: acting chairman, department of economics, Temple University.

Robert W. French: acting dean, College of Business Administration, University of Illinois, Chicago.

Virginia L. Galbraith: chairman and professor, department of economics and sociology, Mount Holyoke College.

Meredith B. Givens: chief, Manpower and Human Resources Branch, Education and Planning Service, Office of Technical Cooperation and Research, AID.

Roy J. Hensley: chairman, department of economics, University of Miami.

John Kane: head, department of economics, University of Arkansas.

R. W. Kautz: acting head, department of economics and political science, University of Saskatchewan, Saskatoon.

Juanita M. Kreps: director of undergraduate studies, Duke University.

Robert A. Lynn: resident director, Whiteman Air Force Base Graduate Program in Business, University of Missouri.

Henry Ponder, Fort Valley State College: dean, Alabama A. and M. College.

Alan L. Ritter: assistant dean, School of Business Administration, Emory University.

Charles L. Shaffer: assistant to the dean, School of Business Administration, The American University.

Robert Solomon: director, Division of International Finance, Federal Reserve System.

Rolf A. Weil: president, Roosevelt University.

Clifton R. Wharton, Jr.: acting executive director, Agricultural Development Council, Inc., New York.

Appointments

Robert T. Aubey: assistant professor of business, University of Wisconsin.

G. Leland Bach, Carnegie Institute of Technology: Frank E. Buck Professor of Economics and Public Policy, Graduate School of Business, Stanford University.

David S. Ball: instructor in economics, North Carolina State University.

Lee S. Balliet, University of Texas: instructor in economics, University of Wisconsin.

Jack Baranson, Indiana University: economist, International Bank of Reconstruction and Development.

W. Scott Bauma: associate professor, School of Business Administration, University of Oregon.

R. G. Beck: assistant professor of economics, University of Saskatchewan, Saskatoon.

John Bell, University of Illinois, visiting professor of economics, California State College, Los Angeles.

John D. Bossons, Carnegie Institute of Technology: associate professor of political economy, University of Toronto.

Thomas F. Brady: lecturer in economics, Queens College, City University of New York.

Louis F. Buckley, Bureau of Employment Security, U.S. Department of Labor: associate professor of industrial relations, Loyola University.

Edward G. Cale: visiting professor of economics, University of Oregon.

Garrey E. Carruthers: research associate, department of economics, Iowa State University.

John J. Casson, Jr., Port of New York Authority: economist, The United Corporation.

Nai-Ruenn Chen, University of California, Berkeley: assistant professor of economics, Cornell University.

A. P. Clark: assistant professor of management and business law, University of Florida.

Donald P. Cole: instructor in economics, Drew University.

Eaton H. Conant: associate professor, School of Business Administration, University of Oregon.

John P. Cooke: assistant professor of economics, University of Miami.

James Crotty: lecturer in economics, State University of New York, Buffalo.

Clarence T. Cuthbert, U.S. Army (retired): instructor in economics, Temple University.

Fred Dickey: instructor in economics, State University of New York, Albany.

P. C. Dooley: assistant professor economics, University of Saskatchewan, Saskatoon.

Herbert C. Driver: research associate, department of economics, Iowa State University.

Sheila B. Eastman, York University: assistant professor of economics, University of Toronto.

Peter Eilbott: assistant professor of economics, Queens College, City University of New York.

W. Bruce Erickson, Bowling Green State University: assistant professor of management, production and transportation, University of Minnesota.

Donald R. Escarraz, University of Florida: associate professor of economics, University of Georgia.

Louis M. Falkson: assistant professor, College of Engineering, Cornell University, 1966-67.

Yuan-Sheng Fan: coordinator, Committee of Economics, Republic of China and executive director, Chinese Society for Electronic Data Processing, Taiwan.

Rawle Farley, Ministry of Planning and Development, Libya: professor of economics, State University of New York, Brockport.

Irving M. Field: assistant professor, College of Economics and Business, Washington State University.

Paul Fischer: lecturer in economics, Queens College, City University of New York.

Heywood Fleisig: assistant professor of economics, Cornell University.

Charles F. Framingham: research associate, department of economics, Iowa State University.

Raymond S. Franklin: lecturer in economics, Queens College, City University of New York.

Barry L. Friedman: assistant professor of economics, North Carolina State University.

Walter Galenson, Social Science Research Council Committee on the Economy of China: visiting professor, Industrial and Labor Relations School, Cornell University, 1966-67.

Alex Gardezky: instructor in economics, Temple University.

Jack E. Gelfand, Temple University: professor of economics, State University of New York, Albany.

Alfred Gobar: assistant professor of finance, California State College, Los Angeles.

Christopher Green: assistant professor of economics, North Carolina State University.

Abdessatar Grissa: assistant professor of economics, University of Connecticut.

Wahidul Haque, University of Alberta: associate professor of political economy, University of Toronto.

C. Obert Henderson: associate professor, College of Economics and Business, Washington State University.

John B. Henderson: international economist, Joint Economic Committee, U.S. Congress.

Roy J. Hensley: professor of economics, University of Miami.

David F. Holmes: research associate, department of economics, Iowa State University.

William Holmes: assistant professor of economics, Temple University.

George Ignatin: instructor in economics, Trinity University.

Madelyn L. Kafoglis: interim associate professor of finance, University of Florida.

Kye Chil Kim: assistant professor of economics, University of Connecticut.

Sun Kee Kim: assistant professor of economics, California State College, Los Angeles.

Sungwoo Kim: lecturer in economics, State University of New York, Buffalo.

Alan P. Kleinman: research associate, department of economics, Iowa State University.

Andrew I. Kohen: instructor in economics, University of Connecticut.

Marshall Kolin: assistant professor of economics, Rutgers—The State University.

Serge-Christophe Kolm: associate professor of economics, Stanford University, winter 1967.

Karen Koziara: assistant professor, School of Business Administration, Temple University.

Robert E. Krainer: assistant professor of business, University of Wisconsin.

Udo E. Kramer, Copley International Corporation, La Jolla, California: associate professor of international business and marketing, School of Business Administration, The American University.

Judith R. Lave: instructor in economics, Graduate School of Industrial Administration, Carnegie Institute of Technology.

K. Laycock: instructor in economics, University of Saskatchewan, Saskatoon.

Guo-I Lee: member, Planning Commission for Recovery of Mainland China, Taiwan.

Pong S. Lee: assistant professor of economics, State University of New York, Albany.

Malcolm Liggett: assistant professor of economics, California State College, Los Angeles.

Jung-Chao Liu, McGill University: economist, International Bank for Reconstruction and Development.

Louis F. Lombardi: principal economist, Division of Water Resources, Department of Conservation and Economic Development, Virginia.

Milton D. Lower: lecturer in economics, University of Maryland.

Yao-Chi Lu: research associate, department of economics, Iowa State University.

Denis I. Lucey: research associate, department of economics, Iowa State University.

James A. MacMillan: research associate, department of economics, Iowa State University.

Shah Mahmoud: instructor in economics, Rutgers—The State University.

William F. Massy: visiting professor of marketing, Graduate School of Industrial Administration, Carnegie Institute of Technology.

Franklin McCarthy: assistant professor, School of Business Administration, University of Oregon.

Warren W. Menke: research associate, Bureau of Economic and Business Research, University of Florida.

Richard U. Miller: assistant professor of business, University of Wisconsin.

Raymond Nimeh: assistant professor of economics, University of Saskatchewan, Saskatoon.

Bernard Okun, Queens College, City University of New York: associate professor of economics, Brooklyn College.

R. Rodney Pakonen: instructor, College of Economics and Business, Washington State University.

Ransford W. Palmer, Central Connecticut State College: assistant professor of economics, The Catholic University of America.

Robert Pettengill: professor of economics, State University of New York, Albany.

Joyce Pickersgill: assistant professor of economics, California State College, Fullerton.

Arnold Raphaelson, University of Maine: associate professor of economics, Temple University.

Terry E. Rogers: lecturer in economics, Queens College, City University of New York.

John Rowntree: assistant professor of economics, State University of New York, Buffalo.

Ugo Sacchetti, International Monetary Fund: adviser, economics department, International Bank for Reconstruction.

A. E. Safarian, University of Saskatchewan: professor of economics, University of Toronto.

H. Arthur Sandman: instructor in economics, North Carolina State University.

Gerald W. Sazama: assistant professor of economics, University of Connecticut.

Gerald E. Schluter: research associate, department of economics, Iowa State University.

Jugal K. Sharma: assistant professor of economics, University of Calgary, Canada.

Jerry J. Shipley: lecturer in economics, University of Maryland.

Allen Sinai: assistant professor of economics, University of Illinois.

Russell J. Singer: instructor in economics, Rutgers—The State University.

Rafael A. Sison: staff economist, Council of Governments, Metropolitan Washington, D.C.

Lawrence B. Smith: assistant professor of economics, University of Toronto.

Emil G. Spitzer: senior economic affairs officer, United Nations, New York.

William M. Sukel: instructor in business administration, Emory University.

Lloyd A. Swanson: assistant professor, College of Economics and Business, Washington State University.

William K. Tabb: assistant professor of economics, University of Connecticut.

Sheila Tschinkel: lecturer in economics, University of Connecticut.

Theodore Tsukaharo: assistant professor of economics, California State College, Los Angeles.

Bernard D. Tuchman: lecturer in economics, Queens College, City University of New York.

George M. von Furstenberg: assistant professor of economics, Cornell University.

Theodore Voyadjis: assistant professor of economics, California State College, Fullerton.

Hsin Fu Wang: appointment in economics, University of Portland.

Pearl R. Willing: staff economist, U.S. Army Corps of Engineers North Atlantic Division.

Robert J. Willis: assistant professor of economics, Wesleyan University.

John Parke Young, Division of International Finance, Department of State: visiting professor of economics, Claremont Graduate School, 1966-67.

Nail C. Yucel: instructor, College of Economics and Business, Washington State University.

Dister K. Zschock: lecturer in economics, State University of New York, Stony Brook.

Leaves for Special Appointments

David A. Baerncopf, University of Oregon: associate professor, Amos Tuck School of Business Administration, Dartmouth College, spring 1967.

C. Phillip Baumel, Iowa State University: chief-of-party, Iowa-Peru project on agrarian reform and economic development, AID, 1966-68.

Philip M. Carroll, Colorado State University: resident director, Ohio State University Graduate Center, Ellsworth Air Force Base, South Dakota, 1966-67.

Alpha C. Chiang, University of Connecticut: visiting professor, Cornell University, 1966-67.

Benjamin Cohen, Harvard University: Office of Program Coordination, AID, 1966-67.

Louis De Alessi, Duke University: appointment at Institute of Defense Analyses.

F. Trenery Dolbear, Jr., Carnegie Institute of Technology: visiting assistant professor of economics, Stanford University, 1966-67.

William T. Esterbrook, University of Toronto: principal adviser Ministry of Economic Planning, Government of Tanzania, Dar Es Salaam.

Reed R. Hansen, Washington State University: acting chief, Personal Taxation Staff, U.S. Treasury, 1966-67.

Sidney Klein, Rutgers—The State University: visiting professor of mathematics and economics, University of Hong Kong and research associate, Institute of Modern Asian Studies, University of Hong Kong, 1966-67.

Walter Krause University of Iowa: chief, survey team to Colombia, Inter-American Committee on Alliance for Progress, spring 1966.

Ta-Chung Liu, Cornell University: Jacob Ziskind visiting professor of economics, Brandeis University, 1966-67.

Arthur T. Mosher, Agricultural Development Council: appointment to department of agricultural economics, University of Minnesota, 1966-67.

David M. Nowlan, University of Toronto: economic adviser, Ministry of Economic Affairs and Development, Dar Es Salaam, Tanzania.

Erik Thorbecke, Iowa State University: head, Sector and Market Analysis Section, AID, 1966-67.

H. John Thorkelson, University of Connecticut: visiting associate professor, University of Saskatchewan, Regina, 1966-67.

Stephen G. Triantis, University of Toronto: director, Harvard project in Greece on economic planning.

J. William Uhrig, Iowa State University: director of economic research, Farmers Grain Dealers' Association, Iowa.

Pan A. Yotopoulos, University of Wisconsin: visiting associate professor of economics and acting director, Economic Research Center, University of Hawaii, 1967.

Resignations

Sonia Gold, Carnegie Institute of Technology, June 30, 1966.

Wladimir Naleszkiewicz, University of Notre Dame.

Norton C. Seeber, Carnegie Institute of Technology, June 30, 1966.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois 60201.

Vacancies

Labor economists and/or statisticians: The measurement of the economic damages suffered by those who are wrongfully injured or killed is a new area for the application of economic and statistical knowledge and research techniques. This field has been pioneered successfully and Associated Appraisers of Earning Capacity is now providing this service to attorneys on a nationwide basis. Those in it appraise the losses and are required to testify as an expert in court trials and must withstand hostile cross-examination. It is one of the highest-paid forms of work in the profession and it is possible to engage in it as a part-time activity. Requirements: (1) minimum status of associate professor; (2) experience in research in collective bargaining agreements and fringe benefit programs; (3) location in or near a major metropolitan area. Openings now available in following areas: Boston, Philadelphia, Pittsburgh, Chicago, Minneapolis, St. Louis, New Orleans, Kansas City, Oklahoma City. Waiting list open in other areas. Send résumé and list of publications to: Philip Eden, President, Associated Appraisers of Earning Capacity, 1303 Walnut Street, Berkeley, California, 94709.

Economists: With continued growth and a new M.A. program in economics the Department of Economics, California State College at Fullerton will have openings in September, 1967, for two additional staff members. Need a Ph.D. economist (rank and salary open) whose primary responsibility will be teaching economic theory at the graduate level in the M.A. program. Ideally (but not necessarily), this person should have an additional interest in teaching in the area of research, development and technological change. Also need a Ph.D. (or near-Ph.D.) labor economist (rank and salary open), with a theoretical and quantitative as well as institutional orientation. This person will be expected to assist in developing a new, interdisciplinary Institute of Industrial and Labor Relations in the College. In general, Department seeks persons with a high degree of technical economic competence, dedicated to teaching, and devoted to continuing professional activity and development. The College, 6 years old and rapidly expanding, is located on the fringe of the Los Angeles metropolitan area. For further details, write (enclosing detailed résumé): Dr. N. Townshend-Zellner, Department of Economics, California State College, 800 North State College Boulevard, Fullerton, California.

Economist: Large midwestern commercial bank is seeking economist to do research and writing on general business conditions and financial markets. A career position with unusual potential, this work involves contact with commercial banking, trust, and bond departments. Minimum requirement: master's degree or equivalent. Salary dependent upon qualifications and experience. Excellent working conditions and benefits. Please submit résumé.

Economics, business administration: Norwich University will have a vacancy and a new position in the Department of Economics and Business Administration beginning in the academic year of 1967-68. One position requires a Ph.D. or D.B.A. in economics or business, with emphasis on managerial economics, systems analysis, and industrial management. Candidate for this opening must have teaching experience, and experience with government or business will be given full consideration. The second position requires at least a master's degree and teaching experience in accounting through the intermediate level as well as principles of economics. Salaries and rank are competitive with other undergraduate colleges in New England. Write to: Dr. Charles J. Jorgensen, Chairman, Department of Economics and Business Administration, Norwich University, Northfield, Vermont, 05663.

Economics, business administration: Teaching, with excellent research opportunities. Quantitative analysis, production management, international economics, micro-economic theory, public finance, business cycles. Salary and rank dependent on qualifications. Ph.D.'s strongly preferred. Apply with résumé to: Dean Leo M. Loll, University of Alaska, College, Alaska, 99735.

Economists: Positions are available at the assistant and associate professor rank in the economics department of a rapidly expanding Canadian university. Needed are a transportation economist and a monetary theorist to teach both graduate and undergraduate courses. Salary and rank will depend on qualifications and experience. The present salary scale is \$8,325-\$11,375 for assistants and \$11,475-\$15,125 for associates. Vacancies also exist for micro and macro specialists to teach at the undergraduate level. Enrollment at the University is 4,000 and is increasing at about 20 percent per year. Appointments are from July 1, with the academic session running from mid-September to mid-April. Salary is paid on an annual basis. The University offers generous fringe benefits such as travel and research grants and sabbatical leave at 80 percent of salary after six years. (In calculating sabbatical, teaching experience elsewhere is transferable up to a maximum of two years.) Teaching load is not more than 9 hours per week. Send detailed résumé to: Dr. F. R. Anton, Head, Department of Economics, University of Calgary, Calgary, Alberta, Canada.

Economics: September, 1967, position open at assistant or associate professor rank in rapidly growing economics curriculum of a state college. Courses to be taught include principles and undergraduate electives (negotiable) as well as first-year graduate level courses (also negotiable). Requirements are Ph.D. or master's with advance credits and college teaching or other allied experience. Salary range is \$7,250-\$12,000 for nine months, depending on education and other qualifications. Summer extra. Write to: Dr. Eugene W. Schooler, West Chester State College, West Chester, Pennsylvania, 19380.

Economists: Permanent teaching positions in an expanding economics department. Salary, \$8,500-\$12,000 for academic year. Openings in several fields of specialization. Ph.D. or near Ph.D. required. For further information, write and send résumé to: Phillip A. May, Department of Economics, School of Arts and Sciences, Northern Michigan University, Marquette, Michigan, 49855.

Economics and business: Rapidly expanding new university, favorably located in the Vancouver metropolitan area, aiming at excellence in undergraduate teaching and strong graduate program, invites applications at all rank levels. Doctorate or near-doctorate required. Favorable academic conditions and advancement opportunities. Additional specialists needed in many areas, including accounting, business finance, commercial law, econometrics, economic history, economic theory, educational economics, international business, management, marketing, mathematical economics, monetary economics, operations research, production, public finance, regional economics, statistics. Write to: Dr. Parzival Copes, Head, Department of Economics and Commerce, Simon Fraser University, Burnaby 2, British Columbia, Canada.

Economics: State University College at Buffalo has two positions, beginning in September, 1967. One is for an assistant professor to teach introductory courses; salary around \$7,500. The second is for an associate professor to teach principles and advanced courses. For this position a Ph.D. is required; salary range is \$9,550-\$11,230. Please write to: Dr. Mohamed El-Beahry, Chairman, Economics—Political Science, State University College, 1300 Elmwood Avenue, Buffalo, New York, 14222. Telephone: 862-5118.

Economist: Ph.D. preferred; doctoral candidates and those with master's degree will be considered. Principles of economics, labor economics, public finance, history of economic thought, macroeconomics. Rank of instructor or assistant professor, depending on advanced degree and teaching experience. Salary range, \$7,500-\$10,000. Liberal fringe benefits. Please send résumé and names and addresses of two references to: Chairman, Department of Economics and Business Administration, Lebanon Valley College, Annville, Pennsylvania, 17003.

Economists, mathematicians, statisticians, operations analysts: Needed by the Research Analysis Corporation to participate in defense-related studies of resource allocation techniques and applications. Areas of interest include systems analysis of defense forces, weapon systems, and manpower and material resources, and its constituent elements of cost-effectiveness analysis and military cost analysis. A strong capability is maintained in computer-assisted model building. Staff members are assisted in acquiring a working knowledge of computer techniques if they do not already have it. Candidates should have at least a master's degree. For additional information, send résumé to: Mr. John G. Burke, Supervisor of Professional Staffing, Research Analysis Corporation, McLean, Virginia, 22101.

Economist: September, 1967. Principles and advanced course to be selected. Twelve-hour load. Assistant or associate. Ph.D. required. Small college of about 2,000 enrollment where emphasis is placed on teaching. Write to: Dick Smith, Head, Department of Social Science, Tarleton State College, Stephenville, Texas, 76402.

Accounting: A full-time accounting instructor is needed beginning January, 1967. Teaching load four or five three-credit courses. Salary and rank based on experience and education. Christian liberal arts college. Write to: Dr. Stewart Lee, Geneva College, Beaver Falls, Pennsylvania, 15010.

Economists: Wellington College (Arts and Science) of the University of Guelph, a rapidly expanding new college, has vacancies at all ranks for economists in most areas of specialization. Ph.D. or near Ph.D. required. Write to: Dr. J. W. Skinner, Head, Department of Economics, Wellington College, University of Guelph, Guelph, Ontario, Canada.

Economists and econometricians: We seek several outstanding men or women for the professional staff of a management consulting firm whose principal clients are blue chip industrial companies. Graduate training in economics at a leading university and several years of experience in business, consulting, or teaching required. New York City location. Ten percent travel. Salary open and depends on qualifications and previous achievements. P335

Tax economists: Office of Tax Analysis, U.S. Treasury Department, has opening for several economists in Washington, D.C. Candidates should have a doctorate or master's degree in economics with some background in public finance and orientation toward quantitative economics. Duties involve studies of the effects of proposed revisions of tax law relating to both domestic and foreign activity. Positions are within the competitive Civil Service at Grades 9 through 15, depending on qualifications, salaries from \$7,696 through \$17,550 per annum. Write to: Director, Office of Tax Analysis, U.S. Treasury Department, Room 4217 Main Treasury, Washington, D.C. 20220.

Accounting and finance: The position of Department Head, Accounting and Finance Department, will be open June 15 or September 1, 1967. Requires a Ph.D. or D.B.A. in accounting or finance, with administrative and teaching experience. C.P.A. and business experience desirable. Rapidly expanding state regional university (13,000 enrollment). Salary competitive and commensurate with qualifications. Contact: Dean Earl A. Roth, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Economist: For one- or two-year teaching assignment in economics and/or business administration at one of the most progressing international universities. All lectures are in English. Ph.D. or D.B.A. and some teaching experience are required. Salary depends on qualifications, present rank, and experiences. The position may be permanent for Iranian citizens who meet the above qualifications. Contact: Dr. Manuchehr Vessal, Provost, Pahlavi University, Shiraz, Iran.

Economist: New Zealand University requires senior economist. Strictly limited amount of teaching and supervision graduate theses. At least half time available for own research which should preferably relate directly or indirectly to the New Zealand economy or to the Pacific. Appointment for two or three years at professor or associate professor level. Family fares paid. Write: Registrar, University of Otago, Dunedin, New Zealand.

Development economist: Specialist in development and transportation planning. Assignments may be as a member of an integrated group of civil and transportation engineers, economists and cost estimators in the preparation of comprehensive national and regional transportation plans, as well as the analysis of specific transportation investments or individual responsibility for identifying and preparing national investment priorities for developing countries. Requirements for this permanent position would be a Ph.D. in either economics or general business administration, some prior experience on comparable assignments, willingness to permanently relocate outside of the United States or accept travel throughout the world, U.S. citizenship preferred. Salary commensurate with experience. Send detailed curricula vitae, including current earnings and references to: Derish M. Wolff, Director, Development Economics Group, 100 Halsted Street, East Orange, New Jersey, 07018.

Economics and business: Rapidly expanding state college, favorably located in the heart of the Gulf Coast petro-chemical complex and deep water port of major importance. Undergraduate business and economics program with M.B.A. curriculum to be added next year. Economics positions open for fall of 1967: professor and head of Department of Economics, salary range \$10,000-\$12,000; associate professor, salary range \$9,000-\$10,500. Business positions open for fall of 1967: professor, salary range \$10,000-\$12,000; associate professor, salary range \$9,000-\$10,500. Salary quotations are for nine-month academic year; a faculty member will receive one-fourth of this salary if employed during nine-week summer session. Qualifications of Ph.D. and teaching experience. Write to: Dr. Armand L. Perrault, Dean, Division of Commerce, McNeese State College, Lake Charles, Louisiana, 70601.

Economist: Resource economist in water and land use planning demand studies, projections, and project evaluation. Position immediately available. Master's degree in economics or planning with a major in economics, plus work toward a doctor's degree. A total of three years of graduate training required. Two years of experience may be substituted for graduate work beyond the master's degree. Salary range \$8,500 to \$11,050. Fringe benefits include annual increment, health benefits program with major medical coverage, vacation and sick leave provisions, retirement program with life insurance, 40-hour week. Excellent opportunity with nationally recognized river development government agency. Send résumé to: Delaware River Basin Commission, Administrative Division, P.O. Box 360, Trenton, New Jersey, 08603.

Economics and business: The University of Lethbridge (a provincial university) invites applications for two teaching appointments available for Ph.D.'s or near-Ph.D.'s in the field of economics (mathematical economics, statistics, and international economics) and business administration (accounting, management, and business finance). Rank and salary are open. Please submit complete résumé to: Mr. Carl B. Johnson, Acting President, University of Lethbridge, Lethbridge, Alberta, Canada.

Economists: With continued growth and a new Department of Economics in the School of Arts and Sciences, California State Polytechnic College, Kellogg-Voorhis, Pomona, California, will have at least two positions open in September, 1967 (rank and salary open). Immediate needs are for instructors capable of teaching econometrics, programming, and forecasting at the undergraduate level. Also expected to teach some sections of the basic principles course. Ph.D. preferred but not mandatory. Especially interested in young graduate students completing degree requirement or economists with or without degree but with experience in quantitative economic analysis. For further information write: George Galbreath, Head, Department of Economics, California State Polytechnic College, Pomona, California.

Undergraduate economics: Liberal arts college for women expects to make appointment to begin September, 1967. Introductory and advanced undergraduate courses. Ph.D. preferred; master's required. Rank and salary dependent on training and experience. Write to: C. T. Ruddick, Vice-President, Lake Erie College, Painesville, Ohio, 44077.

Teaching opportunities abroad: University positions, instructor to professor level, in the developing nations of Asia, Latin America, and primarily Africa. Two-year contracts. Salary dependent on level and location; transportation provided. Write to: Overseas Educational Service, 522 Fifth Avenue, New York, New York, 10036. An equal opportunity employer.

Economists: Positions available with TVA's Regional Studies Staff in Knoxville, Tennessee, for candidates interested in regional economic development and economic planning. Duties involved are: to assist in formulation of a regional growth simulation model in program evaluation, particularly with respect to waste management systems; to test and evaluate results; to analyze problems in corporate planning of regional development programs; to assist in evaluating research problems submitted by line divisions; to analyze program functions and suggest procedures for evaluating program contributions to regional economic development. Candidates should have at least a master's degree. Salary \$8,100 to \$13,025. Send résumé to: Chief, Employment Branch (AER), Tennessee Valley Authority, Knoxville, Tennessee, 37902.

Accountants: With continued growth and expanding academic programs, the Accounting Department, Texas A. and M. University has openings for two additional staff members. One, an outstanding tax and auditing specialist with a capability in computers as related to accounting in general and auditing in particular. Applicants should possess terminal degree and preferably be in mid-career and interested in a balanced program of teaching, research, and publications. Salary to \$16,500 for nine months with opportunity for additional summer term teaching at a comparable rate. Second, an assistant professor interested in principles and theory, cost, and computer applications in accounting. Applicant should possess terminal degree. Salary to \$10,750 for nine months with opportunity for additional summer term teaching. Applicants for both positions may send credentials to: Dr. Jack W. Coleman, Head, Accounting Department, School of Business, Texas A. and M. University, College Station, Texas, 77843.

Finance: With continued growth and expanding programs, the Finance Department has an opening for an additional staff member. This is for an assistant professor with salary to \$10,750 for nine months with opportunity for additional summer term teaching. Requirements call for a terminal degree. Appointee should be interested in specialization in the institutional aspects of finance. Applicants for this position may forward their credentials to: Dr. John E. Pearson, Director, School of Business, Texas A. and M. University, College Station, Texas, 77843.

Operations research, mathematical economics: Position open at instructor-assistant professor level in a rigorous graduate program in industrial administration. Engineering background or industrial experience desirable. Should be interested in teaching courses in decision theory, statistics, operations research, and an undergraduate course in quantitative economics. Top salaries. Write to: Alfred L. Thimm, Director, Graduate Program in Industrial Administration, Union College, Schenectady, New York, 12308.

Economics: New state university. Openings in Department of Economics, all fields. For more information, write: Howard Mahan, Chairman, Social Sciences Division, University of South Alabama, Mobile, Alabama, 36608.

Director economic-marketing research: A consumer product research and promotional organization supported by 15 Latin-American countries is seeking a person to head up six-man department of international marketing and economic research. Must have advanced degree, preferably doctorate, in economics, marketing, chemistry, and agronomy. Interest in South American and Spanish-speaking capability desirable. Location in New York City. Salary consistent with experience and qualifications plus liberal benefits. Write in confidence to: Case and Company, Inc., Management Consultants, 600 Fifth Avenue, New York, New York, 10020.

Economics: Small liberal arts college seeks Ph.D. in economics for August, 1967. Some combination in international trade, comparative economic systems, marketing, history of economic thought, or labor economics desired. Institution is state supported, new, and emphasizes opportunity for small classes and experimental approaches in instruction; rank and salary open. Address inquiries to: Roy Wood, Head, Department of Economics, Asheville-Biltmore College, Asheville, North Carolina, 28801.

Economists: Applications invited for various positions in rapidly expanding program. Salary and status dependent on qualifications and experience. Salaries up to \$19,000, pensionable, fringe benefits. Send applications to: Dr. Rawle Farley, Chairman, Department of Economics, State University of New York, Brockport, New York, 14420.

Statistics, macroeconomics, principles of economics: One of the younger professors is being given a leave of absence for the academic year 1967-68, so that he will be able to complete his doctoral dissertation. We seek a professor (Ph.D. or Ph.D. candidate) for a one-year appointment to teach the following courses: fall, 1967, two sections of principles of economics and two sections of introductory statistics; spring, 1968, two sections of principles of economics, one section of macroeconomics, and one section of intermediate statistics. While the term of employment is for one year only, Loyola College is in the process of expanding its program, and it may be possible to retain a suitable applicant on a long-term basis. Salary is open, because of the unusual circumstances. Loyola College is operated by the Jesuit Fathers. However, the religion of the applicant is not a material factor. We have a student body composed of Catholics and non-Catholics. Contact: Rev. Henry Lavin, Dean, Loyola College, 4501 North Charles Street, Baltimore, Maryland, 21210.

Economics: Several new permanent positions in eighteen-man economics department at a rapidly growing state university (16,500 students) with increasing emphasis on upperclass and graduate education. Positions can be filled at any rank and in most fields of specialization. Ph.D. in economics required for all positions. Senior ranks require teaching experience and record of publication as well as interest in working with graduate students. Opportunity to combine specialty teaching with research work under reduced class load. Nine-month salary ranges from \$9,000 to \$17,000-plus. Summer teaching and research grants commonly pay one or two months' additional salary. Send résumé and references to: Jack W. Skeels, Department of Economics, Northern Illinois University, De Kalb, Illinois, 60115.

Transportation economist or transportation specialist: U.S. Army Corps of Engineers, Civil Works Directorate, requires several transportation economics or transportation specialists to fill staff positions in division and district offices in various parts of the country. Positions are from grade level GS-5 (\$5,331) to GS-13 (\$12,873) and are a part of the federal Civil Service program. Basic requirements are a degree in economics or business administration and appropriate experience or training for performing the following duties: supervises or produces economic evaluation studies of proposed waterway or harbor improvements; structures potential area of influence of proposed improvement, determines movements of commodities in area of influence, and assesses economic efficiencies of moving these commodities by various carrier modes—water, rail, road, and pipeline. Should be familiar with cost and rate structures of these carrier modes as well as evolving technologic changes affecting their interrelationships in an overall transportation system for a region and for the entire nation. Applicants should apply to: Mr. N. A. Back, Chief, Economics and Evaluation Branch, Civil Works Directorate, Office, Chief of Engineers, Washington, D.C., 20315.

Economists and regional economists: U.S. Army Corps of Engineers, Civil Works Directorate, requires approximately fifty qualified economists to fill newly established staff positions in division and district offices in various parts of the country. Positions are from grade level GS-5 (\$5,331) to GS-13 (\$12,873) and are a part of the federal Civil Service program. Basic requirements are: a degree in economics or closely allied field and appropriate experience or training for performing the following duties: identification of the need for and the benefits of water and related land resource developments for such purposes as navigation, water supply, water quality, flood control, power, shore protection, irrigation, recreation, and fish and wildlife enhancement; comparative advantages of alternate means for meeting water resource needs; the application of economic data and procedures in the formulation of comprehensive plans for river basins and multipurpose projects; the role of water resources in regional and area development; and the allocation of costs among project purposes. Applicants should apply to: Mr. N. A. Back, Chief, Economics and Evaluations Branch, Civil Works Directorate, Office, Chief of Engineers, Washington, D.C., 20315.

Economists: Federal agency has openings for two economists, GS-7 (\$6,451), GS-9 (\$7,696), GS-11 (\$9,221), or GS-12 (\$10,927 per annum) to start. Training in statistics required. One of the openings requires additional preparation in mathematics. Candidates for higher grade positions must have experience in business economics or economic research. Submit résumé to: Dr. John T. Walter, Director, Economic Studies Division, Post Office Department, Washington, D.C., 20260.

Economics: Ph.D. with teaching experience, associate or full professor, to head department of small liberal arts college. Nine months' salary range \$7,500 to \$11,000, liberal fringe benefits. Position open September, 1967. Write to: President L. Vernon Caine, Illinois College, Jacksonville, Illinois, 62650.

Business administration: Due to an expanding undergraduate and graduate program, positions are open for faculty in the areas of accounting, finance, production management, marketing, business communications, statistics, and office education. Ph.D. required. Prefer research-oriented persons. Write to: Dean Robert L. Thistlethwaite, College of Business, Northern Illinois University, De Kalb, Illinois, 60115.

Economist: September, 1967. Principles and advanced course. Twelve-hour maximum. Assistant or associate. Rank and salary dependent upon background. Ph.D. or near Ph.D. required. Coeducational college of approximately 1,800 enrollment, located 25 miles north of Boston. Write to: Dr. Edward J. Burns, Chairman, Department of Economics, Merrimack College, North Andover, Massachusetts, 01845.

Economists: Many developing countries turn to the United Nations and its specialized agencies for help and advice in dealing with their problems. Consequently, the United Nations needs the services of highly qualified economists in almost every field of specialization for its technical assistance and pre-investment programs. These overseas assignments in the developing countries vary from a few months to several years, although most of them are for "one year with possibility of extension." Positions with the U.N. Development Program offer economists with graduate degrees and 10 or more years of practical experience an unusual opportunity to participate in the stimulating programs of economic development which characterize a large number of emerging countries. Such assignments often can be fitted into the university professor's sabbatical year; for longer assignments universities have been willing to grant leaves of absence. This program provides a unique opportunity for university economists to fill interesting and worthwhile jobs which will benefit the developing countries and contribute to their academic careers. The United Nations also seeks economists and econometricians for research and operational posts at its headquarters and Regional Economic Commissions. These posts require candidates for an M.A. or Ph.D. in economics or econometrics and several years of experience which has involved substantial economic research. Most appointments are for a minimum of two years, although a limited number are for shorter periods and can be worked into sabbatical years. U.S. citizens should send résumés to: Director, Office of International Organization Recruitment, Department of State, Washington, D.C., 20520. Non-U.S. citizens should apply directly to: Office of Personnel, United Nations, New York, New York, 10017.

Labor economists and statisticians: Attorneys throughout the U.S. are discovering that expert economists and statisticians can provide valuable assistance in ascertaining the extent of monetary losses suffered because of wrongful injury or death. General qualifications for those desiring to engage in this rewarding professional activity include university faculty status of associate professor and above, labor economic orientation, and located near a major city. Established regional economic consulting organization active in this field and the general consulting business is expanding nationally. Both ownership participation and associate opportunities available. If interested, write to: D. Carl Deiter, President, Economic and Statistical Consultants, Inc., Box 17184, Tampa, Florida, 33612.

Economists: Office of International Operations, Internal Revenue Service, has unique and intellectually challenging career opportunities for economists in Washington, D.C. Duties involve the application of economic analysis to intercorporate affairs between related corporations to determine whether they reflect market or arm's length standards. Function is to provide analysis and recommendations on the "reasonableness" of intercorporate prices, and whether or not the corporations involved have failed to clearly reflect their true taxable income. These studies cover the entire spectrum of business activity and will have an important bearing on the development of tax law guidelines and precedents involving intercorporate pricing in the expanding field of international commerce. Positions are available at entrance salaries ranging from \$10,000 to \$15,000. Position and starting salary will depend upon educational background and professional experience. For further details, write to: Director of International Operations, Attention of Chief, Economic Advisory Branch, Internal Revenue Service, Washington, D.C., 20225.

Economics and department head: Opening for Ph.D. with teaching experience in growing economics department. Teaching assignments primarily in fields of general economics, statistics and quantitative methods, labor, growth and comparative economics. Currently a two-man department. College located ideally in rural community but only one hour from Philadelphia. Retired applicants welcome. Send résumé to: D. E. Strick, Dean, Millersville State College, Millersville, Pennsylvania, 17551.

Economics, temporary position: Opening for 1967-68 school year for Ph.D. candidate. Full-time or part-time appointment available. Teaching assignments in basic economics, public finance, comparative economics. Ten miles from Lancaster; 65 miles from Philadelphia. Ideal for dissertation-writing student. Send résumé to: D. E. Strick, Dean, Millersville State College, Millersville, Pennsylvania, 17551.

Economist: Liberal arts college, Catholic auspices, urban residential location. Enrollment 1,850. Day and evening divisions. Emphasis upon teaching and personal relationship with students. Distinctive program of concentration featuring undergraduate seminars and fostering independent study. Desire a man with Ph.D. or close to a doctorate. Must be able to teach also in business administration. Labor economics interest preferred. Appointment range: instructor-associate professor. Salary competitive; depends on experience. Submit résumé to: Chairman, Department of Business and Economics, Bellarmine College, 2000 Norris Place, Louisville, Kentucky, 40205.

Economist: Michigan church-related (Methodist) four-year liberal arts college seeks Ph.D. or Ph.D. candidate for teaching of principles and intermediate economics courses. Flexible course assignment; assistant or associate professor to begin September, 1967; generous fringe benefits. P336

Economists Available for Positions

(Italics indicate fields of specialization)

Agricultural economics: Man, 26, single; M.A., working towards Ph.D. in agricultural economics. Two years of graduate and undergraduate teaching experience; more than 2 years of research experience; several research papers published. Seeks college teaching or position at research organization. Available immediately. E1513

Comparative economic systems, planning, Soviet and European economics, development, economic integration, principles: Man, 38, married; Dr. rer. pol., 1 year at Harvard University. Associate professor; 11 years of research and 6 years of teaching experience. Seeks teaching and/or research position for the academic year 1967-68. E1538

Economic education: Man, 40; A.B. in economics, M.S. in education. Ten years in teaching. Knowledge of research; writing ability. Desires work with foundation, publication, or school. Interested in methods of education regarding present economic system, or situations where information is being gathered on specific problems and material is synthesized into reports and recommendations. E1540

Macroeconomics, money and banking, business cycles, public finance: Man, 45; Ph.D., Harvard. Nine years of teaching, graduate and undergraduate; 3 years as government economist; 7 years of business experience. Modest publication record, including text in macroeconomics. At present associate professor at large university. Desires teaching position, preferably in metropolitan university in midwest or northeast. Available in June or September, 1967. E1541

International economic development, international trade, finance, marketing: Man, 42, married; lacking thesis for M.A. Bilingual; over 8 years of experience in economics and marketing research and development. Desires international trade development opportunity. E1543

Money and banking, macroeconomics, principles, public finance, land economics, economic history, comparative systems, Soviet economy: Man, 39, married, Canadian; Honors B.A. and M. A. in economics, Ph.D. thesis well under way. Fluent in German. Fifteen years of business experience (accountant) and 3 years of university teaching experience in economics. Seeks research and/or teaching position. Will consider other positions. West Coast of U.S. or Canada. Available in 1967. E1545

Operations research, econometrics, statistics, mathematical economics: Man, 48; B.S. in S.S., Ph.D. course work virtually completed. Desires research position. Willing to relocate. Available immediately. E1548

Economics, business: Man, 56, married; Ph.D. Twenty years of teaching experience, 9 as department head; business experience. Various publications. Desires teaching, research, administration, or some combination. Available immediately. E1550

Public finance and fiscal policy, monetary economics, micro- and macroeconomic theory, economic history and development: Man, 28, married; M.A., Ph.D. Three years of teaching experience as a graduate teaching assistant; 1 year as instructor at a leading state university in the U.S. Publications in European learned journals. Interest in econometrics. Currently assistant professor at a large state university. Desires teaching position with opportunity for research, preferably in a university offering a doctoral program. E1556

Marketing, management, communications, data processing, international business, government and business, principles, thought, agricultural economics, development, systems, public finance, consumer economics: Man, 36, married; M.B.A. (marketing), M.A. (economics), U.S.C. Extensive price and cost analysis work, systems and procedures, and procurement exposure in industry. Presently teaching in community college in California. Seeking professional advancement in four-year liberal arts college. Will relocate to any area. Salary and position open. E1561

Economic theory, monetary theory, international relations, business cycles and forecasting, labor economics: Woman; M.B.A. and additional work towards Ph.D. Publications. Experience in college teaching, business, and government. Seeks teaching or research and writing position. Prefers Midwest location. E1565

International economics, economic development and growth, price-distribution theory, macroeconomics: Man, early 40's, married; B.A., Washington, M.B.A., M.A., Ph.D., Chicago (1951); Phi Beta Kappa, Magna cum laude. Teaching, government, and research experience; fluent Spanish; book, articles, reviews in quality journals; knows and uses mathematics, statistics. Last position, Academic Staff, London School of Economics. Seeks quality academic position teaching and research in university with graduate program; likes students. References. Available immediately. E1567

Principles, economic theory, mathematical economics, money and banking, public finance: Man, 35; B.A., M.A., Ph.D. course work except dissertation completed in 1959. Seven years of full-time teaching experience at various state universities and colleges; 3 years as associate professor. Substantial research in empirical and theoretical studies. In *Who's Who in South and Southeast U.S.A.* Currently working as a senior research economist with a U.S. government agency. Interested in a teaching position in a metropolitan area or an area close to large libraries and research environments. Will relocate. Available in June, 1967, or earlier if desired. E1575

Business administration, management: Man, 44; Ph.D. Professor; experienced administrator and teacher; publications. Available during 1967-68. E1579

Micro- and macroeconomics, public finance, international economics, statistics: Man, 28; candidate for M.A., near completion, currently finishing thesis. Mathematics background; research assistantship in medical economics; publications. Seeks research position. E1583

United States economic development, labor economics, health, industry studies: Man, 27, married; Ph.D., Columbia University, requirements completed except defense of dissertation. Six years of diversified research experience, last 4 years with well-known nonprofit economic research organization; several independent research studies and responsible research assistance on many more; strong knowledge of statistical methods. Seeks responsible research position or teaching position with extensive opportunities for research, preferably in New York area. Available in February, 1967. E1585

Economic principles, macro and micro theory, statistics, business literature, geography, regional development, management, marketing: Man, 29, married; B.B.A., M.B.A. (economics). Experience in business, research, teaching, including chairmanship of small department. Has publications and other recognitions of abilities. Seeks teaching position in northeastern college or university (Ph.D. program within commuting distance). Available in summer or September, 1967. E1586

Econometrics, linear programming, inventory control, simulation, agricultural economics, applied and mathematical economics, micro- and macroeconomics, economic development: Man, 33, married; B.S., M.A., M.S., Ph.D. from one of the top universities of the U.S. Two years of teaching experience. Desires teaching and research position. Available in summer, 1967. E1587

Economic history, economic theory, population, money and banking: Man, single; B.S., M.B.A., Ph.D. requirements completed, dissertation considerably advanced. Ten years of teaching experience; business experience. Desires permanent position with an opportunity for research and for an advancement in a growing college. Available in September, 1967. E1588

International economics, economic development, economic history of Europe and Asia, economic theory: Man, 35, married; B.A., M.S., Ph.D. Four years of college teaching experience; currently associate professor of economics; publications. Seeks teaching position with opportunity for research. Available in June or September, 1967. E1591

Principles, theory, American economic history, history of economic thought, money and banking, business economics: Man, 39, married; Ph.D. Twelve years of teaching experience; also experience as department chairman. Desires position in teaching, research, or business. Available in September, 1967. E1594

Monetary theory and policy, statistics, economic theory, economic development, mathematical economics: Man, 27, married; all Ph.D. requirements completed except dissertation, which is in progress and should be completed by June, 1967. National Defense Fellow. Some teaching and research experience. Desires teaching position with research opportunities. Available in June, 1967. E1595

Mathematical economics, econometrics, microeconomic and macroeconomic theory, international economics, statistics, economic development: Man; Ph.D. Graduate and undergraduate teaching and research experience. Interested in teaching position with opportunity for research in an institution offering a doctoral program. E1596

Quantitative economic and business analysis, regional economics, transportation, economic development, international trade and finance, money and banking, marketing, management, statistics, accounting: Man, 39, single, U.S. citizen; Ph.D., Columbia University. Twelve years of progressive professional experience in economic research and teaching with leading institutions in N.Y.-D.C. area. Seeks a responsible teaching and/or research position. Available in February, 1967. E1598

Principles, public finance, money and banking, history of economic thought: Man, 49, married; Ph.D. Fifteen years of college and university teaching experience. Desires position stressing good teaching. E1599

Micro- and macro theory, growth and development, international economics, public finance, history of economic thought, statistics: Man, 46, married; M.A., Ph.D. Associate professor; modest publications; over 12 years of university and college teaching. Available in fall, 1967. E1600

Public finance, micro- and macroeconomics: Man, 31, married; Ph.D. expected in June, 1967. Ford Foundation Fellowship. Several years of full-time teaching experience at major eastern university. Desires college-university teaching position with the opportunity and facilities for research. Available in September, 1967. E1602

Statistics, mathematical economics, economic theory, international economics, comparative systems, economic development and planning, economic growth: Man, early 30's; M.A., M.B.A., all Ph.D. requirements except thesis completed. Six years of teaching experience. Desires teaching position in or close to New York City. E1603

Economic development, agrarian reform, Latin-American studies, agricultural labor, economic theory and thought: Man, 33, married; Ph.D., Berkeley, 1962. Employed in research and operational activities by international organization and U.N. agency since 1962; research in Latin America; teaching experience. Seeks teaching position with opportunity for research at university with interdepartmental program in Latin-American studies and/or economic development. Available in fall 1967. E1604

Business management, managerial economics, government and business, labor relations, personnel management: Man, 52, married; B.S. (business administration) with highest distinction, M.A. (economics), Phi Beta Kappa, Beta Gamma Sigma. Twenty-five years of experience in all phases of general management in heavy consumer and defense industries; extensive operating and consulting experience with U.S. government in economic stabilization and defense matters; university staff and visiting lecturer in economics, labor relations, and government-business relationships. Seeks university or other administrative or teaching position. Available on reasonable notice.

E1606

Macro- and microeconomics, international economics, money and banking, business cycles: Man, B.A., M.A., Ph.D. in 1960. Six years of full-time teaching of various university economics courses; Ford Foundation Research Fellowship; modest publication record; presently assistant professor in a state university. Desires a change in geographical location.

E1607

Labor economics and history, American economic history, comparative economic systems, consumer economics, introductory statistics: Man, 28, married; Ph.D. is anticipated in June, 1967. Four years of college teaching experience. Desires teaching position at coeducational institution having economics as a major field. Also interested in doing research on current policy issues. Available in June, 1967.

E1608

Economic development, industrial organization, economic thought, labor, macroeconomics: Man, 51, married; Ph.D. Now in top level post with leading research foundation. Twelve years of college teaching experience; 10 years of U.S. government and overseas consulting. Nine books; over forty articles and reports. Desires permanent professorship at good university, preferably one with a solid graduate program. Available in fall, 1967.

E1610

Micro- and macroeconomics, international economics, monetary theory and banking, economics of development: Man, 25, married; B.Sc.(Econ), University of London, with specialization in economics, analytical and descriptive, M.Sc.(Econ) with specialization in economic development; 2 more years of further graduate study and research at a leading American university. West Indian national migrating to Canada seeks suitable post in teaching or research. Available in fall, 1967.

E1611

Statistics, microeconomics, money and banking: Man, 26; Ph.D. course work completed. Secondary school, industrial and college experience. Desires academic position. Prefers Midwest; will consider other. Available in September, 1967.

E1612

Economic development, international economics, economic history: Man, married; Ph.D. Long world-wide experience in economic development policy and administration in headquarters and field. Interested in teaching, research, or combination with graduate student training and development of university contract and service program, preferably though not restricted to Southeast. Available in June or September, 1967.

E1613

Macro- and microeconomics, development economics, monetary economics, economic thought, international economics, public finance: Man, 34, married; Ph.D. leading eastern university. Publications; several years of teaching; government experience and research. Desires teaching position or research. Available in June or September, 1967.

E1615

Development and international economics, labor economics, national income theory and cycles, principles, Latin-American economics: Man, 28, married; M.A. in Economics, Ph.D. candidate, working on dissertation. Two and a half years of college teaching experience. If job is in N.Y.C. or vicinity, might consider part-time assignment. Seeks teaching and/or research assignment. Available in June, September, or December, 1967.

E1617

Macroeconomics, government finance, history of thought, European economic history, customs unions, international trade, money and banking: Man, 46; Ph.D. Fluent German, Italian, French; executive and planning business experience; 7 years of teaching experience; some publications; present associate at a state university. Seeks equivalent position near metropolitan center East Coast.

E1618

History of economic thought, macroeconomics, monetary theory, economic development, with emphasis on Latin America: Man, 35, married; Ph.D. (economics). Six years of teaching experience. Presently completing Rockefeller Foundation grant. Desires teaching position with research opportunities. Available in summer, 1967.

E1619

Industrial relations and personnel management, economic planning, management practice, marketing, regional development, rural economics: Man, 26, single, Indian; M.Com. in General Management. Trained in labor welfare; commercial assistant manager; some lecturing experience. Desires research and/or teaching or administrative position with college, university, and research organization; also interested in management training and working abroad for private business or international agency. E1621

Development economics, planning, economic theory, international trade, money, banking, insurance, public finance, labor economics, economic history, business administration: Man, 45, married; Ph.D. Over 40 publications; 20 years of research, business, and graduate teaching experience in Europe, U.S., Latin America, Middle East, Africa, and Asia; fluent in 8 languages; formerly U.N. Technical Assistant, at present Industrial Sector Programmer at regional development authority (SUDENE), Recife, Brazil, on behalf of the O.A.S. Seeks responsible teaching and/or research, consulting, administrative position with university, research organization, or international business in U.S. or abroad. E1622

Price theory, welfare economics, macro-theory, industrial organization, growth, international trade, quantitative methods: Man, 35, married; outstanding academic record, doctoral work completed except dissertation. Trained in a leading economics department and at several important research institutes. Extensive and successful teaching experience. E1623

Principles of economics, management, data processing, systems, taxation: Man, 46, married; B.S. in B.A., University of Missouri, M.B.A. candidate presently completing thesis. Ten years of experience in accounting, data processing, and management with major company. Desires to teach in college or university and study at the doctoral level. Understands people and goal-directed behavior. Believes creative ideas should be applied both in industry and education. Available either in the summer or fall, 1967. E1624

International finance, public finance and fiscal policy, national income accounting, economic development, economics of welfare, regional economics: Man, 38, married; M.A., additional work towards Ph.D. Fellowships; languages; 9 years of progressive research experience; publications. Seeks research position. Willing to relocate. E1625

Micro-theory, managerial economics, mathematical economics, marketing theory, international trade: Man, 27, married, Scandinavian; M.B.A., doctorate in June, 1967. Four years of university teaching experience. Seeks assignment as visiting lecturer for 1967-68 at U.S. university or college. E1626

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Mathematical economics, econometrics, statistics, interindustry analysis, macro- and microeconomics: Man, 31, married; Ph.D. Some teaching and research experience. Desires teaching or research position. Available in fall, 1967. E1630

International economics, managerial economics, government and business, comparative economic systems, principles: Man, 42, married; Ph.D. Experience as department chairman; modest publications. Available in September, 1967. E1631

Micro- and macroeconomics, history of economic thought, public finance, money and banking: Man, early 40's, married; Ph.D. Administrative, consulting, and teaching experience; presently department head and associate professor. Would like greater administrative responsibility and full professorship. Available in summer or fall, 1967. E1632

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Statistics, microeconomics, macroeconomics: Man, 28, married; B.B.A., M.A. in Economics. Three years of experience in analytical and research work, especially in the application of advanced technology to various areas of business. Also worked extensively with trade associations in the development of statistical and marketing programs. One year of teaching experience. Desires a highly responsible and challenging position as a research analyst or a research specialist with either an industrial concern or a consulting group. E1634

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Economic and business statistics, econometrics, economic theory, economic development, transportation: Man, 43, married; Ph.D. Adequate publications; 14 years of full- and part-time teaching and research experience; 2 years of full-time research abroad and 2 years of administrative experience. Seeks teaching and/or research position. Available in September, 1967. E1637

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Economic development, international economics, monetary economics, micro- and macroeconomic theory: Man, 31, Indian, single; B.Com. (Honours), M.A., Ph.D. (U.S.A.). Three years of teaching experience as a graduate assistant; 3 years as an assistant professor. Publications in leading U.S. and European learned journals. Currently an assistant professor at a state university in U.S. Seeks permanent or temporary teaching position in a Canadian university. Available in June, 1967. E1639

Macro- and micro-theory, economic history, history of economic doctrines, development and international economics, money and banking, public finance: Man, 31, married; Ph.D., 1965. Five years of college teaching experience; Romance languages; some publications. Desires teaching position allowing possibility to continue research. E1640

History of economic thought, development of industrial civilization, business and society, micro economic theory, principles, interdisciplinary social science: Man, 30; B.A., M.A. in economics, Ph.D. in general education (social science) with emphasis in economics to be completed by August, 1967. Five years of teaching experience. At present assistant professor at midwestern university. Desires teaching position. Available in September, 1967. E1641

Principles, labor economics, money and banking, history of economic thought, micro- and macroeconomics, elementary statistics: Man, 38, married; Ph.D. Seven years of teaching experience. At present associate professor at large state college. Desires teaching with opportunity to do research. Willing to relocate. Available in June or September, 1967. E1642

Principles, economic development: Man, 41, married; B.S. in chemistry, M.A. in economics, 50 semester hours beyond M.A. in economics and history of science and technology. Two years of college chemistry teaching; 10 years of experience as industrial chemist; 4 years of college economics teaching. Currently an associate professor of economics but desires any position at or near a university that would make possible completion of doctorate. David H. Burger, 34 Frawley Drive, Fulton, New York, 13069, 315-593-2486.

International economics, monetary economics: Man, 46; M.B.A., Ph.D. Professor at large eastern university; author with expertise and knowledge of languages. Seeks interesting teaching position close to research libraries or international-financial assignment. Available in September, 1967. E1643

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Principles, money and banking, international trade, industrial organization: Man, 26, married; M.A., Ph.D. course work completed in July, 1967. Publications; fluent Spanish and German. Seeks position outside U.S. or, if in U.S., at international organization. E1650

Regional economics, public finance, monetary theory and policy, micro and macro theory, economic thought, development of mature economies: Man, 27, married; all course work toward Ph.D. Teaching and research experience. Desires teaching and/or research position, prefers Washington, D.C., New York State, or Canada. Available in summer or fall, 1967. E1651

Industrial organization, economic growth, planning advanced theory: Man, 42; Ph.D. Presently professor and chairman of department of economics in Indian university, language of instruction English; with a maximum of two years leave. Fourteen years of teaching and research; number of publications in pertinent fields, including two books on industrial organization in India. Desires teaching position in United States or Canadian college or university for one or two years; purpose is the acquiring of experience abroad, not the improvement of income. Leave will start with beginning date of position. Family will not accompany applicant. Available at any time after March, 1967. E1652

Economic development, economic planning, money and banking, micro and macro theory, economic thought, international economics, comparative economic systems, principles: Man, 33, married; M.A., Ph.D. Modest publications; widely traveled. Two years of research experience in a highly responsible administrative position. Presently assistant professor in a large midwest Canadian university on a temporary assignment. Desires teaching and/or research in a progressive institution in Canada. Available in June, 1967. E1653

Public finance, money and banking, principles, history of economic thought: Man, 29, married; B.S.(accounting), M.B.A., Ph.D.(economics) expected in June, 1967. Research fellowship; several teaching fellowships; 3 years of university teaching experience as instructor in economics, undergraduate. Desires position either in teaching, administration, research, or some combination. Prefers New England but would consider other locations. Available in September, 1967. E1654

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Principles, labor, consumer comparative economics, history of economic thought: Woman, retired after 20 years of college teaching in one institution; German Dr. rer. pol., M.P.A. (U.S.). Seeks position with college in East on a year-to-year basis. Available in September, 1967. E1657

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ABBA P. LERNER

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Abba Lerner has contributed significantly to progress in more different fields of economics than any other living economist: among them, welfare economics, public finance, employment theory, inflation, international trade, wages, monopolistic competition, capital, and interest. In every case his contributions have been impressive and in several, fundamental. Modern welfare economics, for example, is heavily indebted to him; modern international trade theory owes much to his early articles. Throughout this extraordinary series of achievements, he has used the simplest and rarest of analytical tools: a mind of astonishing clarity and rigorousness. Often charged with being impractical—as, for example, when he advocated functional finance, or when he undertook to demonstrate “the *rapprochement* between socialism and capitalism”—he has lived to see many of his apparently impractical ideas come to life. He is an expositor and a teacher of rare ability, and he repeatedly demonstrates to us all the virtues in economic analysis of logic, precision, directness, and simplicity, combined with grace, style, humor, and wisdom.

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Number 3

A PERMANENT INCOME THEORY FOR INVESTMENT: SOME EMPIRICAL EXPLORATIONS

By ROBERT EISNER*

We have learned, perhaps a bit belatedly, that no meaningful, stable relation is to be expected between some ill-defined, really "transitory" income and consumption. An increase in income for one week cannot be expected to have the same effect upon consumption as an equal increase in the rate of income over a period of a month or year or a decade. The extent to which consumption is related to "income" must depend upon the length of time over which income is averaged or integrated.

At the root of the problem, however, is a more fundamental issue, of critical importance in the marriage of the economic theorist and the working econometrician. Our economic theory reminds us again and again that bygones are bygones. Current or past values of variables, no matter over how long a period they are measured, are bygones except insofar as they establish initial conditions and expectations of *future* variables. Thus, in the consumption function we may expect measured current or past income to be relevant insofar as it affects current wealth and expectations of future income. As has been stressed by Modigliani and Brumberg [7] the relation between consumption and measured income in any body of data will depend upon the relation, in that body of data, between measured incomes and the initial wealth and expected future incomes or, in Milton Friedman's terminology, the relation between measured income and "permanent income."

*The author is professor of economics, Northwestern University. An earlier version of this paper was presented to the First World Congress of the Econometric Society in Rome, September 1965, and I am deeply indebted to the economists, research workers, and computer programmers who have labored with me in its preparation. They include Marjorie Bechtel, Betty Benson, Robert M. Coen, Joel Fried, Jon Joyce, Elsie Kurasch, Albert Morris, Hugh Pitcher, Judith Pitcher, Jon Rasmussen, Jay Salkin, Kenneth Smith, and Patricia Wishart. Particular credit should be given to the McGraw-Hill Publishing Company department of economics and to Margaret K. Matulis of McGraw-Hill, who have made available the original data. The research has utilized the facilities of the Computing Center and the Econometrics Research Center of Northwestern University. It has enjoyed the important financial support of a series of grants from the National Science Foundation.

My own exploration of the varying estimates that might be expected from different structurings of data began with confirmation of a hypothesis by Friedman [5] that, if cross sections of households were subdivided into groups relatively homogeneous in permanent income, transitory components were likely to dominate the intragroup variances and bias downward the estimates of slope and elasticity of the consumption-income relation. I further observed that, when the group means were taken as observations, slopes and elasticities were both higher than those calculated from the cross sections of individual households, and consumption was indeed estimated to be an almost homogeneous (linear) function of income [1].

It is reasonable to expect the same phenomena in estimates of the investment function. Again, bygones must be bygones. This is true, after all, regardless of the variables that we consider significant for investment. Neither output (or sales), nor earnings, nor rates of interest, nor technological change should affect the current or future rates of investment except insofar as they affect initial conditions or expectations of relevant future variables. Initial conditions may of course include the existing capital stock and states of technology and finances, which have been determined by past variables. They may even include an immediately past rate of investment which can only be changed rapidly at considerable cost. But it is still the initial conditions and expected future paths of variables to which the investment function must relate. Estimates of the investment function which use past values of variables as observations will be as meaningful and stable as the relations among those past variables and the true arguments of the investment function.

A prime determinant of capital expenditures must be changes in expected future demand or the relation between expected future demand and existing capacity. A sophisticated or flexible application of the acceleration principle, by which the rate of investment demand depends upon the acceleration of the rate of the demand for output, will reflect this underlying relation between expected demand and capacity to the extent that it measures variations in past demand which are not merely bygones but which may be viewed by business decision-makers as "permanent." Thus, one could expect little relation between current investment and the current rate of change of output or the rates of change of output over a short preceding period of time. One should, however, expect to find a much more substantial relation between current capital expenditures and measures of the rates of change of demand over a considerable number of past periods.

Further, estimates of the investment function based upon the covariance of capital expenditures and measures of changes in demand

will indicate higher coefficients of demand variables where the variance and covariance relate more to "permanent" components of demand. In previous work I have confirmed this hypothesis by comparisons of estimates of the relation between investment and previous changes in sales on an intraindustry, interfirm basis with similar estimates on an inter-industry-year basis [2]. On the assumption that average changes in sales of all of the firms in an industry in a given year would prove a better proxy for future demand relevant to investment than would the sales experience of an individual firm, the industry-year regressions should, as they did, yield the higher sales-change or "accelerator" coefficients. In other work I have also noted that expectations of future sales changes as well as the ratios of actual to desired utilization of capacity, as reported by firms, related in the expected positive manner to actual capital expenditures [3].

I. Scope of the Present Paper

In the present paper I shall extend my earlier work in several directions. (1) I shall include a sufficient number of years of observations to permit critical comparisons of pooled cross section and time series data and shall present appropriate F-ratios for differences in regressions, along with standard errors of differences in coefficients for both time series and cross section decompositions of the total sums of squares. (2) I shall expand the work with explicit variables measuring expected sales changes and business views of the relation between their actual and desired utilization of capacity. (3) I shall introduce for the first time in large bodies of data of this kind variables measuring market evaluations of expected earnings (and implicitly expected demand). This last will be made possible by calculation of a variable defined as the sum of bonded indebtedness and the market value of outstanding shares of common and preferred stock, a variable which we shall denote as "the value of the firm."

It seems appropriate to describe briefly the rather remarkable collection of data on which this paper is based. Previous reports involve estimates from individual firm responses in McGraw-Hill capital expenditure surveys from 1949 through 1958, but chiefly from 1955, when responses became somewhat more numerous, supplemented by collateral financial data for each of the McGraw-Hill respondents, coded in such a manner as to preserve the confidential character of the surveys. For the work here reported upon, with intensive effort and considerable use of high speed computing facilities the body of data was brought up through the year 1962. We were therefore able to analyze eight years of capital expenditure data as well as various collateral statistics extending from 1962 back to as far as 1946. In addition, an

entirely new body of data bearing on the value of the firm has now been collected for the years 1959 through 1962.

We have information on an individual firm basis for some 800 firms,¹ a major portion of the McGraw-Hill sample, accounting for a substantial amount of the capital expenditure and general economic activity in the private sector of the U.S. economy. The data are also being used in estimates of production functions,² the role and formation of anticipations, inventory investment, and a number of related matters. On this occasion, however, we shall report only on certain preliminary findings with respect to capital expenditures.

II. Cross Sections and Time Series and Measures of Expected Future Demand

Among the variety of estimates of parameters of investment functions which have been presented, it has been noted, particularly, that estimates derived from cross sections of data have differed from those obtained from time series. (See, for example, Kuh [6].) Estimates have also differed when derived from time series aggregates, time series of industry data, or time series of data of individual firms. Production function estimates have varied as well, when derived from cross sections of individual firms, "two-digit" or "three-digit" industries, states, or nations. A common element, I submit, in all of these differences is that in each case the observed variables are in a different relation to the unobserved variables from which we start in our economic theory. In particular, with regard to the investment function, the variance in past sales changes or profits or other variables may be related to expected demand or the expected profitability of investment in quite varied ways among different types of cross sections and time series. It will be one purpose of this paper to use the extensive and multidimensional body of data at our disposal to explore these differences as they relate to certain plausible hypotheses about economic behavior. We shall hence present estimates of parameters of the investment function based upon least squares regressions of the following structurings of the same basic data:

1. Firm time series for firms with more than one year of observations, utilizing deviations of observations of each year around the mean of the firm's observations for all years. These deviations are summed for all firms.
2. Industry time series, involving deviations about the means for all

¹ Firms for which capital expenditure data of any single year are available, however, number only between 450 and 710. See Appendix A for a tabulation of observations used (in Tables 1, 2, and 3) by industry and year.

² Reported upon in [4].

years within each industry, where each observation is the mean of all observations of individual firms within the industry during the year, weighted by the number of firms. These deviations are summed for all industries.

3. Firm cross sections within industries, using deviations about the means of observations within each industry for each year, summed for all years and all industries.

4. Firm cross sections across industries, which use deviations about the overall mean for each year, summed for all years.

5. Cross sections of firm means across industries, utilizing the means of the observations of each firm with more than one year of observations, involving deviations of these firm means about the overall mean of underlying individual observations.

6. Industry cross sections, involving deviations of industry-year-mean observations about the means for all industries for each year, summed for all years.

7. Overall deviations of observations from their means, where observations differ as to firm or industry as well as to year.³

It will be our purpose not only to offer empirical evidence for caution in interpreting any particular set of estimates by noting the differences among them, but also to suggest and test the role of a permanent income hypothesis in explaining the differences. At least with regard to demand variables, one should expect that variation over time in the experience of an individual firm is likely to have the smallest relative "permanent" component. Since investment must be undertaken on the basis of expected profitability over long periods of time, firms may be expected to be cautious in altering their rates of investment in response to relatively short-term fluctuations in demand. By utilizing a distributed lag function, estimating separately and without constraint the coefficients of individual lagged variables, we may expect to pick up some longer run effect but we should still look for the role of demand variables to be obscured significantly in firm time series.

Turning to liquidity variables, a priori reasoning is somewhat less certain. If imperfections in capital markets (perhaps particularly for small firms) tie financing to current or past profits, capital expenditures may proceed at a more rapid rate when the flow of profits has been more rapid.

We might also expect a relatively reduced role for demand variables in cross sections of the firms within each industry although perhaps a greater role than in the firm time series discussed above. For here we

³ Appendix B states algebraically the precise nature of the deviations used in the various regressions.

may argue that business decision-makers are unlikely to view the difference between their own firm's experience in a given year and that of their industry as clearly indicative of differences in long-run expectations which should affect capital expenditures. The force of this argument is somewhat weakened by the fact that our "industries" are quite large industry-groups sufficiently heterogeneous in character to encompass a wide variety of experience.

Regressions of industry time series should perhaps show a stronger role for demand than those of firm time series. This role may still be restricted, however, by the probability that much of the interyear, intraindustry variance in demand over the years under study was likely to be viewed as transitory. One should clearly expect estimates suggesting a greater role for demand variables in longer time series which include great long-run variations in demand.

The role of past demand should show up most clearly in interindustry cross sections. At the firm level this may become apparent in cross sections across industries but permanent effects may be partially obscured by the "noise" or errors in variables of individual year observations. Year-to-year transitory fluctuations should, however, tend to wash out in cross sections of firm means, which would capture more of the longer-run differences among firms. And similarly, to the extent that demand has been growing more rapidly in one industry than in other industries, firms within that more rapidly growing industry are more likely to have favorable long-term demand expectations than are firms elsewhere whose growth in demand happens to have been larger than the mean of their average-growth industry. Business thinking, on what may appear to be an unsophisticated level to the scientific observer, runs frequently in terms of accustomed "shares of the market." This, however, may actually be a reflection of the statistical law of large numbers. The experience of all the firms in a given industry may be a better estimator of future prospects of an individual firm than may the single past experience of that firm itself.

We should expect that, if past demand enters into the investment function as a proxy for the ratio of expected future demand to existing capacity, the role of past sales changes will be supplemented or partially replaced by variables dealing explicitly with expected future sales changes, the utilization of existing capacity, and market evaluation of changes in expected future earnings. To examine these factors, we shall make use of responses to McGraw-Hill questions regarding expected changes in sales over four-year and one-year periods and to questions bearing on both actual and preferred rates of utilization of capacity. In the case of the latter we have constructed a variable (uc), defined as the ratio of actual to preferred rate of utilization of ca-

capacity, which may be taken as a direct measure of the pressure of current demand upon capacity. We have also utilized our financial data to construct measures of the ratios of (1) earnings to market value of the firm (r), (2) market value of the firm to net worth plus depreciation reserve plus bonded indebtedness (m), and (3) the change in market value of the firm (Δv).

III. *The Variables and Methods of Analysis and Presentation*

We shall hence report results of five sets of regressions involving individual firms—time series, cross sections within industries, cross sections across industries, cross sections of firm means, and overall⁴—and, where there are sufficient observations, three industry regressions—time series, cross sections, and overall. As in previous work, it has been deemed advisable to exclude observations containing extreme values of any of the variables. Upper and lower bounds of acceptable intervals were established on the basis of preliminary analysis of means and standard deviations. Intervals were generally set so that inclusion of at least 99 per cent of the values on each variable might be expected.

No attempt was made to utilize information from incomplete observation vectors. Hence a considerable number of observations were rejected because of missing information on only one or several variables. The "Definitions and Sources of Variables and Intervals for Accepted Values," which follows shortly below, describes precisely the variables utilized and indicates the intervals for acceptable values. Each table reports the number of observations rejected because of extreme values of at least one of the variables in the observation vector. In the time series, which are pooled regressions of deviations about the means of observations for individual firms and industries, respectively, a degree of freedom is lost for each firm or each industry included. Cross section regressions across industries were in effect pooled regressions of deviations about the means for each year; hence one degree of freedom was lost for each year of observations, and one for each industry in each year in the cross sections within industries.

Industry regressions are based on means of all of the observations included in the cross sections for each industry-year. There are 10 "industries" in all, but in a number of cases failure to direct certain questions to an industry resulted in eliminating all of its observations for one or more years. Industry-year observations were in each case weighted by the number of individual firm observations. The various regressions therefore involve several partitionings of the overall sum of

⁴ Firm overall regressions will, in some cases, relate to time series observations only and thus match the sums of squares of deviations of time series observations and observations based on cross sections of firm means.

DEFINITIONS AND SOURCES OF VARIABLES AND INTERVALS FOR ACCEPTABLE VALUES

| Variable ^a | Symbols and Definitions | Source ^b | Acceptable Interval ^c |
|---|---|---------------------|----------------------------------|
| Capital Expenditures as Ratio of Gross Fixed Assets | $i_t = \frac{I_t}{K_{57}}$ | MH/FD | [.6,0] |
| | $i_t^* = \frac{I_t}{K_{t-1}}$ | MH/FD | [.6,0] |
| Capital Expenditure Anticipations as Ratio of Gross Fixed Assets | $i_{t+1}^t = \frac{I_{t+1}^t}{K_{57}}$ | MH/FD | [.6,0] |
| Capital Expenditures as Ratio of Price Deflated Gross Fixed Assets ^a | $i_{pt}^* = \frac{I_t}{K_{p,t-1}}$ | MH/FD | [.6,0] |
| Relative Sales Change Ratio | $\Delta s_t = \frac{3(S_t - S_{t-1})}{S_{56} + S_{57} + S_{58}}$ | FD | [.7, -.6] |
| | $\Delta s_t^* = \frac{3(S_t - S_{t-1})}{S_t + S_{t-1} + S_{t-2}}$ | FD | [.7, -.6] |
| Net Profits as Ratio of Gross Fixed Assets | $p_t = \frac{P_t}{K_{57}}$ | FD | [.7, -.4] |
| | $p_t^* = \frac{P_t}{K_{t-1}}$ | FD | [.7, -.4] |
| Net Profits as Ratio of Price-Deflated Gross Fixed Assets ^a | $p_{pt} = \frac{P_t}{K_{p,t-1}}$ | FD | [.7, -.4] |
| 1953 Depreciation Charges as Ratio of 1953 Gross Fixed Assets | $d_{53} = \frac{D_{53}}{K_{53}}$ | FD | [.2,0] |
| Expected Relative Sales Change Over Next Four Years | $\Delta s_{t+4}^t = \frac{S_{t+4}^t - S_t}{S_t}$ | MH | [1, -.4] |
| Expected Relative Sales Change Over Next Year | $\Delta s_{t+1}^t = \frac{S_{t+1}^t - S_t}{S_t}$ | MH | [.7, -.6] |
| Ratio of Actual to Preferred Rate of Utilization of Capacity | $uc_t^r = \frac{UC_t}{UC_t^o}$ | MH | [1.3,.3] |

DEFINITIONS AND SOURCES OF VARIABLES AND INTERVALS FOR ACCEPTABLE VALUES

| Variable ^a | Symbols and Definitions | Source ^b | Acceptable Interval ^c |
|--|--|---------------------|----------------------------------|
| Market Value of Firm=Sum of End-of-Year Bonded Indebtedness and Market Value of Common and Preferred Stock | $V_t = H_t + E_t$ | FD | — |
| Rate of Return = (Net Profits + Depreciation Charges + Interest Payments) ÷ Market Value of Firm | $r_t = \frac{P_t + D_t + B_t}{V_t}$ | FD | [.7, -.4] |
| Ratio of Market Value of Firm to Net Worth+Depreciation Reserve +Bonded Indebtedness | $m_t = \frac{V_t}{NW_t + R_t + H_t}$ | FD | [5,.1] |
| Relative Change in Market Value of Firm | $\Delta v_t = \frac{V_t - V_{t-1}}{V_{t-1}}$ | FD | [1.5, -.75] |
| Time Trend Integer, Beginning with Zero for First Year of Dependent Variable | T | — | [7,0] |

^a All flow variables (I , I'_{t+1} , S and P) except depreciation charges (D) and rate of return (r) are price deflated. No stock variables are price deflated except gross fixed assets (K_p), where indicated by a p subscript. Deflation procedures for flow variables are described in [3], pp. 141-142. Where price deflated values of gross fixed assets were used they were calculated as the sum of the previous five years of deflated capital expenditures plus an estimate of the deflated value of gross fixed assets not acquired in these five years. Thus,

$$K_{pt} = \sum_{j=0}^4 I_{p,t-j} + \frac{K_t - \sum_{j=0}^4 I_{t-j}}{\frac{1}{M} \sum_{h=1}^M P'_{t-h-4}},$$

where P' is a price deflator, p as a subscript denotes a price deflated variable and M is the lesser of an estimate of the firm's length of life of capital

$$\left(\frac{1}{D_{63}/K_{63}} \right)$$

and the number of years remaining back to 1946. (All gross fixed assets acquired prior to 1946 were thus arbitrarily assigned the post-1945 mean price deflator calculated above.

^b MH=McGraw-Hill surveys.

FD=Financial data, generally from Moody's.

MH/FD= Numerator from McGraw-Hill and denominator from financial data.

^c $[U, L]$ = Closed interval, including upper and lower bounds.

$[U, L)$ =Interval including upper bound but not lower bound.

squares and cross products: firm time series plus cross section of firm means equal firm overall (time series observations only); firm cross section within industries plus industry cross section equal firm cross section across industries; and firm cross section within industries plus industry overall equal firm overall.

IV. *The Findings*

Let us note first in Table 1 some of the broad outlines of the results. Looking at the firm-overall regression, pooling all observations for all industries for the eight years from 1955 to 1962, we can find once more a distributed lag accelerator. The sum of the sales change coefficients of .417 suggests that an increase in sales will bring about a substantial but less than proportionate increase in capital stock if a sufficiently long period, in this case six years, is allowed for firms to recognize the increase as permanent and to formulate and carry out appropriate plans to adjust their capital stock.

In the present work we have introduced current and lagged profits and met the resulting problem of multicollinearity by calculating the standard error of the sum of the two profits coefficients. The sum of only .053 in the firm-overall regression suggests that little more than 5 per cent of extra profit results in capital expenditures, other things remaining the same. With all of 3,147 observations, standard errors are small and each of the sales change coefficients as well as the sum of the profits coefficients clearly differs significantly from zero in a statistical sense. The curiously negative coefficient for current profits defies immediate explanation. Perhaps higher current capital expenditures cause higher current depreciation charges and higher current interest payments, and also entail "start-up" or other costs, all reducing current net profits.

It may also be noted that the ratio of 1953 depreciation to 1953 fixed assets, a measure of (the reciprocal of) length of life of capital and hence of replacement requirements, has a substantial, positive coefficient. One might infer that "replacement investment," as a ratio of 1957 gross fixed assets, is equal on the average to the constant term of .023, plus .053 times the mean of profits, .097, plus .763 times the mean of depreciation, .053. This comes to .069, which is 81 per cent of the mean capital expenditure ratio of .085.

So far, we are largely confirming earlier findings with a lesser body of data. Let us now note, also in Table 1, the contrast between results of the cross section of firms across industries and the cross section of firms within industries. By our hypothesis that firms may view differences between their own sales experience and that of other firms within their industry as less "permanent" than differences between

TABLE 1—CAPITAL EXPENDITURES, 1955-62, AS A FUNCTION OF SALES CHANGES, PROFITS AND DEPRECIATION: FIRM AND INDUSTRY CROSS SECTION AND OVERALL
REGRESSIONS AND INDUSTRY TIME SERIES

$$i_t = b_{10} + \sum_{j=1}^7 b_{1j} \Delta s_{t+1-j} + \sum_{j=8}^9 b_{1j} p_{t+8-j} + b_{1,11} d_{53} + u_t$$

| Variable or Statistic (1) | Regression Coefficients and Standard Errors | | | | | | | Means and Standard Deviations ^a (9) |
|--|---|-----------------------------|--------------------------------------|----------------------------|-----------------------|-------------------------|-----------------|---|
| | Firm Cross Sections | | | Firm Overall (5) | Industry | | | |
| | Within Industries (2) | Across Industries (3) | Means Across Industries (4) | | Time Series (6) | Cross Section (7) | Overall (8) | |
| Constant term or i_t | | | | .023 (.002) | | | .017 (.007) | .085 (.069) |
| Δs_t | .088 (.010) | .086 (.010) | .104 (.050) | .085 (.010) | .012 (.050) | -.011 (.089) | .011 (.067) | .046 (.124) |
| Δs_{t-1} | .072 (.009) | .088 (.009) | .162 (.053) | .097 (.009) | .174 (.052) | .245 (.070) | .263 (.055) | .034 (.125) |
| Δs_{t-2} | .062 (.009) | .074 (.009) | -.033 (.057) | .068 (.009) | .084 (.046) | .151 (.061) | .120 (.044) | .033 (.128) |
| Δs_{t-3} | .055 (.009) | .063 (.009) | .137 (.050) | .059 (.009) | .071 (.046) | .093 (.059) | .085 (.042) | .036 (.124) |
| Δs_{t-4} | .041 (.010) | .050 (.010) | .059 (.052) | .054 (.009) | .092 (.042) | .085 (.062) | .097 (.045) | .034 (.120) |
| Δs_{t-5} | .036 (.010) | .037 (.010) | -.043 (.058) | .033 (.009) | .010 (.044) | -.011 (.062) | -.041 (.051) | .048 (.118) |
| Δs_{t-6} | .007 (.010) | .012 (.010) | .065 (.052) | .020 (.009) | .034 (.036) | -.018 (.062) | .009 (.045) | .039 (.122) |
| p_t | -.083 (.027) | -.085 (.028) | -.082 (.125) | -.094 (.028) | .298 (.230) | .309 (.300) | .250 (.263) | .097 (.095) |
| p_{t-1} | .166 (.028) | .136 (.029) | .116 (.131) | .146 (.029) | -.016 (.202) | -.508 (.312) | -.440 (.273) | .095 (.091) |
| d_{53} | .595 (.048) | .763 (.041) | .771 (.068) | .763 (.041) | — | 1.267 (.176) | 1.256 (.168) | .053 (.028) |
| $\Sigma \Delta s$ coefficients | .361 (.028) | .410 (.027) | .450 (.052) | .417 (.027) | .477 (.195) | .533 (.137) | .544 (.130) | |
| Σp coefficients | .084 (.013) | .051 (.013) | .034 (.022) | .053 (.013) | .281 (.220) | -.199 (.075) | -.190 (.072) | |
| n (-184) ^b | 3,147 | 3,147 | 523 | 3,147 | 80 | 80 | 80 | |
| r.d.f. ^c | 3,057 | 3,129 | 512 | 3,136 | 61 | 62 | 69 | |
| \hat{R}^2 ^d | .162 | .244 | .410 | .248 | .240 | .629 | .643 | |

^a From firm overall, and similarly in subsequent tables.

^b n = number of observations; the figure following in parentheses is the number of individual firm observations eliminated because of extreme values for one of the variables.

^c r.d.f. = residual degrees of freedom.

^d \hat{R}^2 = adjusted or unbiased coefficient of determination.

their own sales experience (or that of their industry) and the experience of all other firms, we should expect the sum of the coefficients of sales changes to be less in the case of the within-industry regression. And so it is: .361, as against .410 in the firm cross section across industries. Correspondingly, the sum of coefficients of sales changes in the industry cross section equals .533, consistent with our hypothesis that differences between industries would reflect in larger part differences in the permanent component of changes in demand. The within-industry profits coefficients are somewhat higher than those across industries, suggesting perhaps that firms do tend to invest their transitory increases in profits the year after they are received. Corresponding to these somewhat higher coefficients of lagged profits in the

TABLE 2—CAPITAL EXPENDITURES, 1955-62, AS A FUNCTION OF SALES CHANGES AND PROFITS: FIRM AND INDUSTRY CROSS SECTIONS AND FIRM TIME SERIES AND OVERALL REGRESSIONS

$$\hat{z}_t = b_{20} + \sum_{j=1}^7 b_{2j} \Delta s_{t+1-j} + \sum_{j=8}^9 b_{2j} p_{t+8-j} + u_t$$

| Variable or Statistic | Regression Coefficients and Standard Errors | | | | | | Differences in Coefficients and Standard Errors | |
|---------------------------------|---|--|-----------------------------|--------------------------------------|------------------------|--------------------------------------|--|---|
| | Firm Time Series | Cross Section of Firm Means ^a | Firm Overall ^{a,b} | Firm Cross Section Within Industries | Industry Cross Section | Firm Cross Section Across Industries | Cross Section of Firm Means - Firm Time Series [(3)-(2)] | Industry Cross Section - Firm Cross Section Within Industries [(6)-(5)] |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Δs_t | .060 (.010) | .154 (.056) | .098 (.010) | .094 (.010) | .082 (.119) | .100 (.011) | .093 (.057) | -.012 (.120) |
| Δs_{t-1} | .058 (.010) | .134 (.059) | .104 (.010) | .076 (.010) | .261 (.094) | .095 (.010) | .076 (.060) | .185 (.095) |
| Δs_{t-2} | .038 (.010) | .005 (.063) | .086 (.009) | .070 (.010) | .227 (.081) | .090 (.010) | -.033 (.064) | .157 (.081) |
| Δs_{t-3} | .024 (.010) | .180 (.056) | .081 (.009) | .065 (.010) | .169 (.078) | .084 (.010) | .156 (.057) | .104 (.079) |
| Δs_{t-4} | .030 (.010) | .036 (.058) | .071 (.010) | .050 (.010) | .136 (.083) | .067 (.010) | .006 (.059) | .087 (.084) |
| Δs_{t-5} | .021 (.010) | .042 (.064) | .058 (.010) | .049 (.010) | .054 (.082) | .059 (.010) | .020 (.065) | .004 (.083) |
| Δs_{t-6} | .011 (.009) | .079 (.058) | .039 (.010) | .018 (.010) | .020 (.082) | .029 (.010) | .067 (.059) | .003 (.083) |
| p_t | -.022 (.031) | -.100 (.139) | -.070 (.030) | -.068 (.028) | .230 (.403) | -.060 (.030) | -.078 (.143) | .298 (.404) |
| p_{t-1} | .202 (.028) | .211 (.146) | .187 (.030) | .181 (.029) | -.117 (.414) | .177 (.031) | .010 (.149) | -.298 (.415) |
| $\Sigma \Delta s$ co-efficients | .244 (.045) | .629 (.056) | .537 (.027) | .422 (.028) | .950 (.168) | .524 (.027) | .385 (.071) | .527 (.170) |
| $\Sigma \Delta p$ co-efficients | .179 (.034) | .111 (.024) | .117 (.013) | .114 (.013) | .113 (.083) | .117 (.013) | -.068 (.041) | .000 (.084) |
| n (-184) | 3,125 | 523 | 3,125 | 3,147 | 80 | 3,147 | | |
| r.d.f. | 2,593 | 513 | 3,115 | 3,058 | 63 | 3,130 | $F(9, 3,115)$ | $F(9, 3,130)$ |
| \hat{R}^2 | .055 | .264 | .163 | .120 | .329 | .159 | = 5.44 ^c | = 9.36 ^c |

^a Time series observations only.^b Constant term = .0531, with standard error of .0018.^c The corresponding 1 per cent point for the distribution of F is approximately 2.42.

regression for individual firms within industries, however, there is to be noted a sum of profits coefficients which is significantly negative in the industry cross section. Indeed the difference between the sums of profits coefficients in these latter regressions is .283, with a standard error of .077, quite significantly different from zero.

Turning now to the firm time series results in Table 2, we note first that the accelerator effect is less marked. While sales change coefficients are positive for each lagged sales change and quite significantly so for all except the last, the sum of the coefficients of .244 is decidedly less than the corresponding sum of .629 in the cross section of firm means shown in Table 2. The F -ratio of 5.44 for the reduction of residual variance confirms the heterogeneity of the regressions; the difference of .385 in the sums of coefficients, with a standard error of .071, is clearly statistically significant. This result is consistent with our hypothesis that firms would view variations in their own sales experience over a relatively short period of time (the eight years from

1955 to 1962, in this regression) as in lesser part permanent than the differences between their own average sales experience and the average experience of all other firms in the economy. To the extent that variation over time of their own sales experience is part of the variation over time of the experience of the industry as a whole, it may be viewed in larger part as permanent. Confirmation of this is suggested (in Table 1) by the sum of .477 for the coefficients of sales changes in the industry time series, larger than that in the firm time series (Table 2) although still smaller than that of the cross section of firm means.

The depreciation variable, as we have defined it, is a constant for each firm, and virtually a constant for each industry (differing from year to year in a given industry only with differences in the identities of firms in the industry sample); hence it was excluded from the time series regressions. As a consequence, higher profits coefficients in the time series relations may have reflected some of the role of replacement investment which is postponed until a year when profits are relatively higher. It may be added that introduction of a time trend variable, in those regressions where time varied, produced no appreciable change in either the regression coefficients or the constant term. The coefficient of the time trend variable was itself virtually zero.

One may wonder more about the role of our depreciation variable. It is instructive therefore to examine further Table 2, in which the depreciation variable has been deleted from the cross section regressions. We then note that the coefficients of sales changes and profits are somewhat higher. A possible explanation is that the 1953 ratio of depreciation to fixed assets was higher in more rapidly growing firms; perhaps, depreciation ratios were higher for firms which had invested heavily in the years immediately leading up to 1953 and firms investing heavily in those years were firms which continued to grow and invest heavily in future years. In any event, with the exclusion of the depreciation variable the sum of .950 for the coefficients of the sales changes in the industry cross section is comfortably close to the value of unity hypothesized by the writer in earlier work as appropriate under a certain set of assumptions (which include among others, sales changes dominated by permanent components, a production function homogeneous of the first degree, and the lack of effective ceilings or floors to investment); the F -ratio of 9.35 for the differences of regressions and the standard error of .170 for the excess of .527 over the within-industry sum of sales change coefficients of .422 clearly suggest statistical significance in the decomposition of cross section deviations.

Table 3 indicates that capital expenditure anticipations, in the main, are explained by the same factors as capital expenditures themselves. A perhaps conspicuous difference is to be found in the firm time series,

TABLE 3—CAPITAL EXPENDITURE ANTICIPATIONS, 1955-62, AS A FUNCTION OF SALES CHANGES, PROFITS AND DEPRECIATION:
FIRM AND INDUSTRY TIME SERIES, CROSS SECTION AND OVERALL REGRESSIONS

$$i_{t+1}^i = b_{30} + \sum_{j=1}^7 b_{3j} \Delta s_{t+1-j} + \sum_{j=8}^9 b_{3j} p_{t+8-j} + b_{3,10} d_{t3} + u_3$$

| Variable or Statistic (1) | Regression Coefficients and Standard Errors | | | | | | | Differences in Coefficients and Standard Errors | | |
|---|---|--|-------------------------------------|--|-------------------------------------|--|-----------------------------------|--|---|--|
| | Firm Time Series (2) | Cross Section of Firm Means ^a (3) | Firm Overall ^a (4) | Firm Cross Section Within Industries (5) | Industry Cross Section (6) | Firm Cross Section Across Industries (7) | Industry Time Series (8) | Industry Overall (9) | Cross Section Means—Firm Time Series [(3)-(2)] (10) | Industry Cross Section—Firm Cross Section Within Industries (as deleted) (11) |
| Constant term or $\frac{t}{t+1}$ | | | | | | | | | | |
| Δs_t | .046 (.010) | — (.065) | .057 (.002) .109 (.011) | .088 (.011) | .101 (.098) | .094 (.011) | .047 (.058) | .013 (.008) .056 (.075) | .186 (.065) | .103 (.129) |
| ... | | | | | | | | | | |
| \hat{p}_t | .271 (.032) | .008 (.161) | .158 (.032) | .132 (.030) | .375 (.330) | .126 (.031) | .542 (.268) | .383 (.293) | -.263 (.164) | .142 (.436) |
| \hat{p}_{t-1} | -.010 (.029) | .112 (.169) | -.033 (.034) | -.052 (.031) | -.555 (.344) | -.077 (.032) | -.098 (.236) | -.562 (.305) | .122 (.171) | -.109 (.447) |
| d_{ts} | — | — | — | .707 (.032) | 1.331 (.193) | .881 (.044) | — | 1.347 (.188) | — | — |
| $\Sigma \Delta s$ coefficients t to $t-6$ | .090 (.046) | .720 (.064) | .565 (.030) | .368 (.031) | .642 (.151) | .433 (.029) | .513 (.228) | .599 (.145) | .630 (.079) | .638 (.183) |
| $\Sigma \hat{p}$ coefficients | .261 (.034) | .120 (.028) | .125 (.013) | .080 (.014) | -.179 (.083) | .049 (.014) | .444 (.257) | -.180 (.080) | -.141 (.044) | .033 (.091) |
| n (-184) | 3,125 | 523 | 3,125 | 3,147 | 80 | 3,147 | 80 | 80 | | |
| r.d.f. | 2,593 | 513 | 3,115 | 3,057 | 62 | 3,129 | 61 | 69 | $F(9, 3,115)$ =9.89 | $F(9, 3,130)$ =9.49 |
| \bar{R}^2 | .063 | .255 | .162 | .165 | .630 | .254 | .208 | .626 | | |

^a Time series observations only.

where the sum of the coefficients of sales changes is only .090, as against .244 in the regressions of actual capital expenditures, while the sum of profits coefficients equals .261 for anticipations as against .179 for actual capital expenditures. May this not be interpreted as a partial confirmation of the existence of a business myth regarding the relative roles of profits and demand? The relation between profits and what businesses *say* they will invest the following year is a better one than that between profits and what they actually do invest.⁵ Their actual investment, even merely the timing of it, is influenced more by sales changes, which are apparently almost ignored in statements of capital expenditure anticipations.

The anticipations relation shown in Table 3 also brings into sharper focus a difference in the roles of profits in the time series and cross section which might have been noticed less distinctly in Table 2 with regard to actual capital expenditures. For the sum of profits coefficients of .261 in the firm time series exceeds the sum of .120 in the cross section of means by the amount of .141, more than three times the standard error of .044 for that difference. The greater covariance of anticipated capital expenditures and profits over time than between firms suggests that profits may have a greater role in the timing of capital expenditures than in determining their total amount.

Table 4, dealing with only the years 1960 through 1962, differs from the earlier tables in the addition of variables measuring the rate of return as a ratio of the market value of the firm (r) and the ratio of the market value of the firm to the sum of net worth, depreciation reserves, and bonded indebtedness (m). Both of these additional variables were introduced in current and lagged form, with sums and standard errors of sums of coefficients of the current and lagged variables again presented to get around problems of multicollinearity. Results, however, are somewhat disappointing. Thus, it might have been expected that the value-of-the-firm ratio (m) would be positively associated with capital expenditures, but this is not confirmed in the coefficients for any of the individual firm regressions and the number of observations was insufficient in the industry regressions to warrant their presentation.⁶

⁵ The difference cannot be explained by differences in the relative lags from *current* actual capital expenditures and anticipations relating to *next year*. For with comparable deletions of sales change and profits variables, the coefficients of similarly lagged profits are, for expenditures and anticipations, respectively, .189 and .272, with a standard error of the difference just under .04.

⁶ The value-of-the-firm ratio might appear conceptually better if the depreciation reserve were netted out of the denominator, which denotes essentially the *accounting* value of the firm. It may be doubted, however, whether this change would significantly affect the results, particularly in view of the dubious quality of the relation between accounting and economic depreciation.

TABLE 4—CAPITAL EXPENDITURES, 1960-62, AS A FUNCTION OF SALES CHANGES, PROFITS, RATE OF RETURN, VALUE OF THE FIRM RATIO, DEPRECIATION AND TREND:
FIRM TIME SERIES, CROSS SECTION AND OVERALL REGRESSIONS^a

$$i_t = b_{40} + \sum_{j=1}^7 b_{4j} \Delta s_{t+1-j} + \sum_{j=8}^9 b_{4j} p_{t+8-j} + \sum_{j=10}^{11} b_{4j} r_{t+10-j} + \sum_{j=12}^{13} b_{4j} m_{t+12-j} \\ + b_{4,14} d_{63} + b_{4,15} T + u_4$$

| Variable or Statistic (1) | Regression Coefficients and Standard Errors | | | | Means and Standard Deviations (6) |
|--|---|---------------------------------|---------------------------------|----------------------------|--|
| | Firm Time Series (2) | Firm Cross Section | | Firm Overall (5) | |
| | | Within Industries (3) | Across Industries (4) | | |
| Constant term or i_t | | | | -.003 (.011) | .082 (.070) |
| $\Sigma \Delta s$ coefficients | .140 (.162) | .395 (.058) | .431 (.057) | .431 (.057) | |
| p_t | .219 (.082) | -.032 (.074) | -.052 (.074) | -.053 (.074) | .081 (.085) |
| p_{t-1} | .270 (.091) | .102 (.074) | .058 (.074) | .059 (.073) | .082 (.085) |
| r_t | -.281 (.117) | -.007 (.096) | -.011 (.096) | -.009 (.094) | .108 (.046) |
| r_{t-1} | -.118 (.114) | .015 (.091) | .087 (.092) | .084 (.090) | .104 (.047) |
| m_t | -.015 (.012) | -.002 (.009) | .003 (.009) | .003 (.009) | .938 (.597) |
| m_{t-1} | .013 (.010) | .008 (.008) | .006 (.008) | .006 (.008) | .990 (.607) |
| d_{53} | — | .785 (.100) | .989 (.083) | .988 (.083) | .055 (.030) |
| T | -.003 (.003) | — | — | -.002 (.003) | .958 (.820) |
| Σp coefficients | .488 (.123) | .070 (.038) | .006 (.036) | .006 (.036) | |
| Σr coefficients | -.399 (.166) | .008 (.070) | .075 (.068) | .076 (.068) | |
| Σm coefficients | -.002 (.017) | .006 (.006) | .009 (.006) | .009 (.006) | |
| n (-52) | 606 | 669 | 669 | 669 | |
| r.d.f. | 373 | 625 | 651 | 653 | |
| \hat{R}^2 | .055 | .235 | .356 | .355 | |

^a F -ratio for differences of regressions of firm time series and cross sections of firm means was 1.52 (14, 591), not significant at the .05 probability level, and hence no differences of coefficients are presented.

With regard to the variables measuring rate of return (r), it was thought that in a regression already including profits, their coefficients would prove negative. For when profit expectations are higher than

current profits, the value of the firm would be relatively higher, and the current rate of return lower, while with generally high profit expectations, the marginal efficiency of investment would probably be greater and capital expenditures higher. Some support for this chain of reasoning may be noted in the time series results, where the sum of the coefficients of rates of return is indeed negative and significantly so.

We may recall that the variable measuring rates of return includes depreciation charges in the numerator and that time series variation in depreciation involves, at least in part, changes stemming from application of the accelerated depreciation provisions of the Internal Revenue Act of 1954. The negative time series coefficients of "rates of return" would thus appear also to imply a contradiction of the sometimes asserted argument that higher depreciation charges, *per se*, bring about higher rates of investment. On the other hand, however, the rate of return variable, which includes interest payments in the numerator, may be taken as a measure, although imperfect, of the cost of capital. It may then be argued that its negative coefficient reflects the expected negative relation between capital expenditures and the cost of capital.

Tables 5 and 6 are comparable to Tables 1 and 2, except that capital expenditures and profits are now deflated by the previous year's capital stock rather than capital stock of 1957, and sales changes are deflated by a three-year moving average of lagged sales rather than the average sales centered on 1957. The formulation underlying the earlier tables has certain advantages in easy interpretation and comparison of coefficients of successively lagged terms. Some deflator is necessary to eliminate heteroscedasticity associated with heterogeneity in size of firm. When all sales changes are deflated by average sales of a given period, equal values of sales-change variables of different years reflect equal changes in physical volume of sales. Coefficients relating to different lags are hence directly comparable. Deflating capital expenditures by capital stock of the period corresponding to that of the sales average then permits direct inference about a sum of coefficients of successive sales changes.

Since using this technique of deflation in previous work, however, I have felt some qualms. These concern the possibility that, despite continuing efforts to eliminate from our sample firms that have merged or made major acquisitions in the period under analysis, this use of a deflator of some given past year creates increasing havoc. For if included firms are growing by acquisitions or mergers, both capital expenditures and (generally rising) sales would in later years appear to be higher ratios of 1957 capital stock and sales, respectively. Thus for firms growing by the merger or acquisition route the variables measuring capital expenditures, sales changes (and profits) would all be

TABLE 5—CAPITAL EXPENDITURES, 1955–1962, AS A FUNCTION OF SALES CHANGES, PROFITS, AND DEPRECIATION, ALL EXCEPT DEPRECIATION MEASURED AS RATIOS OF PREVIOUS YEAR'S GROSS FIXED ASSETS OR PREVIOUS THREE YEAR SALES AVERAGE: FIRM AND INDUSTRY TIME SERIES, CROSS SECTION AND OVERALL REGRESSIONS

$$i_t^* = b_{s0} + \sum_{j=1}^7 b_{sj} \Delta s_{t+1-j}^* + \sum_{j=8}^9 b_{sj} p_{t+8-j}^* + b_{s,10} d_{63} + u_s$$

| Variable or Statistic (1) | Regression Coefficients and Standard Errors | | | | | | | Means and Standard Deviations (9) |
|--|---|-----------------------------|-----------------------------|------------------------|-----------------------------------|-------------------------------------|----------------------------|---|
| | Firm Time Series (2) | Firm Cross Section | | Firm Overall (5) | Industry Time Series (6) | Industry Cross Section (7) | Industry Overall (8) | |
| | | Within Industries (3) | Across Industries (4) | | | | | |
| Constant term or i_t^* | | | | .020 (.002) | | | .010 (.008) | .082 (.068) |
| Δs_t^* | .048 (.010) | .079 (.010) | .076 (.011) | .077 (.011) | .046 (.053) | -.037 (.092) | .016 (.081) | .048 (.119) |
| Δs_{t-1}^* | .045 (.010) | .059 (.010) | .076 (.010) | .088 (.010) | .193 (.054) | .168 (.068) | .227 (.064) | .034 (.128) |
| Δs_{t-2}^* | .033 (.010) | .037 (.010) | .051 (.010) | .057 (.009) | .129 (.044) | .114 (.062) | .145 (.050) | .037 (.031) |
| Δs_{t-3}^* | .043 (.010) | .048 (.009) | .061 (.009) | .059 (.009) | .102 (.041) | .117 (.056) | .127 (.044) | .045 (.131) |
| Δs_{t-4}^* | .055 (.009) | .038 (.009) | .052 (.009) | .070 (.009) | .127 (.040) | .127 (.059) | .176 (.046) | .049 (.133) |
| Δs_{t-5}^* | .028 (.009) | .018 (.009) | .021 (.009) | .028 (.009) | .024 (.045) | -.010 (.060) | .032 (.055) | .064 (.133) |
| Δs_{t-6}^* | .024 (.008) | .007 (.009) | .015 (.009) | .018 (.008) | .034 (.035) | .031 (.057) | .031 (.047) | .059 (.143) |
| p_t^* | .269 (.029) | -.002 (.028) | -.011 (.029) | -.006 (.029) | .302 (.189) | .034 (.289) | .241 (.289) | .091 (.095) |
| p_{t-1}^* | .294 (.027) | .121 (.027) | .098 (.028) | .117 (.028) | .189 (.194) | -.095 (.277) | -.155 (.278) | .096 (.099) |
| d_{63} | — | .583 (.052) | .713 (.043) | .644 (.044) | — | .976 (.176) | .619 (.179) | .052 (.028) |
| $\Sigma \Delta s^*$ coefficients | .276 (.042) | .287 (.025) | .351 (.023) | .397 (.024) | .656 (.165) | .510 (.121) | .754 (.121) | |
| Σp^* coefficients | .563 (.028) | .120 (.013) | .086 (.013) | .111 (.013) | .491 (.095) | -.061 (.070) | .086 (.070) | |
| n (-155) | 2443 | 2494 | 2494 | 2494 | 80 | 80 | 80 | |
| r.d.f. | 1994 | 2404 | 2476 | 2483 | 61 | 62 | 69 | |
| \bar{R}^2 | .253 | .187 | .282 | .294 | .648 | .606 | .587 | |

higher than for firms not growing in this fashion, merely because of our out-of-date deflators. This would suggest that my earlier findings in support of the accelerator relation might be at least in part spurious.

To test this distributing possibility, variables were redefined as indicated in the regressions reported upon in Tables 5 and 6, as well as subsequent tables. I am indeed happy to report that the results are essentially undisturbed.⁷

One marked departure which we may note in Table 5 is the decidedly positive coefficient of the current profit variable in the firm time se-

⁷ The somewhat smaller sample in Tables 5 and 6 is caused chiefly by the loss of observations for which sales information was available for 1956, 1957, 1958 but not always for other years. In particular, lack of earlier sales information would eliminate observations because of the absence of lagged sales for the denominator of the last lagged sales change variable.

TABLE 6—CAPITAL EXPENDITURES, 1955-62, AS A FUNCTION OF SALES CHANGES AND PROFITS, MEASURED AS RATIOS OF PREVIOUS YEAR'S GROSS FIXED ASSETS OR PREVIOUS THREE-YEAR SALES AVERAGE: FIRM AND INDUSTRY CROSS SECTIONS AND FIRM TIME SERIES AND OVERALL REGRESSIONS

$$i_t^* = b_{60} + \sum_{j=1}^7 b_{6j} \Delta s_{t+1-j}^* + \sum_{j=8}^9 b_{6j} p_{t+8-j}^* + u_6$$

| Variable or Statistic | Regression Coefficients and Standard Errors | | | | | | Differences in Coefficients and Standard Errors | |
|----------------------------------|---|--|-----------------------------|--------------------------------------|------------------------|--------------------------------------|--|---|
| | Firm Time Series | Cross Section of Firm Means ^a | Firm Overall ^{a,b} | Firm Cross Section Within Industries | Industry Cross Section | Firm Cross Section Across Industries | Cross Section of Firm Means—Firm Time Series [(3)-(2)] | Industry Cross Section—Firm Cross Section Within Industries [(6)-(5)] |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\Sigma \Delta s^*$ coefficients | .276 (.042) | .509 (.045) | .487 (.024) | .342 (.025) | .807 (.131) | .456 (.024) | .233 (.062) | .465 (.133) |
| Σp^* coefficients | .563 (.028) | .080 (.023) | .155 (.013) | .138 (.013) | .132 (.074) | .138 (.013) | -.483 (.037) | -.007 (.075) |
| n (-155) | 2,443 | 440 | 2,443 | 2,494 | 80 | 2,494 | | |
| $r.d.f.$ | 1,994 | 430 | 2,433 | 2,405 | 63 | 2,477 | $F(9,2,433)$ | $F(9,2,477)$ |
| \bar{R}^2 | .253 | .307 | .233 | .145 | .423 | .202 | =18.96 ^c | =10.97 ^c |

^a Time series observations only.

^b Constant term = .0444 with standard error of .0019.

^c The corresponding 1 per cent point for the distribution of F is approximately 2.42.

ries regression. The sum of the profit coefficients here is a rather astonishingly large .563 and, correspondingly, .491 in the industry time series. This serves to underscore our earlier inference, from the contrast of time series and cross section results observed in examining Table 3, that profits may have a role in determining the timing of investment rather than its average rate over time.

Table 7 enables us to explore the role of expected sales changes in regressions for all of the years 1955-1962. Although the number of observations is markedly reduced because of missing information on expected sales changes, we still have 1,465 individual firm observations and 60 industry-year-mean observations. We now observe some positive association between expected sales changes, particularly those over the longer-run four-year period, and capital expenditures. It would appear that past sales changes may be a proxy for the relation between expected future demand and capacity but that expected sales changes over a four-year period, as reported by McGraw-Hill respondents, offer further information bearing on long-run or permanent sales. The suggestion seems strongest in firm time series where the introduction of sales expectations increases the sum of all coefficients of sales changes from .276 in Table 5 to .346 in Table 7, and in within-industry firm cross sections where it increases the sum of all coefficients of sales changes from .287 to .342. This is consistent with our hypothesis that relatively more of the variance of past sales changes within firms

TABLE 7—CAPITAL EXPENDITURES, 1955-62, AS A FUNCTION OF EXPECTED AND ACTUAL SALES CHANGES, PROFITS AND DEPRECIATION,
ALL EXCEPT DEPRECIATION MEASURED AS RATIOS OF PREVIOUS GROSS FIXED ASSETS, SALES OR SALES AVERAGE:
FIRM AND INDUSTRY TIME SERIES, CROSS SECTION AND OVERALL REGRESSIONS

$$e_t^* = b_{70} + b_{71}\Delta s_{t+1}^i + b_{72}\Delta s_{t+1}^i + \sum_{j=3}^9 b_{7j}\Delta s_{t+2-j}^* + \sum_{j=10}^{11} b_{7j}p_{t+10-j}^* + b_{711}d_{63} + u_t$$

| Variable or Statistic (1) | Regression Coefficients and Standard Errors | | | | | | | Differences in Coefficients and Standard Errors | | | | Means and Standard Deviations (14) | |
|------------------------------------|---|----------------------------------|-------------------------------------|-----------------------------------|--|-------------------------------------|--|---|-----------------------------|--|---|--|--|
| | Firm Time Series (2) | Cross Section Means (3) | Firm Overall ^a (4) | Industry Time Series (5) | Firm Cross Section Within Industries (6) | Industry Cross Section (7) | Firm Cross Section Across Industries (8) | Industry Overall (9) | Firm Overall (10) | Cross Section of Firm Means-Firm Time Series [(3)-(2)] (11) | Industry Cross Section-Firm Cross Section Within Industries [(7)-(6)] (12) | | Industry Overall -Firm Cross Section Within Industries [(9)-(6)] (13) |
| Constant term or t_1^* | | | .033 (.003) | | | | | -.004 (.014) | .011 (.004) | | | | .080 (.063) |
| Δs_{t+4}^i | .042 (.012) | .090 (.024) | .070 (.011) | .345 (.068) | .029 (.011) | .096 (.068) | .037 (.010) | .160 (.065) | .049 (.011) | .048 (.027) | .067 (.069) | .130 (.066) | .197 (.141) |
| Δs_{t+1}^i | .017 (.019) | -.076 (.050) | .004 (.020) | -.035 (.085) | .020 (.018) | -.127 (.132) | .015 (.018) | -.116 (.118) | .010 (.018) | -.093 (.054) | -.147 (.133) | -.136 (.119) | .065 (.081) |
| d_{61} | — | — | — | — | .521 (.068) | .794 (.241) | .591 (.060) | .375 (.245) | .543 ^b (.062) | — | .273 (.251) | -.146 (.254) | .055 (.024) |
| $\Sigma \Delta s_t^i$ | .059 (.021) | .014 (.045) | .074 (.020) | .310 (.113) | .050 (.019) | -.031 (.126) | .053 (.019) | .044 (.128) | .059 (.019) | -.045 (.050) | -.081 (.127) | -.006 (.130) | |
| $\Sigma \Delta s_t^*$ | .287 (.055) | .399 (.052) | .378 (.032) | .479 (.198) | .293 (.031) | .397 (.167) | .313 (.030) | .557 (.178) | .337 (.031) | .112 (.075) | .105 (.170) | .265 (.181) | |
| $\Sigma \text{all } \Delta s_t$ | .346 (.061) | .412 (.061) | .452 (.037) | .789 (.226) | .342 (.036) | .366 (.194) | .366 (.035) | .601 (.197) | .396 (.036) | .067 (.087) | .024 (.197) | .258 (.200) | |
| Σp_t^* | .551 (.034) | .047 (.026) | .150 (.017) | .309 (.107) | .129 (.017) | -.043 (.091) | .108 (.016) | .145 (.085) | .136 (.016) | -.304 (.042) | -.172 (.093) | .016 (.087) | |
| π (-103) | 1.375 r.d.f. | 323 | 1.375 | 60 | 1.465 | 60 | 1.465 | 60 | 1.465 | $F(11,$ | $F(12,$ | $F(12,$ | |
| \bar{R}^2 | .321 | .294 | .239 | .760 | .213 | .495 | .256 | .570 | .286 | =12.92 ^b | =3.09 ^b | =5.89 ^b | |

^a Time series observations only.

^b The corresponding 1 per cent points for the distribution of F are approximately 2.26 for Col. (11) and 2.19 for Col. (12) and Col. (13).

TABLE 8—CAPITAL EXPENDITURES, 1955, 1957, 1960, 1962 AS A FUNCTION OF SALES CHANGES AND PROFITS, MEASURED AS RATIOS OF LAGGED GROSS FIXED ASSETS AND AVERAGE SALES, DEPRECIATION AND UTILIZATION OF CAPACITY: FIRM TIME SERIES, CROSS SECTION AND OVERALL REGRESSIONS

$$i_t^* = b_{80} + \sum_{j=1}^7 b_{8j} \Delta s_{t+1-j}^* + \sum_{j=8}^9 b_{8j} p_{t+8-j}^* + b_{8,10} d_{83} + b_{8,11} uc_{t-1}^r + u_8$$

| Variable or Statistic (1) | Regression Coefficients and Standard Errors | | | | | Differences in Coefficients and Standard Errors | Means and Standard Deviations (8) |
|---|---|---|----------------------------------|---|---------------------|---|--------------------------------------|
| | Firm Time Series (2) | Cross Section of Firm Means ^a (3) | Firm Overall ^a (4) | Firm Cross Section Within Industries (5) | Firm Overall (6) | Cross Section of Firm Means—Firm Time Series [(3)–(2)] (7) | |
| Constant term or i_t^* | | | -.008 (.022) | | -.043 (.021) | | .084 (.065) |
| $\Sigma \Delta s^*$ coefficients | .156 (.110) | .486 (.075) | .412 (.059) | .304 (.055) | .293 (.056) | .329 (.133) | |
| Σp^* coefficients | .490 (.063) | .051 (.035) | .129 (.028) | .096 (.028) | .120 (.027) | -.439 (.072) | |
| uc_{t-1}^r | .074 (.032) | .030 (.037) | .062 (.024) | .058 (.022) | .072 (.022) | -.043 (.049) | .913 (.123) |
| d_{83} | — | — | — | .590 (.107) | .585 (.109) | — | .058 (.024) |
| $\Sigma \Delta s^* + uc_{t-1}^r$ coefficients | .230 (.108) | .516 (.077) | .474 (.060) | .362 (.055) | .364 (.056) | .286 (.133) | |
| n (-37) | 445 | 159 | 445 | 534 | 535 | | |
| r.d.f. | 276 | 148 | 434 | 500 | 523 | | |
| \bar{R}^2 | .298 | .248 | .221 | .221 | .226 | $F(10, 434) = 4.13^b$ | |

^a Time series observations only.

^b The corresponding 1 per cent point for the distribution of F is approximately 2.37.

and within industries is viewed as transitory. The variance of sales expectations would then prove a net addition to the permanent component of sales variance.⁸

Since questions soliciting the information used in constructing the utilization-of-capacity variable, uc^r , were not included in all McGraw-Hill surveys, the Table 8 regressions introducing uc^r are based on 1955, 1957, 1960, and 1962 observations only. The results would appear to confirm a small but distinctly positive role for this direct measure of the pressure of output upon capacity.

Table 9, for the years 1961 and 1962 only, introduces Δv , measuring the change in value of the firm. It indicates that, even given past sales changes, the rate of investment tends to be greater in those firms that have seen greater increases (or lesser decreases) in the market's evaluation of the firm both for the current year and the past year. The Δv variable, however, seems essentially to be picking up effects attributed to corresponding Δs variables when Δv is not included. This suggests that, at least insofar as investment is con-

⁸ It is also possible, however, that we are really measuring the relation between capacity-expanding investment and the sales increases these are expected to make possible rather than any role of sales anticipations in determining investment.

TABLE 9—CAPITAL EXPENDITURES, 1961-62, AS A FUNCTION OF SALES CHANGES AND PROFITS MEASURED AS RATIOS OF LAGGED GROSS FIXED ASSETS AND AVERAGE SALES, CHANGE IN VALUE OF FIRM AND DEPRECIATION: FIRM AND INDUSTRY CROSS SECTIONS AND FIRM TIME SERIES AND OVERALL REGRESSIONS

$$i_t^* = b_{90} + \sum_{j=1}^7 b_{9j} \Delta s_{t+1-j}^* + \sum_{j=8}^9 b_{9j} p_{t+8-j}^* + \sum_{j=10}^{11} b_{9j} \Delta v_{t+10-j} + b_{9,12} d_{53} + u_9$$

| Variable or Statistic | Regression Coefficients and Standard Errors | | | | | Differences in Coefficients and Standard Errors | Means and Standard Deviations |
|---|---|--|--------------------------------------|----------------|------------------------|--|-------------------------------|
| | Firm Time Series | Cross Section of Firm Means ^a | Firm Cross Section Within Industries | Firm Overall | Industry Cross Section | Cross Section of Firm Means—Firm Time Series [(3)-(2)] | |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Constant term or i_t^* | | | | .018 (.004) | | | .063 (.050) |
| Δs_t^* | .027 (.036) | .031 (.048) | .026 (.021) | .000 (.021) | -.417 (.118) | .004 (.060) | .052 (.103) |
| Δs_{t-1}^* | .059 (.042) | -.041 (.058) | .038 (.020) | .045 (.020) | .080 (.146) | -.100 (.072) | .018 (.101) |
| Δs_{t-2}^* | .091 (.050) | .102 (.061) | .062 (.018) | .070 (.018) | .280 (.161) | .011 (.079) | .060 (.119) |
| Δv_t | .012 (.012) | -.034 (.022) | -.003 (.010) | .014 (.008) | .062 (.039) | -.046 (.025) | .049 (.248) |
| Δv_{t-1} | -.001 (.012) | .095 (.019) | .020 (.009) | .027 (.008) | .170 (.049) | .096 (.023) | .070 (.264) |
| d_{53} | — | — | .470 (.083) | .574 (.068) | .433 (.247) | — | .055 (.029) |
| $\Sigma \Delta s^*$ coefficients | .078 (.211) | .276 (.062) | .189 (.047) | .198 (.046) | .119 (.180) | .198 (.220) | |
| Σp^* coefficients | .385 (.138) | .003 (.035) | .093 (.026) | .067 (.025) | -.144 (.095) | -.382 (.142) | |
| $\Sigma \Delta v$ coefficients | .010 (.022) | .061 (.021) | .017 (.014) | .041 (.013) | .232 (.065) | .051 (.030) | |
| $\Sigma \Delta s^* + \Delta v$ coefficients | .089 (.212) | .337 (.060) | .206 (.047) | .239 (.045) | .351 (.163) | .248 (.220) | |
| $\Sigma p^* + d_{53}$ coefficients | — | — | .563 (.087) | .641 (.072) | .289 (.293) | — | |
| n (-23) | 464 | 232 | 540 | 540 | 20 | | |
| r.d.f. | 221 | 220 | 508 | 527 | 6 | $F(11, 452)$ | |
| \bar{R}^2 | .057 | .294 | .166 | .267 | .879 | $= 3.42^b$ | |

^a Time series observations only.

^b The corresponding 1 per cent point for the distribution of F is approximately 2.29.

cerned, "the market" did little more than project current sales changes. The sum of the Δv coefficients was somewhat larger in the cross section of firm means and largest in the industry cross section, amounting there to .232, with a standard error of .065. One may infer that the transitory, year-to-year variation of market value of individual firms tends to bias downward our estimates of these coefficients.

Finally, in Table 10, we present results with price-deflated gross fixed assets as the divisor of capital expenditures and profit. The results are not found to differ sharply from those shown in Tables 5 and 6, where gross fixed assets were not price deflated; the sum of sales change coefficients is slightly higher and that of profit coefficients slightly lower in the individual firm cross sections. The sample is

TABLE 10—CAPITAL EXPENDITURES, 1955-62, AS A FUNCTION OF SALES CHANGES AND PROFITS, MEASURED AS RATIOS OF LAGGED AVERAGE SALES AND PRICE DEFLATED GROSS FIXED ASSETS, AND DEPRECIATION: FIRM AND INDUSTRY TIME SERIES, CROSS SECTION AND OVERALL REGRESSIONS

$$\hat{z}_{pt}^* = b_{10.0} + \sum_{j=1}^7 b_{10,j} \Delta s_{t-1-j} + \sum_{j=8}^9 b_{10,j} p_{pt+s-j} + b_{10.10} d_{33} + u_{10}$$

| Variable or Statistic | Regression Coefficients and Standard Errors | | | | | | | | | | Differences in Coefficients and Standard Errors | | |
|------------------------------------|---|--------------------------------------|---------------------------------|--------------------------|--|----------------------------|--|----------------------|-------------------|---|---|---|--|
| | Firm Time Series (2) | Cross Section Means ^a (3) | Firm Overall ^{a,b} (4) | Industry Time Series (5) | Firm Cross Section Within Industries (6) | Industry Cross Section (7) | Firm Cross Section Across Industries (8) | Industry Overall (9) | Firm Overall (10) | Cross Section Means Firm Time Series [(9)-(2)] (11) | Industry Cross Section Within Industries [(7)-(6)] (12) | Industry Overall Cross Section Within Industries [(9)-(6)] (13) | |
| Constant term ^b | | | .041 (.003) | | | | | .013 (.010) | .012 (.003) | | | | |
| $\Sigma \Delta s^*$ coefficients | .168 (.080) | .682 (.062) | .608 (.037) | .512 (.256) | .329 (.039) | .520 (.169) | .419 (.037) | .689 (.162) | .436 (.037) | .514 (.102) | .190 (.173) | .360 (.167) | |
| Σp^* coefficients | .595 (.055) | -.003 (.036) | .066 (.023) | .458 (.178) | .119 (.023) | -.195 (.080) | .050 (.021) | -.133 (.079) | .062 (.021) | -.599 (.066) | -.313 (.083) | -.251 (.082) | |
| d_{33} | — | — | — | — | .546 (.068) | 1.000 (.243) | .706 (.059) | .777 (.232) | .696 (.059) | — | .454 (.252) | .231 (.242) | |
| $\Sigma p^* + d_{33}$ coefficients | — | — | — | — | .665 (.070) | .805 (.239) | .756 (.060) | .644 (.233) | .758 (.061) | — | .141 (.249) | — | |
| $n(-47)$ | 999 | 240 | 999 | 69 | 1,054 | 69 | 1,058 | 69 | 1,058 | $F(9, 989)$ | $F(10, 1,040)$ | $F(10, 1,047)$ | |
| r.d.f. | 750 | 230 | 989 | 50 | 979 | 51 | 1,040 | 58 | 1,047 | — | — | — | |
| \bar{R}^2 | .201 | .372 | .262 | .368 | .196 | .641 | .328 | .625 | .336 | = 8.27° | = 5.91° | = 5.91° | |

^a Time series observations only.

^b Mean of \hat{z}_{pt}^* = .070, standard deviation (from overall) = .061.

^c The corresponding 1 per cent points for the distribution of F are approximately 2.43 for Col. (11) and 2.34 for Col. (12) and Col. (13).

somewhat different and considerably smaller, however, as our method of capital stock deflation eliminated observations for which information as to five successive years of capital expenditures was not available. Other differences in coefficients seem fairly well within the range of sampling variability.

V. Summary and Conclusions

Taking time series and cross section slices of the same body of data at the levels of the firm and of broad industry groups, we have noted significant differences in estimates of the factors affecting capital expenditures and have tried to relate them to differing permanent and transitory components in the relevant variances and covariances. Our main findings are as follows:

1. The role of past sales changes, presumably as a proxy for expected long-run pressure of demand on capacity, appears greatest in the case of industry cross sections, and large in cross sections of firms across industries, particularly in cross sections of firm means.

2. The coefficients of past sales changes are correspondingly lower in the within-industry cross sections.

3. The variance of past sales changes about the mean of sales changes for each individual firm (firm time series) has significantly less to do with the variance in capital expenditures than the corresponding variances in the firm cross sections. This is consistent with the view that firms would look upon the short-run variance in their own sales as mostly transitory.

4. Coefficients of sales changes are generally higher in industry time series than in firm time series. This is consistent with the hypothesis of a greater permanent component in industry sales change variance over time.

5. New light is cast on the role of profits in distributed lag investment functions including a considerable number of lagged sales changes. While coefficients of the profit variables are uniformly low in cross sections they are relatively high in most of the time series. Firms apparently tend to make capital expenditures in the period immediately following higher profits, but firms earning higher profits do not make markedly greater capital expenditures than firms earning lower profits. This evidence is consistent with the hypothesis that past profits play some significant role in the timing of capital expenditures but do not affect its long-run average.

6. Expected sales changes may have some role in capital expenditures over and above that noted in current and lagged actual sales changes. The sales change expected over the ensuing four-year period, in particular, is positively related to capital expenditures.

7. The rate of return on market value of the firm does not prove statistically significant in the cross sections but its coefficient is distinctly negative in the firm time series. One may presume that expected future earnings are positively related to the expected profitability of investment, and hence to investment itself, as well as to the value of the firm. Given current profits, the observed negative relation between capital expenditures and rate of return could then be attributed to fluctuations in expected earnings. This is consistent with the general hypothesis that expected future earnings (long-run or permanent income) play an underlying role in the investment function. Some confirmation may be found in positive coefficients of variables measuring changes in the value of the firm, but none in estimates of coefficients of the ratio of the market value of the firm to a "book value" constructed as the sum of net worth, depreciation reserves, and bonded indebtedness.

8. Further evidence of the role in capital expenditures of the pressure of demand on capacity appears in positive coefficients of the ratios of actual to preferred rates of utilization of capacity.

9. Results do not differ markedly when capital expenditures and profits are divided by price-deflated gross fixed assets rather than gross fixed assets at original cost.

All of this should be taken as a preliminary report of intensive investigation of a large and greatly expanded body of individual firm data. It does confirm, however, that wide differences, far beyond those which may reasonably be expected from sampling variability, can appear in estimates of economic relations from the same underlying data when observations are grouped in different ways. The differences in estimates, in the context of the investment function, as in the consumption function, may plausibly be related to differences in the "permanent" components of the variables under consideration. To the extent that our econometric efforts are intended to reveal structural relations rather than to be merely descriptive of particular arrangements of data, it would appear vitally important to define the parameters to be estimated clearly in terms of the nature of the variances to which they are to apply.

APPENDIX A

CLASSIFICATION OF OBSERVATIONS USED IN TABLES 1, 2, AND 3, BY INDUSTRY AND YEAR

| Industry | Number of Observations | | | | | | | | |
|---|------------------------|------|------|------|------|------|------|------|-------|
| | 1955 | 1956 | 1957 | 1958 | 1959 | 1960 | 1961 | 1962 | Total |
| Primary metals | 22 | 31 | 25 | 27 | 25 | 26 | 24 | 26 | 206 |
| Metal working | 102 | 131 | 109 | 96 | 87 | 96 | 90 | 84 | 795 |
| Chemical processing | 64 | 73 | 69 | 58 | 58 | 65 | 56 | 49 | 492 |
| All other manufacturing | 64 | 88 | 78 | 66 | 60 | 62 | 55 | 54 | 527 |
| Mining | 9 | 15 | 12 | 10 | 7 | 12 | 10 | 11 | 86 |
| Utilities | 40 | 41 | 39 | 36 | 41 | 42 | 39 | 40 | 318 |
| Petroleum | 19 | 25 | 21 | 21 | 20 | 16 | 16 | 15 | 153 |
| Stores | 34 | 51 | 47 | 47 | 38 | 35 | 35 | 25 | 312 |
| Railroads | 25 | 27 | 26 | 24 | 21 | 20 | 17 | 13 | 173 |
| Transportation and communication other than railroads | 7 | 10 | 13 | 12 | 12 | 9 | 11 | 11 | 85 |
| All industries | 386 | 492 | 439 | 397 | 369 | 383 | 353 | 328 | 3,147 |

APPENDIX B: ALGEBRAIC STATEMENT OF DEVIATIONS USED
IN THE VARIOUS REGRESSIONS

Let X_{fnt} denote the observation vector of firm f in industry n for the year t .

Let F_{nt} denote the number of firms with observations in industry n for the year t .

Let τ_{fn} denote the number of years of observations for firm f in industry n .

Let N_t denote the number of industries containing observations in the year t .

Let τ denote the number of years for which observations are available. Then,

$$\bar{X}_{fn} = \frac{\sum_{t=1}^{\tau_{fn}} X_{fnt}}{\tau_{fn}} = \text{the mean of observations of all years for firm } f \text{ in industry } n \text{ (firm mean),}$$

$$\bar{X}_{nt} = \frac{\sum_{f=1}^{F_{nt}} X_{fnt}}{F_{nt}} = \text{the mean of observations of all firms in industry } n \text{ in year } t \text{ (industry-year mean),}$$

$$\bar{X}_n = \frac{\sum_{t=1}^{\tau} \sum_{f=1}^{F_{nt}} X_{fnt}}{\sum_{t=1}^{\tau} F_{nt}} = \begin{array}{l} \text{the mean of observations of all firms in industry } n, \\ \text{in all years (industry mean),} \end{array}$$

$$\bar{X}_t = \frac{\sum_{n=1}^{N_t} \sum_{f=1}^{F_{nt}} X_{fnt}}{\sum_{n=1}^{N_t} F_{nt}} = \begin{array}{l} \text{the mean of observations of all firms in all in-} \\ \text{dustries in year } t \text{ (year mean), and} \end{array}$$

$$\bar{X} = \frac{\sum_{t=1}^{\tau} \sum_{n=1}^{N_t} \sum_{f=1}^{F_{nt}} X_{fnt}}{\sum_{t=1}^{\tau} \sum_{n=1}^{N_t} F_{nt}} = \begin{array}{l} \text{the mean of all observations of all industries in} \\ \text{all years (overall mean).} \end{array}$$

Hence,

$X_{fnt} - \bar{X}_{fn}$ = the deviations used in firm time series and

$\bar{X}_{fn} - \bar{X}$ (weighted by τ_{fn}) = the deviations used in the cross sections of firm means, each variable relating only to observations from firms for which $\tau_{fn} > 1$,

$X_{fnt} - \bar{X}_{nt}$ = the deviations used in firm cross sections within industries,

$X_{fnt} - \bar{X}_t$ = the deviations used in firm cross sections across industries, and

$X_{fnt} - \bar{X}$ = the deviations used in firm overall regressions.

Finally,

$\bar{X}_{nt} - \bar{X}_n$ = the deviations used in industry time series.

$\bar{X}_{nt} - \bar{X}_t$ = the deviations used in industry cross sections,

$\bar{X}_{nt} - \bar{X}$ = the deviations used in industry overall regressions, and

$$\frac{F_{nt} \sum_{t=1}^{\tau} N_t}{\sum_{t=1}^{\tau} \sum_{n=1}^{N_t} F_{nt}} = \begin{array}{l} \text{the weight attached to the observation for industry } n \text{ in the} \\ \text{year } t. \end{array}$$

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TAX POLICY AND INVESTMENT BEHAVIOR

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The effectiveness of tax policy in altering investment behavior is an article of faith among both policy makers and economists. Whatever the grounds for this belief, its influence on postwar tax policy in the United States has been enormous. In 1954 and again in 1962 amortization of capital expenditures was liberalized by providing for faster writeoffs. Since 1962 a tax credit for expenditure on equipment has been in force. Nor is tax policy in the United States atypical. As Otto Eckstein [8] has pointed out,

Tax devices to stimulate investment have certainly been the greatest fad in economic policy in the past ten years. In a period when the trends in the use of policy instruments were in the direction of more general, less selective devices, all sorts of liberalized depreciation schemes, investment allowances, and tax exemptions were embraced with enthusiasm all over the non-Communist world.¹

The customary justification for the belief in the efficacy of tax stimulus does not rely on empirical evidence. Rather, the belief is based on the plausible argument that businessmen in pursuit of gain will find the purchase of capital goods more attractive if they cost less.² In view of the policy implications of this theoretical argument, it is surprising that no attempt has been made to estimate the magnitude of tax effects on investment. Previous studies have been limited to calculations of the effects of tax policy on the cost of capital services.³ The relation between these changes in the cost of capital and actual investment expenditures has not been studied empirically. As a result, the most important questions for economic policy—How much investment will result from a given policy measure? When will it occur?—have been left unanswered.

The purpose of this paper is to study the relationship between tax policy and investment expenditures using the neoclassical theory of

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¹ See [8, p. 351]; an excellent comparison of U.S. and European tax policy, including depreciation policy and investment tax credits, is given in [7].

² The effects of tax policy on investment behavior are analyzed from this point of view by N. B. Ture [19, esp. pp. 341-45]; by S. B. Chase, Jr. [3]; and by R. A. Musgrave [15, pp. 53-54, 117-29]. Many other references could be given.

³ See, for example: E. C. Brown [2] and Chase [3, pp. 46-52].

optimal capital accumulation.⁴ First, we measure the cost to the business firm of employing fixed assets. This cost depends on the rate of return, the price of investment goods, and the tax treatment of business income. Second, we determine empirically the relation between the cost of employing capital equipment and the level of investment expenditures. This relationship is a straightforward generalization of the familiar flexible accelerator theory of investment. We first obtain an estimate of the distribution over time of the investment expenditures resulting from a given increment in the desired level of capital services; then we estimate both the amount of investment resulting from a change in tax policy and its distribution over time. We consider the effects of: (1) the adoption of accelerated methods for computing depreciation for tax purposes in 1954, (2) the investment tax credit of 1962, and (3) the depreciation guidelines of 1962. As an illustration we consider the hypothetical effects of (4) adoption of first-year writeoff in 1954 in place of less drastic accelerated depreciation.

Our basic conclusion is that tax policy is highly effective in changing the level and timing of investment expenditures. In addition we find that tax policy has had important effects on the composition of investment. According to our estimates, the liberalization of depreciation rules in 1954 resulted in a substantial shift from equipment to structures. On the other hand, the investment tax credit and depreciation guidelines of 1962 caused a shift toward equipment.

I. Tax Policy and the Cost of Capital Services

The neoclassical theory of optimal capital accumulation may be formulated in two alternative and equivalent ways. First, the firm may be treated as accumulating assets in order to supply capital services to itself. The objective of the firm is to maximize its value, subject to its technology. Alternatively, the firm may be treated as renting assets in order to obtain capital services; the firm may rent assets from itself or from another firm. In this case, the objective of the firm is to maximize its current profit, defined as gross revenue less the cost of current inputs and less the rental value of capital inputs. The rental can be calculated from the basic relationship between the price of a new capital good and the discounted value of all the future services derived from this capital good.⁵ In the absence of direct taxation this relationship takes the form:

$$(1) \quad q(t) = \int_t^{\infty} e^{-r(s-t)} c(s) e^{-\delta(s-t)} ds,$$

where r is the discount rate, q the price of capital goods, c the cost of

⁴ This model has been studied previously by D. W. Jorgenson [11 and 12].

⁵ The equivalence of these two formulations is discussed by D. W. Jorgenson [14].

capital services, and δ the rate of replacement; in this formulation t is the time of acquisition of the capital goods and s the time at which capital services are supplied. Differentiating this relationship with respect to time of acquisition we obtain:

$$(2) \quad c = q(r + \delta) - \dot{q},$$

which is the rental of capital services supplied by the firm to itself. Under static expectations about the price of investment goods, the rental reduces to:

$$(3) \quad c = q(r + \delta).$$

Expression (3) derived above for the cost of capital services may be extended to take account of a proportional tax on business income. We assume that the tax authorities prescribe a depreciation formula $D(s)$ which gives the proportion of the original cost of an asset of age s that may be deducted from income for tax purposes. Further, we assume that a tax credit at rate k is allowed on investment expenditure and that the depreciation base is reduced by the amount of tax credit.⁶ If the tax rate is constant over time at rate u , the equality between the price of investment goods and the discounted value of capital services is:

$$(4) \quad q(t) = \int_t^{\infty} e^{-r(s-t)} [(1-u)c(s)e^{-\delta(s-t)} + u(1-k)q(t)D(s)]ds + kq(t).$$

Denoting by z the present value of the depreciation deduction on one dollar's investment (after the tax credit),

$$(5) \quad z = \int_0^{\infty} e^{-rs} D(s) ds.$$

The implicit rental value of capital services under static expectations then becomes:

$$(6) \quad c = q(r + \delta) \frac{(1-k)(1-uz)}{1-u}$$

Under the Internal Revenue Code of 1954 at least three depreciation formulas could be employed for tax purposes: straight-line, sum of the years' digits, and double declining balance. To obtain the appropriate cost of capital services for each formula, it is necessary to calculate the present value of the depreciation deduction for each one. Throughout we assume that the asset has no salvage value.

For straight-line depreciation, the deduction is constant over a period of length τ , the lifetime for tax purposes:

⁶ This assumption is valid for 1962 and 1963. For 1964 and later years the depreciation base was not reduced by the amount of the tax credit.

$$D(s) = \begin{cases} \frac{1}{\tau} & \text{for } 0 \leq s \leq \tau, \\ 0 & \text{otherwise.} \end{cases}$$

The present value of the deduction is:

$$\begin{aligned} z &= \int_0^{\tau} \frac{e^{-rs}}{\tau} ds, \\ (7) \quad &= \frac{1}{r\tau} (1 - e^{-r\tau}). \end{aligned}$$

For sum of the years' digits, the deduction declines linearly over the lifetime for tax purposes:

$$D(s) = \begin{cases} \frac{2(\tau - s)}{\tau^2} & \text{for } 0 \leq s \leq \tau, \\ 0 & \text{otherwise.} \end{cases}$$

The present value of the deduction is:

$$\begin{aligned} z &= \int_0^{\tau} e^{-rs} \frac{2(\tau - s)}{\tau^2} ds, \\ (8) \quad &= \frac{2}{r\tau} \left[1 - \frac{1}{r\tau} (1 - e^{-r\tau}) \right]. \end{aligned}$$

Tax provisions for double declining balance depreciation are more complicated. A firm may switch to straight-line depreciation at any time. If the switchover point is denoted τ^+ , the double declining balance depreciation formula is:

$$D(s) = \begin{cases} \frac{2}{\tau} e^{-(2/\tau)s} & \text{for } 0 \leq s \leq \tau^+, \\ \frac{1 - e^{-(2/\tau)\tau^+}}{\tau - \tau^+} & \text{for } \tau^+ \leq s \leq \tau \\ 0 & \text{otherwise.} \end{cases}$$

The present value of the deduction is:

$$\begin{aligned} z &= \frac{2}{\tau} \int_0^{\tau^+} e^{-(r+(2/\tau))s} ds + \frac{1 - e^{-(2/\tau)\tau^+}}{\tau - \tau^+} \int_{\tau^+}^{\tau} e^{-rs} ds, \\ (9) \quad &= \frac{\frac{2}{\tau}}{r + \frac{2}{\tau}} [1 - e^{-(r+(2/\tau))\tau^+}] + \frac{1 - e^{-(2/\tau)\tau^+}}{r(\tau - \tau^+)} (e^{-r\tau^+} - e^{-r\tau}). \end{aligned}$$

The switchover point which maximizes z is $\tau^+ = \tau/2$.

Representative values of the present value of the deduction for each of the three methods are given in Table 1.⁷ From this table it is clear that the sum of the years' digits depreciation formula dominates the double declining balance and straight-line formulas in the range of discount rates and lifetimes with which we are concerned. For this reason we have represented the 1954 tax revision as a change from straight-line to sum of the years' digits depreciation formulas.⁸ Under static expecta-

TABLE 1—PRESENT VALUE OF DEPRECIATION DEDUCTION

| Lifetime | Interest Rate | Straight-line | Sum of the Years' Digits | Double Declining Balance |
|----------|---------------|---------------|--------------------------|--------------------------|
| 5 | .06 | .864 | .907 | .888 |
| 5 | .12 | .752 | .827 | .795 |
| 10 | .06 | .752 | .827 | .795 |
| 10 | .12 | .582 | .696 | .651 |
| 25 | .06 | .518 | .643 | .594 |
| 25 | .12 | .317 | .456 | .410 |
| 40 | .06 | .379 | .518 | .469 |
| 40 | .12 | .207 | .331 | .297 |

tions the formulas for the rental value of capital services for these two methods of depreciation are:

straight-line:

$$(10) \quad c = \frac{1-k}{1-u} q(r+\delta) \left[1 - \frac{u}{r\tau} (1 - e^{-r\tau}) \right];$$

sum of the years' digits:

$$(11) \quad c = \frac{1-k}{1-u} q(r+\delta) \left[1 - u \frac{2}{r\tau} \left(1 - \frac{1}{r\tau} \{ 1 - e^{-r\tau} \} \right) \right].$$

Under the assumption that firms behave so as to maximize profit and that the markets for their output are perfectly competitive, a firm's

⁷ The results presented in Table I may be compared with those of Sidney Davidson and D. F. Drake [5]; see also: Davidson and Drake [6].

⁸ The adoption of accelerated methods for computing depreciation in 1954 involved a change from straight-line depreciation to either sum of the years' digits or double declining balance formulas. Since sum of the years' digits offers a slight advantage over double declining balance, we have assumed that accelerated depreciation was taken in the form of the sum of the years' digits. Further, we have assumed that accelerated methods were adopted immediately after they were made available. In fact, approximately 50 per cent (of new assets) were depreciated on an accelerated basis the first year and a similar percentage of the uncovered balance was added in subsequent years. For firms that had negotiated shorter lifetimes than those allowed beginning 1954, there was some incentive to continue using straight-line methods in order to meet the "reserve ratio test," now effectively abandoned. The shorter lifetimes may be approximated by accelerated depreciation.

desired level of capital can be derived from the condition that the value of the marginal product of capital should be equal to the rental price of capital. For a Cobb-Douglas production function, the desired level of capital K^+ is:

$$(12) \quad K^+ = \alpha \frac{pQ}{c},$$

where p is the price of output, Q its quantity, c the rental price of capital, and α the elasticity of output with respect to capital. We assume that the flow of capital services is proportional to capital stock. This completes the determination of desired capital.⁹

To complete the theory of investment behavior it is necessary to specify the relationship between changes in desired capital and actual investment expenditures. After a change in the desired level of assets, plans must be formulated, funds appropriated, orders and contracts let, and so on. We assume that subsequent to a change in desired capital, a certain proportion of the resulting investment expenditure takes place over each interval of time. This proportion may vary by class of asset but is independent of calendar time.¹⁰

Second, we must specify the theory of replacement investment. We assume that subsequent to an investment a certain proportion is replaced over each interval of time. Again, we allow this proportion to vary by class of asset, but assume it to be independent of calendar time. Under our earlier assumption of a constant rate of replacement, investment for replacement is proportional to capital stock. This assumption implies an exponential survival curve for capital goods.¹¹

Under these assumptions the theory of investment behavior takes the form of a distributed lag function; in discrete form this function may be written:

$$(13) \quad I_t = \sum_{s=0}^{\infty} \mu_s \Delta K_{t-s}^+ + \delta K_t.$$

Gross investment in period t , I_t , is the sum of a weighted average of past changes in desired capital and replacement investment. The change in desired capital in period $t-s$ is ΔK_{t-s}^+ ; the parameter μ_s is the proportion of the change in desired capital in period $t-s$ that results in investment expenditures in period t . Replacement investment is proportional to capital stock K_t ; the constant of proportionality is δ , the rate of replacement. An alternative and equivalent form of the distributed lag

⁹ A more detailed derivation of desired capital stock is given in "Anticipations and Investment Behavior," [11, pp. 43-53].

¹⁰ This theory of investment is discussed in more detail in "Anticipations and Investment Behavior," [11, pp. 46-50].

¹¹ See "Anticipations and Investment Behavior," [11, p. 51].

function gives net investment N_t as a weighted average of past changes in desired capital stock:

$$(14) \quad \begin{aligned} N_t &= I_t - \delta K_t, \\ &= \sum_{s=0}^{\infty} \mu_s \Delta K_{t-s}^+. \end{aligned}$$

To estimate the parameters of the distributed lag function, it is necessary to impose further restrictions on the sequence of coefficients $\{\mu_s\}$. We have taken the first two coefficients of this sequence to be arbitrary with the remaining coefficients declining in a geometric series. The final form of the resulting distributed lag function is:

$$(15) \quad N_t = \gamma_0 \Delta K_t^+ + \gamma_1 \Delta K_{t-1}^+ - \omega N_{t-1},$$

where γ_0 , γ_1 , and ω are parameters that characterize the sequence $\{\mu_s\}$. Adding an independently and identically distributed random term ϵ_t to the final form, we obtain the regression function:

$$(16) \quad N_t = \alpha \gamma_0 \Delta \frac{p_t Q_t}{c_t} + \alpha \gamma_1 \Delta \frac{p_{t-1} Q_{t-1}}{c_{t-1}} - \omega N_{t-1} + \epsilon_t,$$

where α is the elasticity of output with respect to capital. The parameters γ_0 , γ_1 , ω and α are unknown and must be estimated.¹² This investment function provides the basis for the statistical results reported in the following section.

To summarize, investment in period t depends on the capital stock at the beginning of the period and changes in the desired level of capital stock in previous periods. The form of the relationship depends on the parameters of the distributed lag function and the rate of replacement. Desired capital depends in turn on the value of output, the rental value of capital input, and the elasticity of output with respect to capital input.

The effects of tax policy on investment behavior enter the investment function through the rental value of capital input. A change in tax policy changes the rental value of capital input. This results in a change in the desired level of capital stock. A change in desired capital stock results in net investment (or disinvestment), bringing capital stock up (or down) to its new desired level. If there are no further changes in tax policy or in the other determinants of desired capital stock, net investment eventually drops to zero. The change in tax policy continues to affect gross investment through replacement of a permanently larger (or smaller) capital stock.

¹² Methods of estimation for such a distributed lag function are discussed by D. W. Jorgenson [13].

Our procedure is, first, to estimate the investment functions under the tax policies that actually prevailed. The results are given in the following section. Second, we employ the estimated investment functions to calculate the investment resulting from alternative tax policies. These calculations are given in Section III. We then analyze the results in order to assess the effectiveness of tax policy in changing the level and timing of investment expenditures. We also study the effects of tax policy on the distribution of investment between plant and equipment.

II. *Estimates of the Parameters of the Investment Function*

To implement the theory of investment behavior outlined in the preceding section, we have fitted the corresponding econometric model to data on investment expenditures from the Capital Goods Study of the Office of Business Economics (OBE).¹³ Data are available for structures and equipment separately and for both manufacturing and non-farm, non-manufacturing sectors of the U.S. economy for the years 1929–63. These data are derived by allocating the commodity flow data on gross private domestic investment from the national product accounts among sectors of destination.

Estimates of capital stock at the beginning of each period K_t , were obtained by applying the following recursion relation to the investment data described above:

$$(17) \quad K_t = I_{t-1} + \delta K_{t-1},$$

where I_t is investment in current prices deflated by an investment goods price index and δ is the rate of replacement, taken to be 2.5 times the inverse of the Bulletin F [21] lifetime. The following values were used for δ :

| | |
|--|--------|
| manufacturing equipment | 0.1471 |
| manufacturing structures | 0.0625 |
| non-farm, non-manufacturing equipment | 0.1923 |
| non-farm, non-manufacturing structures | 0.0694 |

Initial values for capital stock were estimated by cumulating net investment over the whole period for which data are available for each asset.

Published price indexes for gross private domestic investment are biased because to a considerable extent they measure the prices of inputs to the capital goods industries rather than the price of output. To overcome this bias, we used price indexes based on output prices that are close substitutes in production for producers' durables and business structures—the implicit deflator for consumers' durables from the national product accounts and the Bureau of Public Roads price index for

¹³ The OBE Capital Goods Study is reported by George Jaszi, Robert Wasson, and L. Grose [10]. More recent data were kindly supplied by Mr. Robert Wasson of the OBE.

structures.¹⁴ For the years before these indexes are available we used the indexes implicit in the OBE Capital Goods Study, adjusted for bias in the rate of growth. The biases were estimated by regression which yielded the following values: 0.00651 per year for equipment and 0.0183 per year for structures.

The effects of tax policy enter the investment function through the desired level of capital stock. To estimate the desired level we used value added at factor cost as a measure of output $p_t Q_t$. We calculated value added for manufacturing and non-manufacturing, non-farm sectors by adding estimates of capital consumption allowances to national income originating in each sector.¹⁵

The desired level of capital stock also depends on the rental value of capital input c_t . Through 1953 the appropriate rental value of capital services corresponds to straight-line depreciation. Since 1954 the appropriate rental value corresponds to sum of the years' digits depreciation. Until 1962 the investment tax credit k is equal to zero. For 1962 and 1963 this credit is 7 per cent of the value of investment goods. In the formulas for the rental value of capital goods, the tax rate u , the after-tax rate of return r , the investment goods price q , and the lifetime of capital goods allowable for tax purposes τ are variables. The rate of replacement δ is a fixed parameter. The values of this parameter are the same as those employed in calculating capital stock.

We took the corporate tax rate to be the statutory rate prevailing during most of the year. We did not attempt to allow for excess profits taxes during the middle thirties or the Korean War. For the discount

¹⁴ The implicit deflators for structures from the U.S. national accounts are primarily indexes of the cost of input rather than the price of output. The Bureau of Public Roads index for structures is based on the price of output; D. C. Dacy [4] has derived price indexes for road construction based on input and output prices. His index for the price of output grows from 80.5 to 98.2 from 1949 through 1959 while the price of input grows from 61.5 to 102.4 in the same period, both on a base of 100.0 in 1958. The implicit deflator for new construction in the national accounts grows from 51.3 to 103.0 in the same period. Although there is no real alternative to the Bureau of Public Roads index as an output price for structures, it is reassuring to find that the corresponding input price behaves in a manner very similar to that of the input price for all of new construction.

The price indexes for equipment from the U.S. national accounts are based on data from the wholesale price index of the Bureau of Labor Statistics. Since expenditures in the wholesale price index are less than those on the consumer's price index, adjustments for quality change are less frequent and less detailed. Some notion of the resulting bias in the growth of the implicit deflator for producers' durables can be obtained by comparing this index with the implicit deflator for consumers' durables. The producers' durables deflator increased from 64.6 in 1947 to 102.0 in 1959. Over this same period the deflator for consumers' durables increased from 82.7 to 101.4. Both indexes are computed relative to a base of 100.0 in 1958. A direct comparison of the durables components of the wholesale and consumers' price indexes reveals essentially the same relationship.

For further discussion, see Zvi Griliches and D. W. Jorgenson [9].

¹⁵ All data are from the U.S. national accounts; see: U.S. Dept. of Commerce [20] and *Survey of Current Business* [18].

rate before taxes we used the figure of .14 throughout the period. Although there is little evidence that this rate varies over the period of fit, except for cyclical fluctuations, this rate appears to be somewhat conservative.¹⁶ Estimates of lifetimes of assets allowable for tax purposes were obtained separately for assets acquired before 1954 and during and after 1954 from a special Treasury study [23]. The change between the two periods was divided equally between 1954-55 and 1955-56. For 1962 and 1963 the proportional change for the new guidelines relative to existing practice as estimated by the Treasury [24] was applied to the 1961 lifetimes for equipment. Lifetimes assumed were as follows:

| Period | Equipment | Structures |
|---------|-----------|------------|
| 1929-54 | 17.5 | 27.8 |
| 1955 | 16.3 | 25.3 |
| 1956-61 | 15.1 | 22.8 |
| 1962-63 | 13.1 | 22.8 |

Investment functions for equipment and structures for both manufacturing and non-farm, non-manufacturing sectors of the U.S. economy for the years 1931-41 and 1950-63¹⁷ are presented in Table 2. The coeffi-

TABLE 2—INVESTMENT FUNCTIONS FOR MANUFACTURING AND NON-FARM, NON-MANUFACTURING EQUIPMENT AND STRUCTURES FOR 1931-41, 1950-63

| | $\Delta(pQ/c)_t$ | $\Delta(pQ/c)_{t-1}$ | N_{t-1} | $\hat{\alpha}$ | Mean Lag | R_N^2 | R_I^2 | d |
|--|--------------------|----------------------|------------------|------------------|-----------------|---------|---------|-------|
| Manufacturing equipment | .01419 (.00372) | .01242 (.00442) | .6152 (.1001) | .0691 (.0156) | 2.065 (.258) | .7219 | .9566 | 2.036 |
| Manufacturing structures | .00396 (.00131) | .00526 (.00145) | .7658 (.0790) | .0394 (.0126) | 3.840 (.343) | .8475 | .9208 | 2.474 |
| Non-farm, non-manufacturing equipment | .02452 (.00844) | .01460 (.01038) | .4692 (.1342) | .0737 (.0141) | 1.257 (.261) | .6899 | .9616 | 1.738 |
| Non-farm, non-manufacturing structures | .01296 (.00197) | .00227 (.00223) | .8801 (.0322) | .1269 (.0250) | 7.488 (.239) | .9830 | .9908 | 1.435 |

cients associated with $\Delta(p_t Q_t)/c_t$ and $\Delta(p_{t-1} Q_{t-1})/c_{t-1}$, respectively, are estimates of the $\alpha\gamma_0$ and $\alpha\gamma_1$. The coefficient associated with lagged net investment, N_{t-1} , is an estimate of $-\omega$. Using the fact that the coefficients of the distributed lag function sum to unity, an estimate of the elasticity of output with respect to capital may be obtained. This estimate, d , is also presented in Table 2.

Second, the average lag between changes in desired capital and actual investment expenditure as derived from the coefficients of the distributed lag function is presented in Table 2. Finally, Table 2 contains measures of the goodness of fit of the regression— R_N^2 , R_I^2 , and d , the Durbin-Watson ratio. Goodness of fit is measured in two ways: the ratio

¹⁶ A figure suggested by the results of Jorgenson and Griliches [9] is 20 per cent before taxes. This figure excludes capital gains whether realized or unrealized.

¹⁷ The years 1942-1947 are eliminated from the regressions because of the widespread use of nonprice allocation of capital goods during these years.

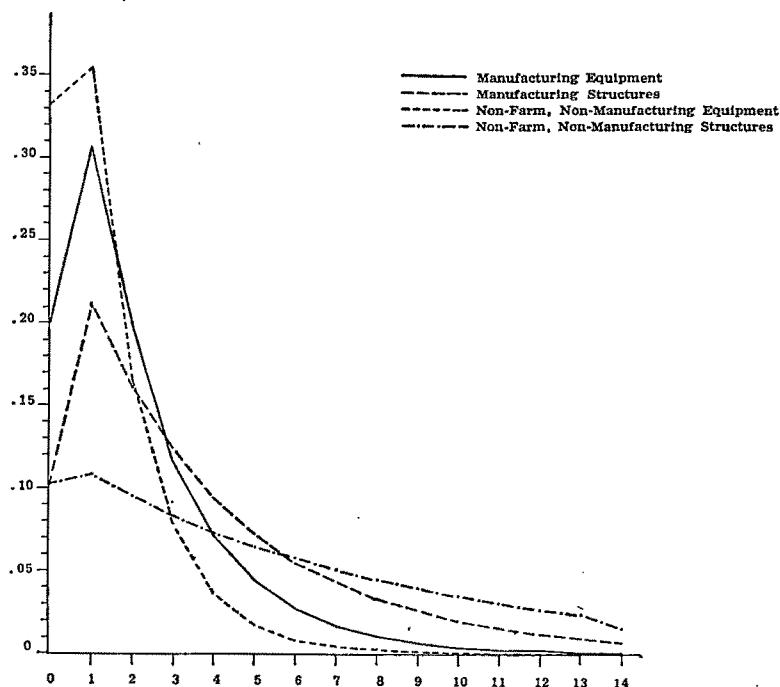


FIGURE 1

of the explained sum of squares to the total sum of squares for gross investment, R_T^2 ; the ratio of the explained sum of squares to the total sum of squares in net investment, R_N^2 . Of course, gross investment is the variable of interest for policy considerations.

Estimates of the coefficients of the distributed lag function $\{\mu_s\}$ may be calculated from estimates of the parameters γ_0 , γ_1 , and ω by the usual recursion formula.¹⁸ The first fifteen terms of the sequence $\{\mu_s\}$ derived by this technique are presented in Figure 1. The general shape of the distributed lag functions coincides with previous results based on quarterly data. A substantial part of the investment takes place during the year in which the change in desired capital occurs. However, even more occurs in the following year. By assumption the proportions of investment that result from a given change in desired capital decline geometrically in subsequent years. The average lag for investment in equipment is approximately 2 years for manufacturing and about 1.3 years for non-manufacturing. The average lag for structures is considerably longer, ranging from 3.8 years in manufacturing to 7.5 years in non-manufacturing.

¹⁸ This formula is given in D. W. Jorgenson [13].

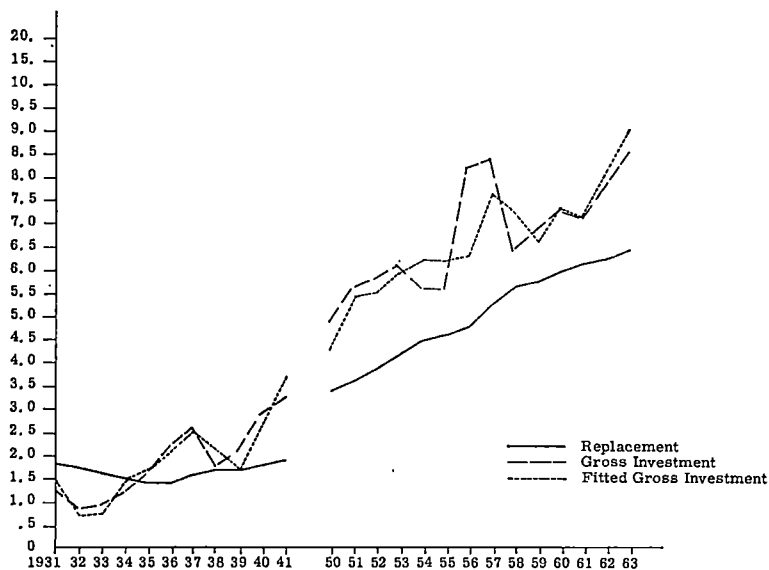


FIGURE 2a. MANUFACTURING EQUIPMENT

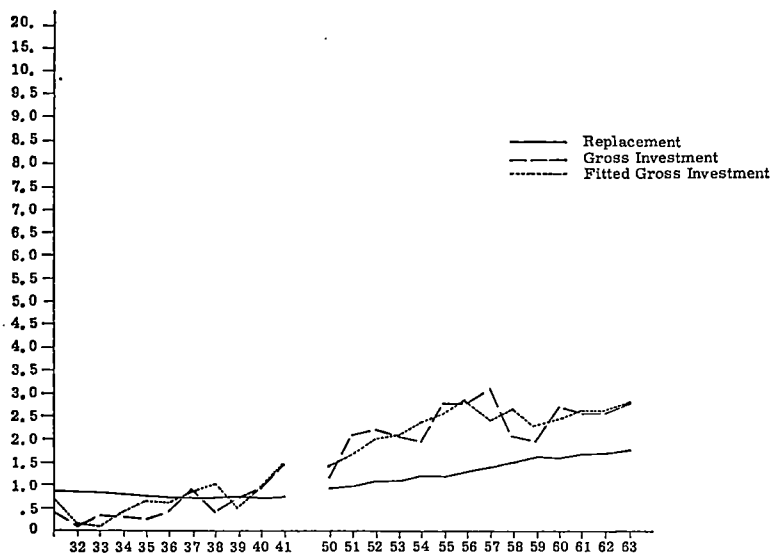


FIGURE 2b. MANUFACTURING STRUCTURES

To give a better notion of the degree of conformity between fitted values of investment and the actual observations, fitted gross investment is plotted against actual gross investment in Figures 2a-2d. Net investment is calculated from the fitted regression; replacement invest-

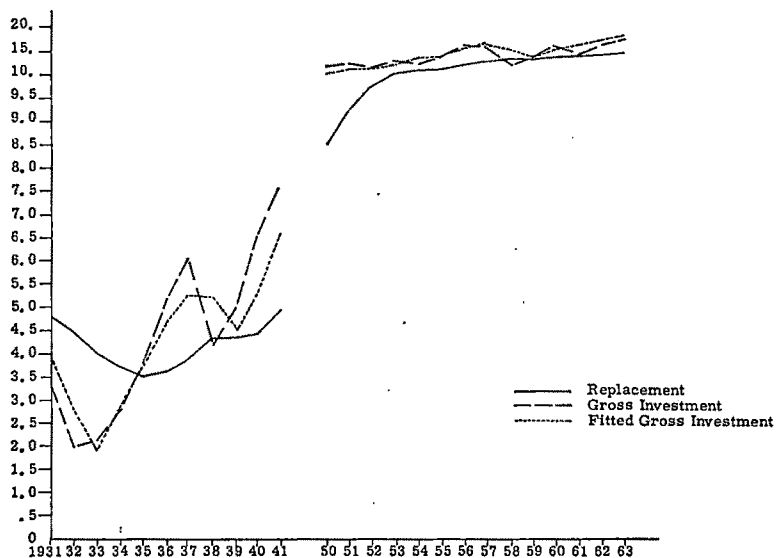


FIGURE 2c. NON-FARM, NON-MANUFACTURING EQUIPMENT

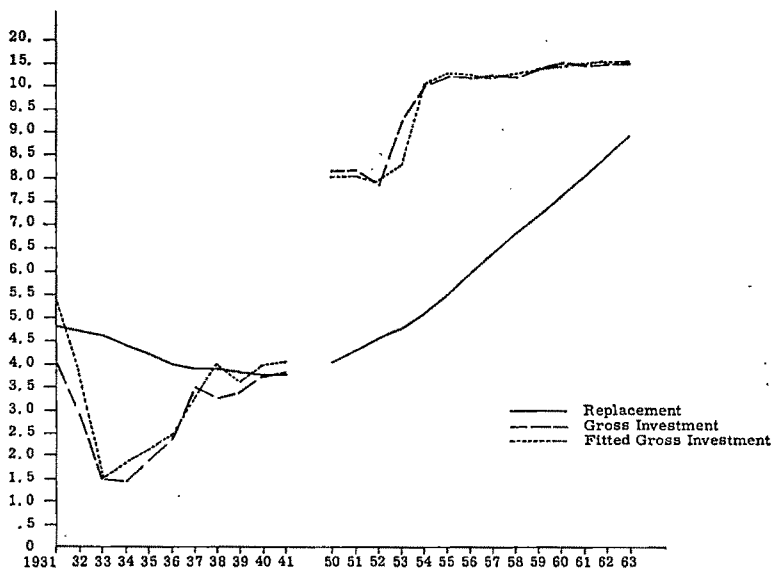


FIGURE 2d. NON-FARM, NON-MANUFACTURING STRUCTURES

ment, taken as a given datum, is then added to obtain the fitted value of gross investment. Data on replacement investment are also plotted in Figures 2a-2d. Despite the wide variability in levels of gross investment during the period 1931 and 1963, the fitted investment functions pro-

vide an accurate representation of actual investment behavior. In almost every series the largest observation is at least ten times the smallest, so that the goodness of fit of the investment functions provides much stronger confirmation for the underlying theory of investment behavior than functions fitted to postwar data alone.

III. *The Effects of Tax Policy on Investment Behavior*

The effects of a change in tax policy are: (1) an initial burst of net investment which brings the capital stock up to the new desired capital stock, (2) a permanent increase in gross investment resulting from replacement of a larger capital stock, and (3) a proportionate increase in net and gross investment caused by changes in other determinants of desired capital stock. To calculate the magnitudes of these effects for various alternative policies, we have assumed that tax policy has no effect on the before-tax rate of return or on the price of capital goods.

We present results for three actual changes and for one hypothetical change in tax policy: (1) the adoption of accelerated methods for computing depreciation for tax purposes in 1954, (2) the shortening of lifetimes for tax purposes allowed for equipment by the depreciation guidelines of 1962, (3) the investment tax credit of 1962, and (4) the hypothetical adoption of first-year writeoff in 1954. For each of the actual changes in tax policy our procedure is to calculate the rental price of capital on the assumption that the change in policy did not take place. We then calculate the changes in desired capital and investment for the resulting rental price of capital. Desired capital and investment depend on the parameters of the investment function; in our calculations, these parameters are replaced by the estimates given in Table 2 above. For the hypothetical first-year writeoff of investment expenditures beginning in 1954 our procedure is to calculate the rental price of capital under this policy. We then calculate the resulting changes in desired capital and investment from the fitted investment functions, as before.

The reductions in the rental on capital goods brought about in 1954 as a result of accelerated depreciation were as follows:

| | Before Change | After Change |
|--|------------------|-----------------|
| Manufacturing Equipment | .310 | .284 |
| Manufacturing Structures | .207 | .188 |
| Non-Farm, Non-Manufacturing Equipment | .375 | .344 |
| Non-Farm, Non-Manufacturing Structures | .218 | .198 |

Our estimates of the increase in net investment, gross investment, and capital stock resulting from this change are given in Table 3. For comparison the actual levels of net investment, gross investment, and capital stock are given in Table 4.

TABLE 3—CHANGE IN NET INVESTMENT, GROSS INVESTMENT, AND CAPITAL STOCK RESULTING FROM ACCELERATED DEPRECIATION, 1954-63
(billions of 1954 dollars)

| Year | Manufacturing Equipment | | | Manufacturing Structures | | | Non-Farm Non-Manufacturing Equipment | | | Non-Farm Non-Manufacturing Structures | | |
|------|-------------------------|------|-------|--------------------------|------|-------|--------------------------------------|-------|-------|---------------------------------------|-------|-------|
| | N | I | K | N | I | K | N | I | K | N | I | K |
| 1954 | .418 | .418 | 0 | .189 | .189 | 0 | 1.059 | 1.059 | 0 | 1.045 | 1.045 | 0 |
| 1955 | .680 | .742 | .418 | .434 | .446 | .189 | 1.214 | 1.417 | 1.059 | 1.246 | 1.319 | 1.045 |
| 1956 | .480 | .641 | 1.098 | .367 | .406 | .623 | .683 | 1.120 | 2.273 | 1.076 | 1.236 | 2.291 |
| 1957 | .305 | .537 | 1.579 | .258 | .320 | .990 | .386 | .954 | 2.957 | .955 | 1.189 | 3.368 |
| 1958 | .154 | .431 | 1.884 | .186 | .264 | 1.249 | .220 | .863 | 3.343 | .896 | 1.196 | 4.324 |
| 1959 | .124 | .423 | 2.038 | .169 | .259 | 1.436 | .202 | .887 | 3.564 | .954 | 1.316 | 5.220 |
| 1960 | .133 | .451 | 2.162 | .190 | .290 | 1.605 | .201 | .926 | 3.766 | .959 | 1.388 | 6.175 |
| 1961 | .089 | .427 | 2.296 | .154 | .266 | 1.796 | .193 | .956 | 3.968 | .904 | 1.399 | 7.134 |
| 1962 | .127 | .478 | 2.385 | .127 | .249 | 1.950 | .274 | 1.074 | 4.161 | .833 | 1.391 | 8.038 |
| 1963 | .179 | .549 | 2.513 | .125 | .255 | 2.078 | .288 | 1.141 | 4.435 | .765 | 1.381 | 8.872 |

TABLE 4—ACTUAL LEVELS OF NET INVESTMENT, GROSS INVESTMENT, AND CAPITAL STOCK, 1950-63
(billions of 1954 dollars)

| Year | Manufacturing Equipment | | | Manufacturing Structures | | | Non-Farm Non-Manufacturing Equipment | | | Non-Farm Non-Manufacturing Structures | | |
|------|-------------------------|-------|--------|--------------------------|-------|--------|--------------------------------------|--------|--------|---------------------------------------|--------|---------|
| | N | I | K | N | I | K | N | I | K | N | I | K |
| 1950 | 1.522 | 4.917 | 23.081 | .257 | 1.240 | 15.726 | 3.579 | 12.086 | 44.238 | 4.172 | 8.141 | 57.189 |
| 1951 | 1.985 | 5.604 | 24.603 | 1.101 | 2.100 | 15.983 | 3.301 | 12.496 | 47.817 | 3.925 | 8.183 | 61.361 |
| 1952 | 1.839 | 5.750 | 26.588 | 1.093 | 2.161 | 17.084 | 2.499 | 12.329 | 51.119 | 3.345 | 7.876 | 65.287 |
| 1953 | 1.914 | 6.096 | 28.428 | .974 | 2.110 | 18.178 | 3.041 | 13.352 | 53.618 | 4.539 | 9.302 | 68.633 |
| 1954 | 1.148 | 5.612 | 30.342 | .841 | 2.039 | 19.152 | 1.692 | 12.588 | 56.659 | 5.589 | 10.668 | 73.172 |
| 1955 | .961 | 5.593 | 31.491 | 1.504 | 2.754 | 19.994 | 3.061 | 14.282 | 58.352 | 7.135 | 12.601 | 78.761 |
| 1956 | 3.398 | 8.171 | 32.452 | 1.478 | 2.822 | 21.499 | 4.633 | 16.443 | 61.413 | 6.041 | 12.002 | 85.897 |
| 1957 | 3.159 | 8.432 | 35.850 | 1.682 | 3.119 | 22.978 | 3.829 | 16.530 | 66.047 | 5.739 | 12.119 | 91.938 |
| 1958 | .672 | 6.410 | 39.009 | .593 | 2.134 | 24.661 | .614 | 12.822 | 69.876 | 5.340 | 12.119 | 97.677 |
| 1959 | 1.013 | 6.850 | 39.682 | .391 | 1.969 | 25.255 | 1.396 | 14.715 | 69.261 | 6.047 | 13.196 | 103.010 |
| 1960 | 1.314 | 7.301 | 40.695 | 1.141 | 2.744 | 25.646 | 2.723 | 16.311 | 70.658 | 6.911 | 14.480 | 109.060 |
| 1961 | .921 | 7.101 | 42.010 | .980 | 2.654 | 26.788 | .840 | 14.951 | 73.382 | 6.585 | 14.634 | 115.970 |
| 1962 | 1.428 | 7.744 | 42.931 | .900 | 2.635 | 27.768 | 2.790 | 17.063 | 74.222 | 6.242 | 14.748 | 122.560 |
| 1963 | 1.935 | 8.461 | 44.360 | 1.057 | 2.849 | 28.668 | 3.172 | 17.982 | 77.012 | 5.878 | 14.817 | 128.800 |

The effects of the switch to accelerated methods for computing depreciation are quite dramatic. For each of the four classes of assets, the change in depreciation rules results in a substantial increase in desired capital stock. The effects of this increase depend on the time lag between changes in desired capital stock and the resulting net investment.

Although essentially the same pattern prevails for all four classes of assets, it is useful to trace out the effects of tax policy on net investment, gross investment, and capital stock for each class. The peak effect on net investment for manufacturing equipment is attained in 1955 with a level of \$.680 billion (in constant 1954 dollars) or 70.8 per cent of net investment in that year. By 1961, the increase in net investment has fallen to \$.089 billion. Over the whole period 16.9 per cent of the net investment in manufacturing equipment may be attributed to the change in depreciation rules. Similarly, the peak effect for non-farm, non-manufacturing equipment is \$1.214 billion in 1955, or 39.7 per cent of the net investment that took place in that year. Over the 1954-63 period 19.1 per cent of the net investment in non-farm, non-manufacturing equipment may be attributed to the change in depreciation rules. By 1961, the increase in net investment has fallen to \$.193 billion.

The pattern of net investment for structures is similar to that for equipment. For manufacturing structures the peak effect on net investment occurs in 1955 with \$.434 billion or 28.9 per cent of the net investment that took place in that year. The decline of net investment in structures is more gradual. By the end of the 1954-63 period the increase in net investment in manufacturing structures due to the change in depreciation rules in 1954 is still \$.125 billion. Over the whole period 20.8 per cent of net investment may be attributed to the change in depreciation policy. For non-farm, non-manufacturing structures the peak effect on net investment is in 1955 with \$1.246 billion or 17.5 per cent of the net investment that took place. This level falls off to \$.765 billion by 1963, the end of the ten-year period, 1954-63. Over the whole period 15.7 per cent of the net investment in non-farm, non-manufacturing structures may be attributed to the change in depreciation rules.

Since capital stock is simply a cumulation of net investment, the pattern of its behavior may be deduced from that for net investment. For both manufacturing and non-farm, non-manufacturing equipment capital stock rises rapidly over the levels that would have prevailed during the first few years following the adoption of accelerated depreciation methods. More than half of the increase over the period, 1954-63, had already occurred for manufacturing by 1957 and for non-farm, non-manufacturing by 1956. The rise in capital stock for structures is more gradual. Half of the total increase had occurred for manufacturing by 1958 and for non-farm, non-manufacturing by 1959.

Turning to the effects of accelerated depreciation on gross investment, we recall that gross investment is simply the sum of net investment and replacement and that replacement rises in proportion to capital stock. Replacement becomes the dominant component of gross investment in equipment by 1958 for manufacturing and by 1957 for non-farm, non-manufacturing. In both sectors gross investment rises to a peak in 1955 with net investment predominating. As net investment declines, replacement investment rises so that gross investment remains nearly stationary at levels somewhat below the 1955 peak. In manufacturing the increase in gross investment due to accelerated depreciation is \$.549 billion in 1963, which may be compared with the peak level of \$.742 billion in 1955. Similarly, in the non-farm, non-manufacturing sector the increase in gross investment due to accelerated depreciation is \$1.141 billion in 1963, compared with a peak of \$1.417 billion in 1955. The pattern in manufacturing structures is similar to that for equipment. The peak level of investment of \$.446 billion is attained in 1955; the 1963 level is \$.255 billion. For non-farm, non-manufacturing structures net investment continues at a high level throughout the period so that gross investment is roughly constant from 1959 to 1963, when the level is \$1.381 billion. This level may be compared with the relative peak of \$1.319 billion in 1955.

The effect of accelerated depreciation on gross investment during the 1954-63 period may be seen by calculating investment resulting from accelerated depreciation as a proportion of the total investment that took place. For equipment 7.1 per cent of gross investment in manufacturing and 6.8 per cent of the gross investment in non-farm, non-manufacturing may be attributed to accelerated depreciation over the period, 1954-63. For structures the percentages are 11.4 for manufacturing and 9.8 for non-farm, non-manufacturing. Another perspective on the effect of the depreciation rules may be obtained by calculating the proportion of gross investment resulting from the change to total investment at the end of the period. For manufacturing equipment 6.5 per cent of gross investment in 1963 is a result of accelerated depreciation; the corresponding percentage for non-farm, non-manufacturing equipment is 6.3 per cent. The effects of the change are more significant in structures. In 1963, 9.0 per cent of gross investment in manufacturing structures could be attributed to accelerated depreciation; similarly, in non-farm, non-manufacturing structures 9.3 per cent of gross investment could be attributed to the change in depreciation rules.

In 1962 new guidelines for the determination of lifetimes allowable for tax purposes were issued [23]. These guidelines involved a substantial reduction in equipment lifetimes allowable for tax purposes. The

reductions in the rental on capital goods which the change in guidelines brought about in 1962 were as follows:

| | Before Change | After Change |
|---------------------------------------|------------------|-----------------|
| Manufacturing Equipment | .273 | .267 |
| Non-Farm, Non-Manufacturing Equipment | .331 | .323 |

We have calculated the effects on net investment, gross investment, and capital stock resulting from the depreciation guidelines of 1962. These calculations give the increase in equipment investment over the levels that would have prevailed had lifetimes remained at their 1961 levels. The results are given in Table 5.

TABLE 5—CHANGE IN NET INVESTMENT, GROSS INVESTMENT, AND CAPITAL STOCK RESULTING FROM 1962 DEPRECIATION GUIDELINES AND THE INVESTMENT TAX CREDIT, 1962–63 (billions of 1954 dollars)

| Year | 1962 Depreciation Guidelines | | | | | | Investment Tax Credit | | | | | |
|------|------------------------------|------|------|--------------------------------------|------|------|-------------------------|------|------|--------------------------------------|-------|-------|
| | Manufacturing Equipment | | | Non-Farm Non-Manufacturing Equipment | | | Manufacturing Equipment | | | Non-Farm Non-Manufacturing Equipment | | |
| | N | I | K | N | I | K | N | I | K | N | I | K |
| 1962 | .185 | .185 | 0 | .504 | .504 | 0 | .509 | .509 | 0 | 1.388 | 1.388 | 0 |
| 1963 | .287 | .315 | .185 | .559 | .656 | .504 | .792 | .867 | .509 | 1.541 | 1.808 | 1.388 |

The impact of the revised guidelines is substantial, though not as dramatic as the shift to accelerated methods of depreciation in the Internal Revenue Code of 1954. The impact is limited to equipment, whereas the effects of accelerated depreciation were much greater for structures than for equipment. The peak response to the new guidelines, occurring in 1963, is less than half the peak response of investment in equipment to the switch to accelerated depreciation. In percentage terms 14.8 per cent of the net investment in manufacturing equipment in 1963 is due to the change in guidelines; 17.6 per cent of the net investment in non-farm, non-manufacturing equipment is due to the change. The impact on gross investment is proportionately smaller. In 1963 only 3.7 per cent of gross investment in manufacturing equipment is due to the new guidelines; 3.6 per cent of investment in non-farm, non-manufacturing equipment could be attributed to the revised lifetimes.

A second change in tax policy during 1962 was the adoption of a seven per cent investment tax credit for machinery and equipment in the Revenue Act of 1962.¹⁹ Seven per cent of the value of purchases of new

¹⁹ Actually, limitations making the tax credit inapplicable to very short-lived assets reduce its effective rate to about 6.6 per cent. In 1964 its effective rate was raised to around ten per cent by allowing depreciation to be taken on the cost before rather than after the tax credit.

plant and equipment is a credit against tax liability. The depreciation base is reduced by the amount of the tax credit. The remaining 93 per cent is then amortized over the lifetime of the equipment. The reductions in the rental on capital goods which the investment tax credit brought about in 1962 were as follows:

| | Before Change | After Change |
|---------------------------------------|------------------|-----------------|
| Manufacturing equipment | .286 | .267 |
| Non-farm, non-manufacturing equipment | .346 | .323 |

To isolate the effects of the investment tax credit, we have calculated the resulting net investment, gross investment, and capital stock. These calculations give the increase in investment over levels that would have prevailed in the absence of the investment tax credit. This increase is given in Table 5.

The effects of a 7 per cent investment tax credit are quite startling. Although the impact is limited to equipment, the peak response of net investment to the tax credit, occurring in 1963, is greater for both manufacturing and non-farm, non-manufacturing than the response to accelerated methods of depreciation. Fully 40.9 per cent of the net investment in manufacturing equipment in 1963 can be attributed to the investment tax credit. The corresponding percentage for non-farm, non-manufacturing equipment is 48.6 per cent. Of course, the impact of the investment tax credit on gross investment is less startling, but this impact is also quite dramatic. Of the total of \$8.461 billion of investment in manufacturing equipment in 1963, 10.2 per cent can be traced to the effects of the investment tax credit. Similarly, of \$17.982 billion of investment in non-farm, non-manufacturing equipment in the same year, 10.1 per cent can be attributed to the investment tax credit. There can be little doubt that an investment tax credit is a potent stimulus to investment expenditure.

The progressive liberalization of depreciation for tax purposes since 1954 has had an important impact on investment behavior. The investment booms of 1955-57 and beginning in 1962 reflect, in part, the response of investment behavior to the changes in tax policy that took place in 1954 and 1962. According to our calculations, the adoption of accelerated depreciation in the Internal Revenue Code of 1954 resulted in a shift in the composition of investment from equipment to structures. Similarly, the adoption in 1962 of new guidelines and the investment tax credit resulted in a shift of investment from structures to equipment. This shift was especially dramatic for the response to the investment tax credit.

The magnitude of the past response to liberalization of depreciation suggests an investigation of the response to further liberalization. To

take an extreme assumption we can investigate the pattern of investment that would result from complete "expensing" or first-year writeoff of investment. Under such a tax policy expenditures on capital account would be treated in the same way as expenditures on current account. As Vernon Smith [17] has demonstrated, the effects of this policy are the same as the effects of no taxation of business income.²⁰ The reductions in the rental on capital goods which first-year writeoff would have brought about in 1954 are as follows:

| | Before Change | After Change |
|--|------------------|-----------------|
| Manufacturing Equipment | .284 | .214 |
| Manufacturing Structures | .188 | .130 |
| Non-Farm, Non-Manufacturing Equipment | .344 | .260 |
| Non-Farm, Non-Manufacturing Structures | .198 | .137 |

We have calculated the effects on net investment, gross investment, and capital stock resulting from the hypothetical adoption of first-year writeoff in 1954. The changes represent the increment in investment and capital stock over the levels that resulted from accelerated depreciation. The increases in net investment, gross investment, and capital stock are presented in Table 6.

The adoption of first-year writeoff for investment expenditures in 1954 would have resulted in a sharp rise in desired capital for all four classes of assets. The effect of this rise on investment is relatively rapid for equipment; for manufacturing net investment in equipment for 1955 would have been over twice as large as a result of first-year writeoff. The relative increase in equipment investment in the non-farm, non-manufacturing sector would have been somewhat smaller. The increase in net investment in equipment for both sectors would have returned to relatively moderate levels by the beginning of the 1960's. The response is much more gradual for structures than for equipment. The increase in net investment in structures would have remained at substantial levels throughout the 1950's and 1960's. The effects of adoption of first-year writeoff in 1954 on gross investment in both equipment and structures would have been substantial throughout the period 1954-63. By the end of the period the chief effect of this policy would have been an increased level of replacement investment.

IV. Conclusion

We have calculated the effects of changes in tax policy on investment behavior for three major tax revisions in the postwar period: (1) the adoption of accelerated methods for calculating depreciation in the Internal Revenue Code of 1954; (2) the reduction of lifetimes used for calculating depreciation on equipment and machinery in 1962; (3) the

²⁰ See also R. A. Musgrave [16].

TABLE 6—CHANGE IN NET INVESTMENT, GROSS INVESTMENT, AND CAPITAL STOCK RESULTING FROM HYPOTHETICAL ADOPTION OF FIRST-YEAR WRITEOFF OF INVESTMENT EXPENDITURES, 1954-63
(billions of 1954 dollars)

| Year | Manufacturing Equipment | | | Manufacturing Structures | | | Non-Farm Non-Manufacturing Equipment | | | Non-Farm Non-Manufacturing Structures | | |
|------|-------------------------|-------|-------|--------------------------|-------|-------|--------------------------------------|-------|--------|---------------------------------------|-------|--------|
| | N | I | K | N | I | K | N | I | K | N | I | K |
| 1954 | 1.606 | 1.606 | 0 | .937 | .937 | 0 | 4.071 | 4.071 | 0 | 5.168 | 5.168 | 0 |
| 1955 | 2.555 | 2.791 | 1.606 | 2.085 | 2.144 | .937 | 4.519 | 5.302 | 4.071 | 5.842 | 6.201 | 5.168 |
| 1956 | 1.692 | 2.304 | 4.161 | 1.633 | 1.822 | 3.023 | 2.310 | 3.962 | 8.591 | 4.691 | 5.455 | 11.011 |
| 1957 | 1.023 | 1.884 | 5.853 | 1.072 | 1.363 | 4.657 | 1.233 | 3.329 | 10.901 | 4.106 | 5.196 | 15.702 |
| 1958 | .508 | 1.520 | 6.877 | .769 | 1.127 | 5.730 | .719 | 3.053 | 12.134 | 3.858 | 5.233 | 19.809 |
| 1959 | .417 | 1.504 | 7.385 | .706 | 1.112 | 6.499 | .691 | 3.163 | 12.854 | 4.126 | 5.768 | 23.668 |
| 1960 | .463 | 1.611 | 7.803 | .807 | 1.258 | 7.205 | .708 | 3.313 | 13.546 | 4.157 | 6.086 | 27.794 |
| 1961 | .311 | 1.527 | 8.267 | .656 | 1.157 | 8.013 | .685 | 3.426 | 14.254 | 3.921 | 6.138 | 31.952 |
| 1962 | .328 | 1.590 | 8.578 | .544 | 1.086 | 8.670 | .642 | 3.515 | 14.940 | 3.617 | 6.107 | 35.873 |
| 1963 | .447 | 1.757 | 8.906 | .538 | 1.114 | 9.214 | .656 | 3.652 | 15.582 | 3.325 | 6.066 | 39.491 |

investment tax credit for equipment and machinery in the Revenue Act of 1962. The effects of accelerated depreciation are very substantial, especially for investment in structures. The effects of the depreciation guidelines of 1962 are significant, but these effects are confined to investment in equipment. The effects of the investment tax credit of 1962 are quite dramatic and leave little room for doubt about the efficacy of tax policy in influencing investment behavior. These three tax policies represent a progressive liberalization of depreciation for tax purposes. To get some idea of the effects of further liberalization we have calculated the impact of the adoption of first-year writeoff of investment expenditures beginning in 1954. This tax policy represents the ultimate liberalization since it is equivalent to treating capital expenditures in the same way as current expenditures for tax purposes. The effects of such a policy on investment expenditure would have been very substantial throughout the period, 1954-63.

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MACROECONOMICS OF UNBALANCED GROWTH: THE ANATOMY OF URBAN CRISIS*

By WILLIAM J. BAUMOL

There are some economic forces so powerful that they constantly break through all barriers erected for their suppression. Such, for example, are the forces of supply and demand which have resisted alike medieval efforts to abolish usury and contemporary attempts to control prices. In this paper I discuss what I believe to be another such mechanism which has colored the past and seems likely to stamp its character on the future. It helps us to understand the prospective roles of a wide variety of economic services: municipal government, education, the performing arts, restaurants, and leisure time activity. I will argue that inherent in the technological structure of each of these activities are forces working almost unavoidably for progressive and cumulative increases in the real costs incurred in supplying them. As a consequence, efforts to offset these cost increases, while they may succeed temporarily, in the long run are merely palliatives which can have no significant effect on the underlying trends.

The justification of a macroeconomic model should reside primarily in its ability to provide insights into the workings of observed phenomena. Its aggregation of diverse variables usually deny it the elegance and the rigor that are provided by microeconomic analysis at its best. Yet macro models have succeeded in explaining the structure of practical problems and in offering guidance for policy to a degree that has so far eluded the more painstaking modes of economic analysis. This article hopes to follow in the tradition—the structure of its basic model is rudimentary. Yet it can perhaps shed some light on a variety of economic problems of our generation.

1. *Premises*

Our model will proceed on several assumptions, only one of which is really essential. This basic premise asserts that economic activities can, not entirely arbitrarily, be grouped into two types: technologically progressive activities in which innovations, capital accumulation, and economies of large scale all make for a cumulative rise in output per

* The author is professor of economics, Princeton University. He wishes to thank the Ford Foundation and the National Science Foundation whose grants greatly facilitated completion of this paper. The basic hypothesis was originally developed jointly with William G. Bowen in the study of the performing arts conducted for the Twentieth Century Fund. In this paper there is no attempt to document the illustrative empirical observations though in every case the data have been examined and appear to support the assertions

man hour and activities which, by their very nature, permit only sporadic increases in productivity.

Of course, one would expect that productivity would not grow at a uniform rate throughout the economy so it is hardly surprising that, given any arbitrarily chosen dividing line, one can fit all goods and services into one or the other of two such categories in whatever way the dividing line is drawn. I am, however, making a much stronger assertion: that the place of any particular activity in this classification is not primarily a fortuitous matter determined by the particulars of its history, but rather that it is a manifestation of the activity's technological structure, which determines quite definitely whether the productivity of its labor inputs will grow slowly or rapidly.

The basic source of differentiation resides in the role played by labor in the activity. In some cases labor is primarily an instrument—an incidental requisite for the attainment of the final product, while in other fields of endeavor, for all practical purposes the labor is itself the end product. Manufacturing encompasses the most obvious examples of the former type of activity. When someone purchases an air conditioner he neither knows nor cares how much labor went into it. He is not concerned one way or the other with an innovation that reduces the manpower requirements for the production of his purchase by 10 per cent if the price and the quality of the product are unaffected. Thus it has been possible, as it were, behind the scenes, to effect successive and cumulative decreases in the labor input coefficient for most manufactured goods, often along with some degree of improvement in the quality of the product.

On the other hand there are a number of services in which the labor is an end in itself, in which quality is judged directly in terms of amount of labor. Teaching is a clear-cut example, where class size (number of teaching hours expended per student) is often taken as a critical index of quality. Here, despite the invention of teaching machines and the use of closed circuit television and a variety of other innovations, there still seem to be fairly firm limits to class size. We are deeply concerned when elementary school classes grow to 50 pupils and are disquieted by the idea of college lectures attended by 2000 underclassmen. Without a complete revolution in our approach to teaching there is no prospect that we can ever go beyond these levels (or even up to them) with any degree of equanimity. An even more extreme example is one I have offered in another context: live performance. A half hour horn quintet calls for the expenditure of $2\frac{1}{2}$ man hours in its performance, and any attempt to increase productivity here is likely to be viewed with concern by critics and audience alike.

The difference between the two types of activity in the flexibility of their productivity levels should not be exaggerated. It is a matter of

degree rather than an absolute dichotomy. The jet airplane has increased the productivity per man hour of a faculty member who is going from New York to California to give a lecture. Certainly the mass media have created what may be considered a new set of products that are close substitutes for live performance and by which productivity was increased spectacularly. In addition, there are, as the reader will recognize, all sorts of intermediate activities which fall between the two more extreme varieties. Yet, the distinction between the relatively constant productivity industries and those in which productivity can and does rise is a very real one, and one which, we shall see, is of considerable practical importance.

In addition to the separability of activities into our two basic categories I shall utilize three other assumptions, two of them primarily for ease of exposition. The reader will recognize, as we proceed, that neither is essential to the argument. The first of the incidental premises consists simply in the assertion that all outlays other than labor costs can be ignored. This assertion is patently unrealistic but it simplifies greatly our mathematical model. A second, far more important, and more realistic assumption is that wages in the two sectors of the economy go up and down together. In the long run there is some degree of mobility in all labor markets and consequently, while wages in one activity can lag behind those in another, unless the former is in process of disappearing altogether we cannot expect the disparity to continue indefinitely. For simplicity I will in the next section take hourly wages to be precisely the same in both sectors, but the model is easily complicated to allow for some diversity in wage levels and their movements.

A final inessential assumption which is, however, not altogether unrealistic, asserts that money wages will rise as rapidly as output per man hour in the sector where productivity is increasing. Since organized labor is not slow to learn of increases in its productivity it is likely to adjust its wage demands accordingly. This assumption affects only the magnitude of the absolute price level in our model, and does not influence the relative costs and prices that are the critical elements in the analysis.

2. *A Model of Unbalanced Expansion*

Assume that the economy is divided into two sectors, sector one, in which the productivity of labor is constant, while in sector two output per man hour grows cumulatively at a constant compounded rate, r . Thus we have for the respective values of outputs Y_{1t} and Y_{2t} in the two sectors at time t :

$$(1) \qquad Y_{1t} = aL_{1t}$$

$$(2) \qquad Y_{2t} = bL_{2t}e^{rt}$$

where L_{1t} and L_{2t} are the quantities of labor employed in the two sectors and a and b are constants.

We suppose wages are equal in the two sectors and are fixed at W_t dollars per unit of labor, where W_t itself grows in accord with the productivity of sector 2, our "progressive" sector, so that

$$(3) \quad W_t = We^{rt}. \quad (W = \text{some constant})$$

We may now derive several properties of such a system. First and most fundamental is *Proposition 1*: The cost per unit of output of sector 1, C_1 , will rise without limit while C_2 , the unit cost of sector 2, will remain constant.
proof:

$$C_1 = W_t L_{1t} / Y_{1t} = We^{rt} L_{1t} / a L_{1t} = We^{rt} / a$$

$$C_2 = W_t L_{2t} / Y_{2t} = We^{rt} L_{2t} / b L_{2t} e^{rt} = W / b.$$

Note that the *relative* costs will behave in this manner whether or not wages increase in accord with (3) for we have

$$C_1 / C_2 = (L_{1t} / Y_{1t}) / (L_{2t} / Y_{2t}) = be^{rt} / a.$$

In practice, we would expect in these circumstances that market demand for the output of sector 1 would decline. Suppose, for example, the elasticity of demand for the two outputs were unity in terms of prices which were proportionate to costs. Then relative outlays on the two commodities would remain constant, i.e., we would have

$$\frac{C_1 Y_1}{C_2 Y_2} = \frac{We^{rt} L_{1t}}{We^{rt} L_{2t}} = \frac{L_{1t}}{L_{2t}} = A(\text{constant}).$$

Hence the output ratio of the two sectors would be given by

$$Y_1 / Y_2 = a L_{1t} / b L_{2t} e^{rt} = aA / be^{rt}$$

which declines toward zero with the passage of time. Thus we have *Proposition 2*: In the model of unbalanced productivity there is a tendency for the outputs of the "nonprogressive" sector whose demands are not highly inelastic to decline and perhaps, ultimately, to vanish.

We may inquire, however, what would happen if despite the change in their relative costs and prices the magnitude of the relative outputs of the two sectors were maintained, perhaps with the aid of government subsidy, or if demand for the product in question were sufficiently price inelastic or income elastic. Then we would have

$$(b/a) Y_1 / Y_2 = L_1 / L_2 e^{rt} = K.$$

Let $L = L_1 + L_2$ be the total labor supply. It follows that

$$(4) \quad L_1 = (L - L_1)Ke^{rt} \quad \text{or} \quad L_1 = LKe^{rt} / (1 + Ke^{rt})$$

and

$$(5) \quad L_2 = L - L_1 = L/(1 + Ke^{rt}).$$

Hence, as t approaches infinity, L_1 will approach L and L_2 will approach zero. Thus we have *Proposition 3*: In the unbalanced productivity model, if the ratio of the outputs of the two sectors is held constant, more and more of the total labor force must be transferred to the non-progressive sector and the amount of labor in the other sector will tend to approach zero.

Finally, we may note what happens to the overall rate of growth of output in the economy if the output ratio for the two sectors is not permitted to change. We may take as an index of output a weighted average of the outputs of the two sectors:

$$I = B_1Y_1 + B_2Y_2 = B_1aL_1 + B_2bL_2e^{rt}$$

so that by (4) and (5)

$$I = L(KB_1a + B_2b)e^{rt}/(1 + Ke^{rt}) = Re^{rt}/(1 + Ke^{rt})$$

where

$$R = L(KB_1a + B_2b).$$

Therefore

$$\begin{aligned} dI/dt &= R[re^{rt}(1 + Ke^{rt}) - Kre^{2rt}]/(1 + Ke^{rt})^2 \\ &= rRe^{rt}/(1 + Ke^{rt})^2. \end{aligned}$$

As a result, the percentage rate of growth of output will be

$$(dI/dt)/I = r/(1 + Ke^{rt})$$

which declines asymptotically toward zero as t increases. We have, then, arrived at *Proposition 4*: An attempt to achieve balanced growth in a world of unbalanced productivity must lead to a declining rate of growth relative to the rate of growth of the labor force. In particular, if productivity in one sector and the total labor force remain constant the growth rate of the economy will asymptotically approach zero.

3. Discussion of the Propositions

The logic of the entire analysis can be restated rather simply in intuitive terms. If productivity per man hour rises cumulatively in one sector relative to its rate of growth elsewhere in the economy, while wages rise commensurately in all areas, then relative costs in the nonprogressive sectors must inevitably rise, *and these costs will rise cumulatively and without limit*. For while in the progressive sector productivity increases will serve as an offset to rising wages, this offset must be smaller in the

nonprogressive sectors. For example (ignoring nonwage costs) if wages and productivity in the progressive sector both go up 2 per cent per year, costs there will not rise at all. On the other hand, if in the nonprogressive sector productivity is constant, every rise in wages must yield a corresponding addition to costs—a two per cent cumulative rise in wages means that, year in year out, costs must be two per cent above those of the preceding year. Thus, the very progress of the technologically progressive sectors inevitably adds to the costs of the technologically unchanging sectors of the economy, unless somehow the labor markets in these areas can be sealed off and wages held absolutely constant, a most unlikely possibility.

We see then that costs in many sectors of the economy will rise relentlessly, and will do so for reasons that are for all practical purposes beyond the control of those involved. The consequence is that the outputs of these sectors may in some cases tend to be driven from the market. If their relative outputs are maintained, an ever increasing proportion of the labor force must be channeled into these activities and the rate of growth of the economy must be slowed correspondingly.

4. *Some Applications*¹

These observations can be used at once to explain a number of observed phenomena. For example, there is evidence that an ever increasing portion of the nation's labor force has been going into retailing and that a rising portion of the cost of commodities is accounted for by outlays on marketing. Now there have been several pronounced changes in the technology of marketing in recent decades: self service, the supermarket, and prewrapping have all increased the productivity per man hour of the retailing personnel. But ultimately, the activity involved is in the nature of a service and it does not allow for constant and cumulative increases in productivity through capital accumulation, innovation, or economies of large-scale operation. Hence it is neither mismanagement nor lack of ingenuity that accounts for the relatively constant productivity of this sector. Since some sort of marketing effort is an inescapable element in economic activity, demand for this service is quite income elastic. Our model tells us what to expect in this case—cumulatively increasing costs relative to those of other economic activities, and the absorption of an ever growing proportion of society's resources by this sector—precisely what seems to have been observed.

Higher education is another activity the demand for whose product seems to be relatively income elastic and price inelastic. Higher tuition charges undoubtedly impose serious hardships on lower-income stu-

¹ Some of the ideas in this section arose out of discussions with Eugene Beem of Sperry and Hutchinson.

dents. But, because a college degree seems increasingly to be a necessary condition for employment in a variety of attractive occupations, most families have apparently been prepared to pay the ever larger fees instituted in recent years. As a result higher education has been absorbing a constantly increasing proportion of per capita income. And the relatively constant productivity of college teaching leads our model to predict that rising educational costs are no temporary phenomenon—that they are not a resultant of wartime inflation which will vanish once faculty salaries are restored to their prewar levels. Rather, it suggests that, as productivity in the remainder of the economy continues to increase, costs of running the educational organizations will mount correspondingly, so that whatever the magnitude of the funds they need today, we can be reasonably certain that they will require more tomorrow, and even more on the day after that.

But not all services in the relatively constant productivity sector of the economy face inelastic demands. Many of them are more readily dispensable than retailing and education as far as individual consumers are concerned. As their costs increase, their utilization tends therefore to decrease and they retreat into the category of luxury goods with very limited markets or disappear almost completely. Fine pottery and glassware produced by the careful labor of skilled craftsmen sell at astronomical prices, though I am told the firms that produce them earn relatively little profit from these product lines which they turn out primarily for prestige and publicity, obtaining the bulk of their earnings from their mass production activities. Fine restaurants and theaters are forced to keep raising their prices, and at least in the case of the latter we know that volume is dwindling while it becomes ever more difficult for suppliers (the producers) to make ends meet.

An extreme example of an activity that has virtually disappeared is the construction (and, indeed, the utilization) of the large and stately houses whose operation even more than their construction allows for little in the way of enhanced productivity, and whose rising costs of operation have apparently decreased their salability even to the wealthy.

These observations suggest something about the likely shape of our economy in the future. Our model tells us that manufactures are likely to continue to decline in relative cost and, unless the income elasticity of demand for manufactured goods is very large, they may absorb an ever smaller proportion of the labor force, which, if it transpires, may make it more difficult for our economy to maintain its overall rate of output growth.

The analysis also suggests that real cost in the “nonprogressive” sectors of the economy may be expected to go on increasing. Some of the

services involved—those whose demands are inelastic—may continue viable on the free market. Some, like the theater, may be forced to leave this market and may have to depend on voluntary public support for their survival. Our hospitals, our institutions of private education and a variety of other nonprofit organizations have already long survived on this basis, and can continue to do so if the magnitude of contributions keeps up with costs. Some activities will either disappear or retreat to a small scale of operation catering primarily to a luxury trade. This fate may be in store for restaurants offering true *haute cuisine* and it is already the case for fine hand-worked furniture and for clothes made to measure. Some activities, perhaps many of the preceding among them, will fall increasingly into the hands of the amateurs who already play a considerable role in theatrical and orchestral performances, in gastronomy, in crafts such as woodworking and pottery. Finally, there is a considerable segment of nonprogressive activity that is dependent on tax support. Some of the problems that go with this position will be considered in the remainder of this paper.

In all the observations of this section there is one implicit underlying danger that should not escape the reader: the inherent threat to quality. Amateur activity has its virtues, as an educational device, as a good use for leisure time and so forth. But in a variety of fields it offers a highly imperfect substitute for the highly polished product that can be supplied by the professional. Unbalanced productivity growth, then, threatens to destroy many of the activities that do so much to enrich our existence, and to give others over into the hands of the amateurs. These are dangers which many of us may feel should not be ignored or taken lightly.

5. *On the Financial Problem of the Cities*

One of the major economic problems of our times is the crisis of the larger cities. Together with their suburban periphery the cities are attracting ever greater segments of our population. Yet at least the core of the metropolis is plagued by a variety of ills including spreading blight as entire neighborhoods deteriorate, increasing pollution of its atmosphere, worsening traffic, critical educational problems, and, above all, mounting fiscal pressures. The financial troubles are perhaps central to the entire issue because without adequate funds one cannot hope to mount an effective attack on the other difficulties. More than one reform mayor has taken office determined to undertake a radical program to deal with the city's difficulties and found himself baffled and stymied by the monstrous deficit which he discovered to be hanging over him, a deficit whose source appeared to have no reasonable explanation. There seems in these cases to be no way to account for the growth in the city's financial needs—for the fact that a municipal budget far above that

which was roughly adequate a decade earlier threatens to disrupt seriously the city's most vital services today. Where the political process is involved it is easy to blame growing costs on inefficiency and corruption but when they take office, reform administrations seem consistently puzzled by their inability to wring out the funds they require through the elimination of these abuses.

A critical element in the explanation becomes clear when we recognize how large a proportion of the services provided by the city are activities falling in the relatively nonprogressive sector of the economy. The bulk of our municipal expenditures is devoted to education which, as we have already seen, offers very limited scope for cumulative increases in productivity. The same is true of police, of hospitals, of social services, and of a variety of inspection services. Despite the use of the computer in medicine and in traffic planning, despite the use of closed circuit television and a variety of other devices, there is no substitute for the personal attention of a physician or the presence of a police patrol in a crime-ridden neighborhood. The bulk of municipal services is, in fact, of this general stamp and our model tells us clearly what can be expected as a result. Since there is no reason to anticipate a cessation of capital accumulation or innovation in the progressive sectors of the economy, the upward trend in the real costs of municipal services cannot be expected to halt; inexorably and cumulatively, whether or not there is inflation, administrative mismanagement or malfeasance, municipal budgets will almost certainly continue to mount in the future, just as they have been doing in the past. This is a trend for which no man and no group should be blamed, for there is nothing that can be done to stop it.

6. *The Role of Static Externalities*

Though these may be troubles enough for the municipal administrator, there are other compelling forces that plague him simultaneously. Among them are the general class of externality problems which have so long been the welfare economist's stock in trade.

Since the appearance of Marshall's and Pigou's basic writing in the area a most significant development has been the growing impact of external costs on urban living. No longer are road crowding and smoke nuisance only quaint cases serving primarily as textbook illustrations. Rather, they have become pressing issues of public concern—matters discussed heatedly in the daily press and accorded serious attention by practical politicians. Newspapers devote headlines to an engineer's prediction that the human race is more likely to succumb to its own pollutants than through a nuclear holocaust, and report with glee the quip that Los Angeles is the city in which one is wakened by the sound of birds coughing.

Now there are undoubtedly many reasons for the explosion in external

costs but there is a pertinent observation about the relationship between population size in a given area and the cost of externalities that seems not to be obvious. It is easy to assume that these costs will rise roughly in proportion with population but I shall argue now that a much more natural premise is that they will rise more rapidly—perhaps roughly as the square of the number of inhabitants. For example, consider the amount of dirt that falls into the house of a typical urban resident as a result of air pollution, and suppose that this is equal to kn where n is the number of residents in the area. Since the number of homes in the area, an , is also roughly proportionate to population size, total domestic soot-fall will be equal to soot per home times number of homes $= kn \cdot an = akn^2$. Similarly, if delays on a crowded road are roughly proportionate to n , the number of vehicles traversing it, the total number of man hours lost thereby will increase roughly as n^2 , since the number of passengers also grows roughly as the number of cars. The logic of the argument is simple and perhaps rather general: if each inhabitant in an area imposes external costs on every other, and if the magnitude of the costs borne by each individual is roughly proportionate to population size (density) then since these costs are borne by each of the n persons involved, the total external costs will vary not in proportion with n but with n^2 . Of course I do not maintain that such a relationship is universal or even that it is ever satisfied more than approximately. Rather I am suggesting that, typically, increases in population size may plausibly be expected to produce disproportionate increases in external costs—thus pressures on the municipality to do something about these costs may then grow correspondingly.

7. Cumulative Decay and Dynamic Pareto Optimality

Economic theory indicates yet another source of mounting urban problems. These are the processes of cumulative urban decay which once set in motion induce matters to go from bad to worse. Since I have discussed these elsewhere I can illustrate the central proposition rather briefly. Public transportation is an important example. In many urban areas with declining utilization, frequency of service has been sharply reduced and fares have been increased. But these price rises have only served to produce a further decline in traffic, leading in turn to yet another deterioration in schedules and another fare increase and so on, apparently *ad infinitum*. More important, perhaps, is the logic of the continued flight to the suburbs in which many persons who apparently would otherwise wish to remain in the city are driven out by growing urban deterioration—rising crime rates, a growing number of blighted neighborhoods, etc. Once again, the individuals' remedy intensifies the community's problems and each feeds upon the other. Those who leave

the city are usually the very persons who care and can afford to care—the ones who maintain their houses, who do not commit crimes, and who are most capable of providing the taxes needed to arrest the process of urban decay. Their exodus therefore leads to further deterioration in urban conditions and so induces yet another wave of emigration, and so on.²

It is clear that these cumulative processes can greatly increase the financial pressures besetting a municipality and can do so in a variety of ways: they can increase directly municipal costs by adding to the real quantities of inputs required for the upkeep of buildings, to maintain levels of urban sanitation, to preserve the level of education attained by an average resident, etc.; they can reduce the tax base—the exodus of more affluent urban inhabitants cause a decline in the financial resources available to the city; and with the passage of time the magnitude of the resources necessary to arrest and reverse the cumulative processes itself is likely to grow so that the city may find it increasingly difficult to go beyond programs that slow the processes slightly.³

² As is to be expected, such dynamic processes may be monotonic or oscillatory, stable or unstable. As examples we might expect the public transportation scheduling problem to grow monotonically while automotive road crowding may be inherently oscillatory. More specifically, let f represent frequency of public bus departure and p be the number of bus passengers. We would expect the relevant relationships to be something such as

$$\begin{aligned} p_t &= a + bf_t && \text{(demand for public transport) and} \\ f_{t+1} &= w + vp_t && \text{(lagged public transport supply response)} \end{aligned}$$

so that

$$f_{t+1} = w + av + vbf_t$$

in which the coefficient vb is presumably positive. On the other hand, if D represents average delay on some road and A is the volume of automotive traffic, the corresponding relationships would be

$$\begin{aligned} D_t &= a' + b'A_t && \text{(delay grows with traffic volume) and} \\ A_{t+1} &= w' - v'D_t && \text{(delays today lead to lower traffic tomorrow)} \end{aligned}$$

so that we have a negative coefficient $-b'v'$ in the difference equation

$$A_{t+1} = w' - a'v' - b'v'A_t$$

Since monotonic and oscillatory time paths correspond respectively to positive and negative variable coefficients in a first-order difference equation, our result follows. An intuitive explanation is fairly simple in each case: increased automotive traffic today causes delays and so may reduce traffic tomorrow. But a decline in number of passengers which causes a deterioration in service leads to still another fall in volume.

As stated, the time path in each case may be stable. But in the latter case, even though the declines will not be unbounded, the limit toward which they tend may be very low and totally unacceptable as a matter of public policy.

³ I have argued that the cumulative processes involve what may be considered dynamic externalities. Each passenger who uses public transportation less frequently imposes the increased likelihood of poorer schedules not only on himself but on others as well. As a result these processes will yield results that do not maximize social welfare. For the private and social marginal rates of transformation between present and future will then differ from one another. The individual will tend to cut down on his use of public transportation by an amount greater

8. *Conclusion—The Financial Problems of the Large City*

The story is perhaps completed if we add to the preceding observations the fact that each city is in competition with others and with its own surrounding areas for industry and for people with the wherewithal to pay taxes. No city government acting alone can afford to raise its tax rates indefinitely. Even if they were politically feasible, mounting tax rates must eventually produce diminishing and perhaps even negative returns as they depress the tax base further.

We can now quickly pull the pieces of our story together. We have just seen that our municipalities are perhaps unavoidably subject to a variety of growing financial pressures: the limited sources of tax funds, the pressures imposed by several processes of cumulative decay, the costs of externalities which seem to have a built-in tendency to rise more rapidly than the population. These phenomena imply that the activities of the municipality will have to be expanded if standards of city life are to be maintained. But the funds available for the purpose are extremely limited. And over all this hangs the shadow cast by our model of unbalanced growth which has shown that the costs of even a constant level of activity on the part of a municipal government can be expected to grow constantly higher.

The picture that has been painted is bleak. It suggests strongly that self-help offers no way out for our cities. All of this would then appear to offer stronger theoretical support for the Heller-Pechman proposal that the federal government can provide the resources necessary to prevent the serious crisis that threatens our larger urban communities and whose effects on the quality of life in our society may become one of the nation's most serious economic problems.

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than that which is optimal because he himself does not bear all of the costs of his action. There is a marginal rate of transformation between the utility derived from public transportation today and that obtainable from transportation tomorrow. If relative prices do not equal that marginal rate of transformation a misallocation of resources is likely to result. The consequences may even be what might be called Pareto-nonoptimal. That is, everyone may be harmed. For example, when automobile traffic becomes sufficiently bad it may become clear that everyone will be better off if passenger cars are banned completely from the downtown area in order to make possible a faster, more efficient public transportation system.

THE ECONOMICS OF TECHNOLOGICAL CHANGE AND THE DEMISE OF THE SHARECROPPER

By RICHARD H. DAY*

Ten years ago, at the University of Oxford, a lecturer on political economy laid it down as axiomatic that science and invention, the division of labor, the law of diminishing returns, could do little to save human labor on the farm.

ELLIS AND RUMELY: *Power and the Plow* (1911)

The economic history of a region is determined by a complicated interaction among geological, biological, technological, social, and economic forces. A vivid portrayal of this process is found in the recent history of the rural American south; the resulting interplay of economic and social movements has been displayed there with irony and violence. Beginning gradually in the late 1930s, the adoption of labor saving technology increased rapidly through the late 1940s and early 1950s. In some cases, the diffusion of a new technique grew by more than 100 per cent per annum.¹ Greatly lowered physical labor coefficients of new techniques created relatively profitable investment opportunities by substituting capital intensive, low variable cost methods for labor intensive, high variable cost methods of production.

A similar process was also at work in other parts of the United States. From 1940 to 1960 the index of man hours of farm work dropped from 191 to 92 for the United States as a whole and in the Delta States of Alabama, Louisiana, and Mississippi from 247 to 93. On the other hand, output per man hour in the production of cotton

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¹Examples were the adoption of highly mechanized rice cultivation methods and self-propelled combines. See Day [3, p. 92].

alone increased more than threefold from an index of 36 to one of 127 [11, p. 46] [4] [9].

The human counterpart of these technical facts was an exodus of 17 million people from U.S. farms. In Mississippi, where the concentration of population in agriculture was much higher than for the nation as a whole, almost a million people left agriculture—a decline of 62 per cent in two decades. In the ten counties of the Mississippi Delta the decline in the rural farm population was also 62 per cent—a drop of 54 per cent occurring from 1950 to 1960 alone.²

During these years agricultural economists focussed on problems of agricultural surplus and policies of control. At a time when the rest of the economy sluggishly ignored the growing influx of displaced agricultural workers, economists and popular commentators ironically suggested policies that would move resources out of agriculture even faster.

In accordance with the policy concerns of that time I attempted to estimate the influence of technology on supplies of agricultural commodities. A “recursive programming model” was developed and applied to a small but more or less representative southern area. But it is evident that this dynamic model of production and technology is equally a dynamic model of resource utilization and labor demand. Moreover, in the shadow of the labor requirements generated by the model moves the history of changing farm organization and emigration. This side of the story received little attention in my initial report.³ Because of its current relevance it seems worth telling now [10] [15].

In the next section the dynamic model of field crop production is briefly described in nontechnical terms. The results of its application to the “Mississippi Delta” are presented in the following two sections. The first of these displays the derived trends in output, technology, and productivity; the second focuses on the demand for labor and the central hypothesis of the paper: the “two-stage push off the farm.” These are followed by a brief section on population effects where collateral census data are given. The interested reader will find a mathematical description of the model in an appendix.

It is important to recognize that the economic model presented here can generate no data beyond those inherent in already known facts. Instead it explains well-known events in terms of basic economic principles. Still it is possible, if the model’s explanatory power is affirmed, to estimate variables for which little quantitative information is avail-

² See Tables 3 and 4.

³ Day [3, Part III, “A Dynamic Production Model, Production Response of Cotton and Alternative Field Crops in the Mississippi Delta, 1940-1957”].

able. Here we do have excellent quantitative data on production. These data are used to test the model. The evidence suggests that the model is correct in essentials if not in every detail. On this inference are based my new estimates of the derived demand for labor and of labor productivity.

Recursive programming provides a general method for studying processes of economic development. It describes how development at once creates new opportunities and generates binding limitations; how it presents a counterpoint of growth and decay. Suitably modified for special properties of time and place, models similar to the one described here should be equally useful for investigating production, investment, and technological change and their consequences in almost any industry.⁴

I. *A Dynamic Model of Production*

A. *The Region and Its Technologies*

My initial study dealt with a small, relatively homogeneous and highly productive part of the south, the alluvial plains of the Mississippi River, or, as it is commonly known, "the Mississippi Delta."⁵ In the 1930s the area was dominated by sharecroppers with mule-powered, small-unit production. Mechanization was introduced in stages, first affecting land preparation and cultivation as tractor power displaced mules, then handweeding as flame throwers and herbicides were applied, and finally harvesting as mechanical cotton pickers replaced the sharecropper and his family. To summarize this picture, four representative technologies were constructed. They were: *Stage I*: Sharecropper unit. Mule-powered cultivation, hand picking of cotton and corn. *Stage II*: Partial mechanization of preharvest operations on the operator's share of the plantation. Tractor-powered land preparations; mule-powered cultivation; handpicking of cotton and corn; small-scale combines for harvesting soybeans, oats; three-man hay balers for hay crops. *Stage III*: Complete mechanization of pre-harvest operations except some handweeding of cotton and corn. Handpicking of cotton.

⁴Subsequent applications to agriculture are Schaller and Dean [12] and Heidhues [7]. A model of urban land development has been proposed by Schlager [13] and one of the U.S. iron and steel industry is described by Tsao [14]. A model that comes closest to the one described here in those aspects that deal with the regional impact of technology on labor demand is being developed in the Social Systems Research Institute by William Tabb and the author. It deals with the U.S. coal mining industry.

⁵The area to which the quantitative model was applied includes the Mississippi State counties of Bolivar, Coahoma, Humphreys, Issaqueno, Leflore, Quitman, Sharkey, Sunflower, Tallahatchie, Tunica and Washington. This small area is similar to the entire Delta region that includes in addition State Economic Areas 7a, 7b, 8a and 8b of Arkansas, Areas 1, 2 and 3 of Louisiana, and Areas 9a and 9b of Missouri. See *State Economic Areas*, U.S. Department of Commerce, Bureau of the Census, 1954.

Complete mechanization of corn. Self-propelled combines for oats and soybeans; one-man hay balers for hay crops. *Stage IV*: Complete mechanization, introduction of rice, a very small amount of hand-weeding of cotton remaining.

These omit some detail in the variety of technologies actually employed. However, a detailed analysis of costs indicated that the main differences likely to have had any economic importance were included.

B. Inputs and Outputs

Decisions involving the application of each technology to three different soil groupings at up to four levels of fertilization were included. In all, about a hundred alternative production processes were used to describe the region's basic set of agricultural opportunities. The process outputs included the production of cotton lint and cotton seed, corn, soybeans, oats, rice, soybean hay, alfalfa, and lespedeza hay, and the utilization of 36 variable inputs including labor, power, machinery, materials, and custom operations. The labor inputs included unskilled labor for chopping weeds, handpicking labor for cotton, tractor drivers, and special machine operators. The first two were almost always provided by sharecroppers, or by displaced sharecroppers in the form of resident or nonresident day laborers. Special machine operators receive a wage premium above tractor drivers and represent a special skill level because of their requirement to make timely repairs in the field in addition to routine operations.

C. Net Returns, Expectations, and the Profit Objective

Price series for outputs and inputs developed from a variety of sources made it possible to compute "per acre net returns" for each year, 1939-1958. Of course, returns are not known at the time the crop is planted and must be guessed by the farmer when he makes his decisions. Farmers' guesses of future prices are based on a variety of sources. The one most amenable to quantitative analysis is the immediate past. For this reason the previous year's prices were used as a first approximation, and this appears to be good enough for useful results.

Because farmers don't know what prices will be, it is useless to assume that they actually maximize profits. On the other hand, it is not useless to suppose that they try to improve profits given their information about the past and their uncertain guesses about the future. Accordingly, the model includes an objective function that represents the farmer's effort to decide process levels (that is the acreage devoted to each product, technology, soil class, fertilizer combination) that will increase total profits.

D. Constraints on Choice

The choice among production opportunities is constrained by land availability, machine capacities, supplies of off-farm inputs, uncertainty, and by the acreage allotments of federal farm policies. These forces are represented by a set of 35 dynamic inequalities that relate the magnitude of these factors to production decisions the preceding year. A brief description of them will complete the resume of the dynamic production model.

Land availability for the region as a whole is a more or less fixed factor. Some land clearing and draining is still taking place but its effects within the time period considered here are negligible. Land can, of course, be bought and sold on the regional land market, but this real estate activity was not included. Rather, my study focussed on the growth and decline of specific technologies. This was not because changes in farm size are uninteresting or unimportant, but because they are essentially derived from investment in capital intensive production. Three land constraints representing the regional availabilities of the three major soil groupings were included.

In order to avoid the complexities of full-blown capital theories and at the same time to emphasize the role of technological change, a simple scheme of representing investment in machine capacities was adopted. I have called it the "maximal potential growth principle," which is based on the empirical observation that "production can be expanded in a geometric ratio . . . during a given unit time period."⁶ Behind it lies the basic logic of production in time that governs the growth of industries generally, and the principles of learning that influence the adoption of new techniques. The former limits the supply of machinery from the manufacturing sector and the latter limits the demand for machinery at any one time by farmers. These two forces conspire to produce an upper bound on the utilization of a given technique during a given year.

Consequently, investment in the new technique is limited in the model to not more than a given proportional increase over the preceding period's utilized capacity. Whether investment proceeds to this limit is determined in the model by farmers' efforts to improve profits. Investment is pushed to the limit only if it accords with the objective of increasing production in a direction that increases profits as the latter can be judged from past experience. Otherwise, it falls short of the limit by a determinate amount, or is zero. The amount of investment is determined by the model, but its limit is predetermined as a behavioral characteristic of the region. This treatment is not a necessary part of

⁶ M. K. Wood observed this "law" in the aircraft industry [16]. For an exegesis of the concept in terms of agriculture see [3, pp. 89-92] or [1].

the method of analysis employed. Rather it is a practical expedient for building a model in a reasonably short period of time that includes investment and emphasizes technological change.⁷

Variable factors are those inputs that—to the individual firm—appear to be available in unlimited supplies at going prices. But what is true as an appearance for the firm is categorically false as a reality for the industry or the region of which it is a part. Though each firm may ignore its negligible effects on variable input markets, that market must strike a balance between available supplies and existing demands for the groups as a whole. This commonplace principle of standard economic theory is unfortunately difficult to accommodate quantitatively because of the rich structure of marketing activities in the “real world.” Yet it is not difficult to allow for the fact that the total amount of a given variable factor in a region is limited at a given point in time to display its effects on production activities.

In the present study the limited supplies of unskilled labor and of commercially produced nutrients were treated as fixed at the time of the production decision. They were assumed to follow “exogenously” determined trends over the history of the period considered. We thus focussed attention on the *effects* of the emmigration of labor and the explosive increase in the use of commercial nitrogen, leaving an *explanation* of those two diverse movements to more general studies. The “pull” of labor from the farm to the village and from the farm and village to industrial centers in the south and north was not explained by the model either. Yet, as we shall show below, our results indicate something quite significant about that pull all the same.

Attention has already been called to the uncertain guesses about the future that direct farmers’ decisions. This uncertainty restrains in any one year the change in acreage of a given crop. In the model this restraint is represented by behavioral bounds that are determined as proportional increases or decreases over the preceding year’s acreages. They circumscribe the flexibility with which the farmer—and therefore the region—responds to changing economic conditions.⁸

⁷For a much more elaborate approach to investment see Heidhues [7]. His concern is with agricultural adjustment in Germany to EEC policy. His methodology is quite similar.

⁸This approach was first used by J. M. Henderson [6]. For a further elaboration see [3, pp. 86-89] or [1]. I should have preferred to accommodate uncertainty in a much different, theoretically more meaningful way, by representing its effects through a stochastic, “risk programming” model, in that way treating the crop selection problem much like the “portfolio selection” problem to which it is so closely similar. But the difficulty of developing such a model without sacrificing detail in what I thought was the more important technological aspects of the study deterred us from that line of development. This “behavioral” approach has the virtue of operationality given current model solving capabilities. It also appears to come very close to the kind of considerations that are really made by the farmer. It has an empirical justification on that account alone.

During the 1940-1958 period the primary "instrumental variables" controlled by federal farm policy were commodity price supports and acreage allotments. Price supports are easily included in the model by replacing price expectations by the support values, whenever the latter were above the former. Allotments simply introduce an added constraint on the acreage totalled for all technologies, soils, and fertilizer levels for a given crop. Cotton had such restrictions in 1940-1943, 1951, 1954-1957, while rice had allotments in 1955-1957.

E. Review of the Recursive Programming Model and How it Works

The model that emerges from the constituents just described is applied to the Mississippi Delta. The objective criterion and the set of constraints are computed for the region as a whole and form a sequence of recursively dependent linear programming problems. The solution of each problem in the sequence approximates the aggregate choice of a large number of similar firms.⁹

The solution procedure begins with an initial set of capacities and expected net returns. The corresponding linear programming problem generates a solution that estimates the acreage of each process, the expected output of each crop, the utilization of each input and the increase—or decrease—in the capacity of each quasifixed factor. These are then used to estimate the availabilities of capacities and the magnitude of the uncertainty constraints for the succeeding period, which together with the exogenously determined supplies of labor and nitrogen, the fixed supply of land, and the actual prices of the given period yield the programming problem for the succeeding year. This chain of constrained maximizing problems is thus solved step by step for each period under consideration. The procedure was applied to data for the Mississippi Delta for the period 1939-1957, generating estimates of production, investment, land and labor utilization for the years 1940-1958 [3, pp. 193-220].

II. Trends in Output, Technology, and Productivity

The Crop Reporting Board (CRB) and the Agricultural Census data on harvested acres were used to test the model. The comparisons are shown in Figure 1 for the major field crops. They are reasonably good for all except oats. In the latter case, the evaluation is based on a shorter period because of missing CRB data. The census data indicate that, even for this crop, the model estimates are not far removed from the trend, at least for the 1940-1950 decade.¹⁰

⁹ For a discussion of the aggregation problem and the justification for this microeconomic interpretation see Day [2].

¹⁰ The proportion of variation in the CRB data "explained" were for cotton .91, corn .72, soybeans .89, oats -.05, and rice .83. Unlike regression models where the method of

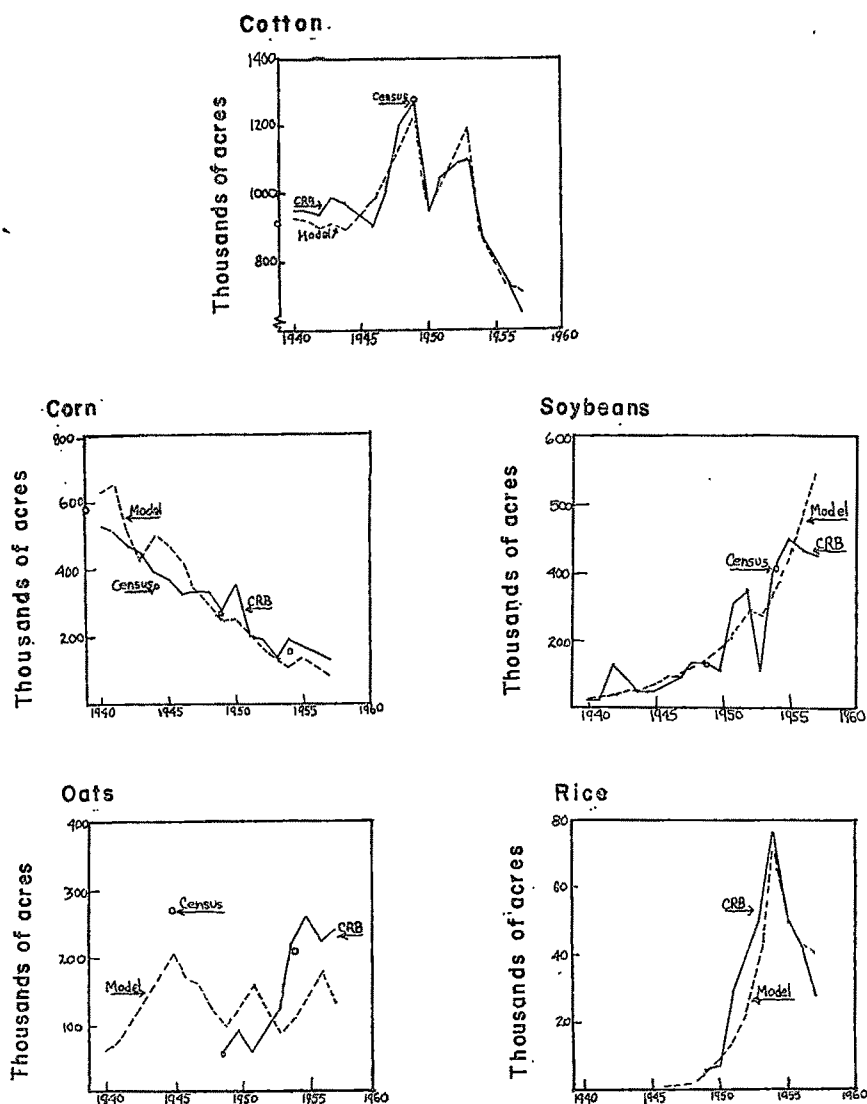


FIGURE I. TRENDS IN OUTPUT: CROP ACREAGES

These graphical comparisons indicate that the model is essentially correct even though it is incapable of estimating acreage changes with exact precision. On this evidence I indeed accept the model's general

estimation guarantees coefficients of determination between zero and one, recursive programming gives coefficients that may be large negative numbers, though, like their regression counterparts they can never exceed one. This explains the negative figure for oats.

validity¹¹ and in this and the next section turn to an exploration of its implications. Some of the latter relate to population movements. For this reason census data can add indirect information about the model's veracity. This is done in a section on population effects.

The top panel of Figure 1 shows the effect on cotton acreages of allotments during the years 1940-1943, 1950, and after 1954. The rapid buildup of cotton acreage during 1944-1949 and again during 1951-1954 contrasts sharply with the allotment years. These trends illustrate the powerful brake of production controls when they are in effect and the growth potential of the crop when unrestricted by government controls. The steady abandonment of corn and the explosive increase in soybeans and in rice acreages display significant shifts in cropping patterns. The decline in corn accompanies the abandonment of the mule technology and the shift of land to the profitable cash crop, soybeans. The sharp break in the explosive adoption of rice is associated with the imposition of federal controls in 1955.

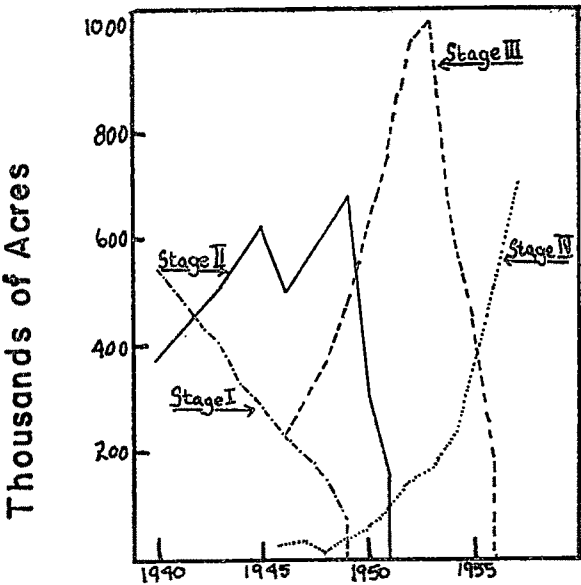
The pattern of technological change *derived from the model* is shown for cotton and corn in Figure 2. Because Stage III technology was introduced in the model in 1945, while in fact it was started in the 1930s, the pattern for Stage II cotton production shows a discontinuity at 1946 that would be eliminated by correcting the initial conditions. The model also exaggerates the adoption of cotton Stage IV after 1954. This is due to an oversimplification of the investment process. But these are errors of detail that do not modify the story of rapid adoption of new technique and rapid abandonment of the old.

Stage I, we recall, is the most labor and mule intensive set of techniques and is associated with the sharecropper tenure system. Roughly speaking one sharecropper family and one or two mules provided the power for a fifteen acre unit of a larger farm or plantation [5] [8]. The abandonment of the Stage I technology in favor of Stage II and Stage III indicates a decline, therefore, in the sharecropper system and a shift to wage labor supplied by resident, former sharecropper families, or by workers transported to the farm from neighboring villages.

Productivity is measured in various ways, but because in the present paper we are concerned with the effect of technological change on the derived demand for labor, it is particularly useful to show productivity in terms of labor input per unit of physical output. Actual input-output ratios change randomly from year to year as yields respond to the capricious movements of weather. To eliminate these deviations we have

¹¹ For a more complete presentation and critique of the model tests see [3, pp. 117-41]. A statistical theory of hypothesis testing is not available for recursive programs. Still a systematic comparison of model estimates with data has been performed.

Cotton



Corn

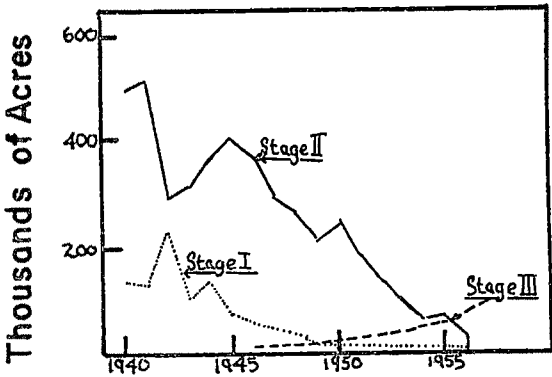
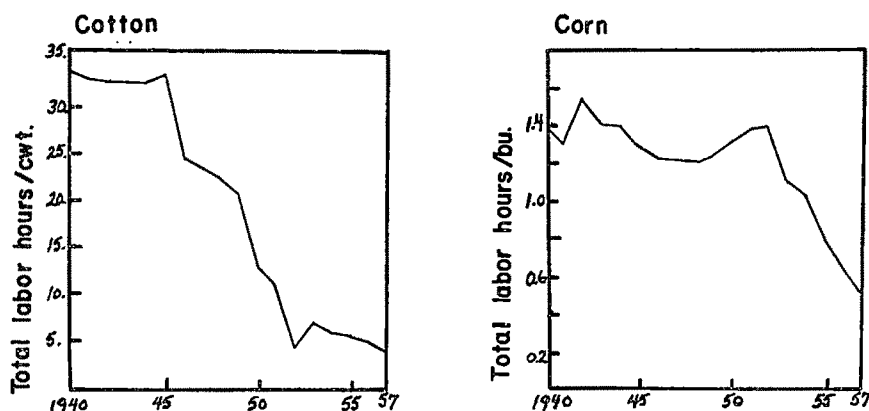


FIGURE 2. MODEL ESTIMATES OF THE ADOPTION OF NEW TECHNOLOGY AND THE ABANDONMENT OF OLD TECHNOLOGY

used the "average" yield coefficients of the model.¹² Also the contribution of each alternative way of producing cotton is included, weighting each by its corresponding process level as estimated by the model.

The ratios thus derived for cotton and corn are illustrated in Figure 3. Separate indexes for skilled and unskilled labor and the numerical counterpart of Figure 3 are presented in Table 1. They display declining ratios for unskilled labor and rising ratios for skilled labor. But the average effect, due to the vastly higher productivity of the skilled labor, is a sharply declining overall trend in the input-output ratios, as shown in the figure. It incorporates both shifts to labor saving technology and to higher levels of fertilization.



Total labor requirements in manhours per unit of output.

FIGURE 3. TRENDS IN AVERAGE LABOR REQUIREMENTS. DERIVED FROM THE MODEL.

III. *The Derived Demand for Labor and the Two Stage Push off the Farm*

The flow of investment into new production techniques implies a changing composition of labor demand. The trends in regional farm labor requirements, estimated by the model, are presented in Table 2. They include the utilization of "unskilled" labor, the use of "skilled" labor (tractor drivers plus special machine operators), and the trend in total utilization of labor of both kinds.

These show the combined effects of changes in machine technology, in fertilizer usage, and in cropping patterns. The effects of the first and last of these items are to reduce labor requirements, the first by substituting capital-intensive for labor-intensive techniques; the last by substituting labor saving for labor intensive crops—primarily soybeans for cotton and corn. The second item, increased fertilizer usage, tends to

¹² For the estimation of average yields see [3, pp. 75-80, 175-84].

TABLE 1—AVERAGE LABOR INPUT PER UNIT OF OUTPUT ESTIMATED BY THE MODEL

| Year | Cotton (hrs./cwt.) | | | Corn (hrs./bu.) | | |
|------|--------------------|---------------|-------|-----------------|---------------|-------|
| | Unskilled Labor | Skilled Labor | Total | Unskilled Labor | Skilled Labor | Total |
| 1940 | 33.5 | .32 | 33.82 | 1.2 | .20 | 1.40 |
| 1941 | 33.0 | .36 | 33.36 | 1.1 | .20 | 1.30 |
| 1942 | 32.5 | .40 | 32.90 | 1.4 | .17 | 1.57 |
| 1943 | 32.5 | .44 | 32.94 | 1.2 | .20 | 1.40 |
| 1944 | 32.4 | .51 | 32.91 | 1.2 | .20 | 1.40 |
| 1945 | 32.6 | .61 | 33.21 | 1.1 | .21 | 1.31 |
| 1946 | 23.5 | 1.07 | 24.57 | 1.0 | .21 | 1.21 |
| 1947 | 22.4 | 1.13 | 23.53 | 1.0 | .21 | 1.21 |
| 1948 | 21.3 | 1.16 | 22.46 | 1.0 | .21 | 1.21 |
| 1949 | 19.4 | 1.30 | 20.70 | 1.0 | .23 | 1.23 |
| 1950 | 11.5 | 1.45 | 12.95 | 1.0 | .34 | 1.34 |
| 1951 | 8.4 | 1.64 | 10.04 | 1.0 | .37 | 1.37 |
| 1952 | 3.0 | 1.82 | 4.82 | 1.3 | .10 | 1.40 |
| 1953 | 5.2 | 1.91 | 7.11 | 0.8 | .30 | 1.10 |
| 1954 | 4.5 | 1.89 | 6.39 | 0.7 | .33 | 1.03 |
| 1955 | 4.0 | 2.02 | 6.02 | 0.7 | .10 | .80 |
| 1956 | 3.1 | 2.29 | 5.39 | 0.4 | .23 | .63 |
| 1957 | 2.4 | 2.50 | 4.90 | 0.3 | .20 | .50 |

cancel some of the downward spiral in labor demand by raising cotton and corn yields per acre and thus increasing per-acre labor requirements for cotton and corn picking. But as hand labor is replaced by mechanical harvesting techniques this effect also declines in importance.

The implications of these patterns for farm organization may best be viewed by considering the contrast with agriculture outside the south. Labor demand in middle-western agriculture is predominantly satisfied by the "farm operator." That is, management and labor are combined. But the tenure pattern in the deep south at the beginning of our period displayed a quite different arrangement. Here the farm operator participated, if at all, only in the mechanized phases of the work. He reserved the remainder of his time for supervising the plantation's portion of the sharecropper's work and attended to the financial aspects of the plantation as a whole.

As a result of technological change this pattern changed radically. The operator himself more and more participated in the work of his now predominantly mechanized enterprise. His decision to invest in machinery also meant a decision to change the status of the sharecropper. The full effect of this pressure cannot fully be appreciated, however, without a look at still another aspect of the region's farm technology: seasonal distribution of labor requirements.

During the early parts of the 1939-1957 period the shift to labor

TABLE 2—FARM LABOR REQUIREMENTS IN THE MISSISSIPPI DELTA
ESTIMATED BY THE MODEL (millions of man hours)

| Year | Unskilled Labor | Skilled Labor | Total Labor |
|------|-----------------|---------------|-------------|
| 1940 | 170.2385 | .6908 | 170.929 |
| 1941 | 168.0305 | .7330 | 168.764 |
| 1942 | 166.6669 | .5615 | 167.228 |
| 1943 | 162.6678 | .6418 | 163.310 |
| 1944 | 161.9752 | .7588 | 162.734 |
| 1945 | 161.0240 | .8685 | 161.893 |
| 1946 | 130.1204 | 1.0672 | 131.188 |
| 1947 | 133.0420 | 1.1261 | 134.168 |
| 1948 | 141.2348 | 1.1899 | 142.425 |
| 1949 | 137.6645 | 1.3185 | 138.983 |
| 1950 | 76.0648 | 1.4504 | 77.515 |
| 1951 | 59.3976 | 1.6621 | 61.060 |
| 1952 | 38.6422 | 1.6477 | 40.290 |
| 1953 | 39.7715 | 1.8218 | 41.593 |
| 1954 | 28.4525 | 1.6837 | 30.136 |
| 1955 | 25.0885 | 1.6628 | 26.751 |
| 1956 | 17.5346 | 1.8570 | 19.392 |
| 1957 | 13.6818 | 1.9334 | 15.615 |

saving technology did not drastically reduce peak season labor demands. Rather it began a radical shift in its seasonal distribution. Stage II technology, for example, eliminated the mule-powered land preparation and cultivation activities. Stage III further reduced hand labor requirements and also mechanized labor requirements, the latter through adoption of larger scale equipment, the former through advancements in the use of flame throwers and herbicide applicators. But only after considerable investment in Stage IV technology took place, did the labor demand during cotton harvesting season decline for technological reasons.¹³ The basis for this seasonal effect is found by examining the average unskilled labor-time distribution functions shown in Figure 4. One can see that the consequence of adopting Stages II and III was to eliminate virtually all hand labor except for the summer weeding and fall harvesting seasons. This meant that the maintenance of sharecroppers the year round became uneconomic. Instead, a combination of resident wage labor and labor hired from nearby villages was favored. The implications for changing social structure are clear.¹⁴

¹³ The labor demand did fall during the 1950, 1954-57 years because of cotton production allotments.

¹⁴ A similar phenomenon will surely be encountered in other developing areas and industries that have quite asymmetric seasonal labor requirements. Seasonal unemployment—or in the aggregate, disguised unemployment, may very well be aggravated by technological advances that increase both output and peak season employment in the region or industry.

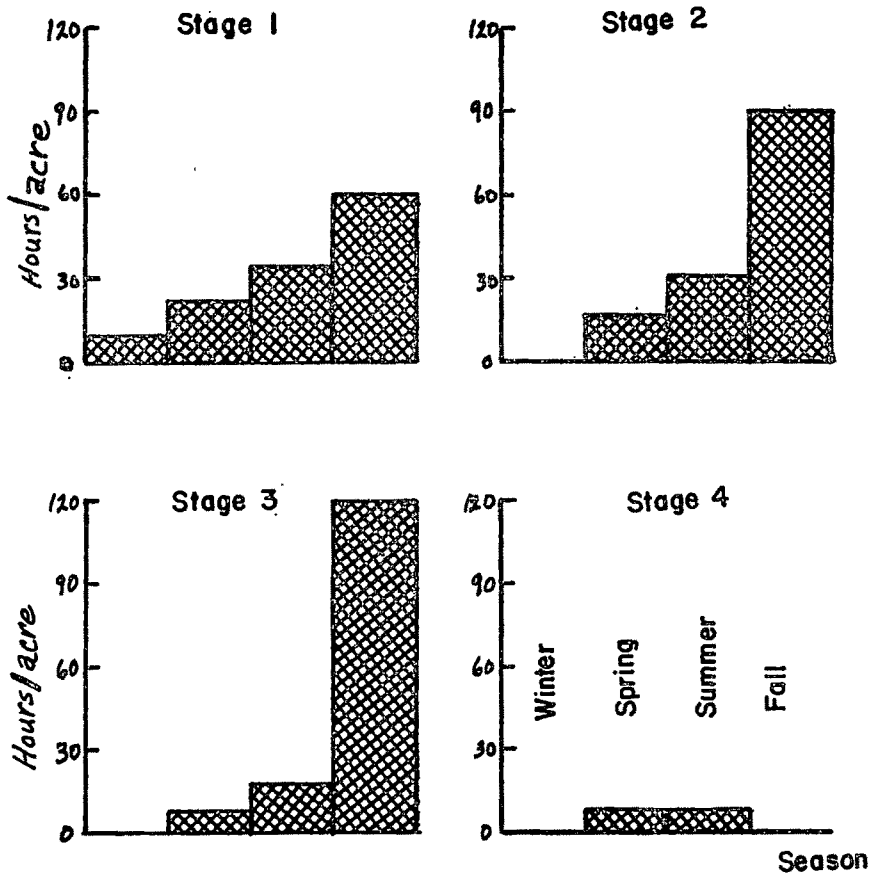


FIGURE 4. SEASONAL DISTRIBUTION OF UNSKILLED LABOR DEMAND IN COTTON PRODUCTION BY STAGE OF TECHNOLOGY.

But before we describe the implied two-stage push of labor off the farm we must take up one further technicality that was not included in the basic model description above. This item is a peculiarity of the model structure that identifies bottleneck and surplus resources. If one of the various constraints of the model is "equated" or "tight" it implies that more of the corresponding resource could be profitably used at current expected prices. If the constraint is "slack" or "loose," the opposite condition is implied.

The implications of this feature in the present analysis are that a declining resource that is "tight" is not being replaced at a rate sufficient to meet demand or, alternatively, it is being "pulled out" of the sector altogether. On the other hand, if the resource is "loose" it is a surplus item, and, if there is some cost to maintaining the surplus, it

is likely to be "pushed out" of the sector. During the recent history investigated here unskilled, harvest period (September-December) labor exhibited both categories [3, pp. 215]. This is shown in Figure 5.

During the war and immediate postwar era labor shortages were felt everywhere. After the war the increase in cotton acreage raised the derived demand for labor faster than the adoption of labor saving techniques lowered it. During this period the model shows that labor was a "tight" resource. But very soon, as the adoption of Stage III and IV methods accelerated, it lost this status, so that from 1941 to 1949 labor is a bottleneck but after that date it becomes a surplus commodity—even at the pitifully low wage rates in existence at the time.¹⁵

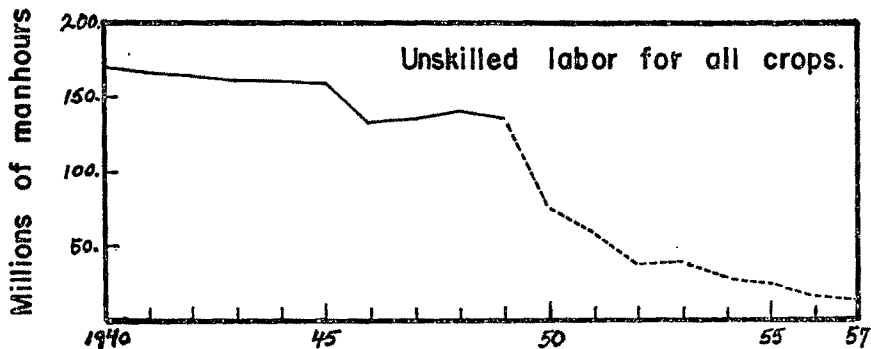


FIGURE 5. THE DERIVED DEMAND FOR UNSKILLED LABOR IN DELTA AGRICULTURE. Solid lines show year of labor "shortage." Dotted lines show year of labor "surplus."

Thus, without looking at migration data at all, it is possible to infer from the dynamic production model that during the first half of our period the labor released by mechanization was at least partially absorbed by the burgeoning economy in the industrial centers of the south and north. After that time external growth in demands for displaced sharecroppers was too sluggish and one may infer that such migration as may have occurred was induced more by a push than by a pull effect.

It is here that the consideration of seasonal labor distribution is important. For the early stages of technological changes were not associated so much with a push of sharecroppers out of the region (because

¹⁵ Wage rates in 1949 were roughly 32¢ per hour. They hit a 1940-58 peak in 1951 of about 40¢ per hour [3, p. 210]. Data are not included here to show the annual income of wage earners in the class we are considering but \$500 per year would not be at the lower end of the wage earners income range. It seems to me unlikely that still lower wages would have moved the surplus faster than its already rapid rate of exodus.

TABLE 3—DECILE POPULATION DATA

| Population | Delta | | | Mississippi | | | United States | | |
|---------------|-------------|------|------|-------------|------|------|---------------|------|------|
| | 1940 | 1950 | 1960 | 1940 | 1950 | 1960 | 1940 | 1950 | 1960 |
| | (thousands) | | | (thousands) | | | (millions) | | |
| Rural Farm | 316 | 257 | 119 | 1400 | 1097 | 543 | 30 | 23 | 13 |
| Rural Nonfarm | 51 | 66 | 128 | 351 | 475 | 814 | 27 | 31 | 40 |
| Urban | 63 | 87 | 121 | 433 | 607 | 821 | 74 | 96 | 125 |
| Total | 430 | 410 | 368 | 2184 | 2179 | 2178 | 131 | 150 | 178 |

of the remaining harvest season peak labor demands), so much as they were associated with a push of sharecroppers off the farm itself (where they had to be maintained year around) to the village (where they provided a conveniently located labor pool that could be inexpensively transported to surrounding plantations and farms).

To summarize, the peak harvesting season handwork load declined only with the adoption of technology Stage IV. It was this peak season load that formerly kept the agricultural worker in relatively short supply—even as he became increasingly urbanized. With this last vestige of economic livelihood rapidly dwindling, there occurred the second-stage push of labor out of the agricultural sector altogether and out of the region in search of some other way of life.

IV. *Population Effects*

If the two-stage push is a correct inference from the model, its effects should be reflected in population movements. We should observe a shift of people from a rural farm to a rural nonfarm status as a result of the first-stage push off the farm. Sharecroppers formerly classified as farmers become rural nonfarm workers as they move from a share-tenure arrangement to a wage-labor status either on the farm or in the villages and small towns. As a result of the second-stage push we should observe a movement of population from the rural to the urban sector and this should be reflected in a population increase in the cities or by a decrease in population in the region as a whole.

While a related ecological study cannot be pursued here in any great depth, a brief consideration of census data should provide a useful check on the analysis. Tables 3 and 4 present decile census data for the Mississippi Delta Area, the State of Mississippi, and the United States as a whole, the former in absolute numbers of people and the latter in percentage changes 1940-50, and 1950-60, 1940-60.

It seems to me there can be little doubt that the two-stage push occurred more or less as the model described it though it is worth re-

TABLE 4—RELATIVE POPULATION CHANGES

| Population | Delta | | | Mississippi | | | United States | | |
|--|---------|---------|---------|-------------|---------|---------|---------------|---------|---------|
| | 1940-50 | 1950-60 | 1940-60 | 1940-50 | 1950-60 | 1940-60 | 1940-50 | 1950-60 | 1940-60 |
| Rural Farm | -19 | -54 | -62 | -22 | -50 | -62 | -23 | -43 | -56 |
| Rural Nonfarm (Cities 2500 or less) | +30 | +93 | +151 | +35 | +71 | +132 | +14 | +29 | +48 |
| Urban (Cities over 2500) | +38 | +39 | +90 | +40 | +35 | +89 | +30 | +30 | +67 |
| Total | -5 | -10 | -12 | 0 | 0 | 0 | +14 | +19 | +34 |

membering that both push effects occurred to some extent at the same time. That is, some farms were in the second-stage push (caused by the adoption of Technology Stage IV) while others were still in the first (caused by the shift to Technology Stages II and III from the Sharecropper Stage I).

It is also important to remember that the census data only measure numbers of people in a given year. Because intervening births and deaths are not reflected in the figures, they only indicate a lower limit on the net flow of people from one sector to another. No doubt the migration was considerably more intense.

V. *Summary and Conclusions*

An economic model of production, investment, and technological change has been described that was tested using data for the Mississippi Delta farm economy. It included regional endowments of land resources, the response of yields to commercial nutrients, alternative cropping enterprises, and production techniques. Guided by the traditional profit maximizing theory of economics, the model explains how the interaction of geographic, biological, technological, and economic forces led in this region to a history of advances in productivity and declines in labor requirements at a revolutionary rate; how in fact within a generation the economic structure and social life of the region underwent a complete transformation.

The results, perhaps modified for differences in timing and technological detail, are reasonably representative of southern agriculture and even to some extent of U.S. agriculture as a whole. It also seems likely that the economics of technological change in other industries is similar and has equally significant implications. Studies involving investigations of several nonagricultural industries using similar methods are now under way in order to explore this possibility.

MATHEMATICAL NOTES

In this appendix the mathematical form of the analysis is outlined in the order followed in the text.

Production Processes, and Input, and Output Matrices

Let P_{hijk} be a production process for the h^{th} crop combination using the i^{th} technological stage on the j^{th} soil type at the k^{th} fertilizer level. Let x_{hijk} be the intensity measured in acres at which this process is operated, y^m_{hijk} be output coefficient (per acre yield) of the m^{th} output commodity and a^n_{hijk} be the input requirement of the n^{th} variable input. Here m ranges over nine output commodities and n ranges over 36 variable inputs including labor, power, machine operation and materials items. Arranging these items in a vertical list we obtain a 9-element output coefficient vector g_{hijk} , say, and a 36-element input coefficient vector a_{hijk} , say, for each of the 103 processes P_{hijk} . These may be stacked side by side to give the 9 by 103 Output Coefficient matrix G , say, and a 36 by 103 Input Coefficient matrix A , say.

This formulation allows for joint production such as would occur in fixed rotation programs. In the present model only part of the processes have this property. Cotton-lint and cotton seed are outputs of all cotton processes so that two positive yield coefficients appear for these. There are also some cotton processes that use mule powered draft operations in which both cotton and corn are produced. In these columns, three yield coefficients are greater than zero. This form enables one to account in a simple manner for the almost fixed complementarity between the production of cotton using mule power and the production of corn to feed mules.

Net Return and the Profit Objective

Let $p(t)$ be a 9-element vector of output prices in time t corresponding to the nine output commodities and $q(t)$ a 36-element vector of input prices at time t corresponding to the 36 input prices. Let $z(t)$ be a 103-element vector of unit process return where z_{hijk} is the net return of the process P_{hijk} . Then we have

$$z(t) = G^T p(t) - A^T q(t)$$

Expected net returns are assumed to be

$$\hat{z}(t) = z(t - 1)$$

Thus, for a given year they are assumed to be equal to what they would have been in the preceding year if the yield coefficients and variable input coefficients had been those used in planning. The profit or objective function that incorporates the goal of maximizing expected profits is

$$\pi(t) = \hat{z}^T(t)x(t)$$

Fixed Factor Constraints (land)

If \bar{x}_j is the regional supply of soil j then

$$\sum_{hik} x_{hijk} \leq \bar{x}_j$$

Quasifixed Factor Constraints (machine capacities)

The flows of services of the various stocks of quasifixed factors associated with a given technology are measured by either total acres of capacity or

total production capacity. In reality capacity in machinery utilization is considerably more flexible than this treatment indicates. At the regional level of aggregation, however, the distortion does not appear to be great.

Let $I_{hi}(t)$ be actual investment in the machine capacity of technological stage h for producing crop combination i , and let $C_{hi}^*(t)$ be the capacity actually utilized, and α_{hi} be a "maximum potential investment coefficient." Then

$$I_{hi}(t) \leq \alpha_{hi} C_{hi}^*(t)$$

Suppose the amount of capacity utilized by a given process is given by the "capital utilization coefficient" b_{hijk} and $x_{hijk}^*(t)$ is the process level in year t then

$$C_{hi}^*(t) = \sum_{jk} b_{hijk} x_{hijk}^*(t).$$

Now by definition $I_{hi}(t) = C_{hi}^*(t) - C_{hi}^*(t-1)$ so that

$$\sum_{jk} b_{hijk} x_{hijk}(t) \leq (1 + \alpha_{hi}) \sum_{jk} b_{hijk} x_{hijk}^*(t-1)$$

The investment parameters " α_i " are really behavioral coefficients that describe the rate at which farmers in the region are willing and able to invest in quasifixed factors, a rate that depends upon growth in knowledge about previously unused machines and upon the rate at which quasifixed factors can be supplied by the industries producing them.

Labor and Fertilizer

In the text labor and fertilizer were considered to be quasifixed factors at the regional level. This treatment rests not on the durability of these inputs but rather on three separate considerations related to firm behavior. One is the effect of money capital, another is growth in knowledge, and a third, imperfect knowledge of supply-price relationships. Money capital is not treated explicitly in this model, but its constraining influence is felt in the region due to the frequent practice of financing production expenses and investment capital. This influence is felt particularly on the purchase of labor and fertilizer which comprise a large part of production expenses in the region. In the case of fertilizer, knowledge about the benefits of increasing intensity of utilization has accumulated steadily. Demand for its use has correspondingly risen with this knowledge. Finally, and quite surprisingly, individual farmers may face a rising supply curve for labor. That is, farmers may restrict their purchase of labor services at existing (perhaps economical) prices because they may expect that to acquire more they would have to pay an uneconomic price. This phenomenon most probably is caused by emigration of unskilled labor from rural areas to neighboring towns. Costs of transporting labor back to the farm raises its price. It is also caused by net emigration out of the region altogether. The supply curve for labor to the firm has probably receded over time due to emigration, and its price elasticity over relevant ranges has probably decreased due to transportation costs.

Unable to develop explicit models of supply for these factors, we treat the

total potential utilization of these inputs as exogenous trends, falling in the case of labor and rising in the case of fertilizer utilization. If ℓ and f are the input indexes of labor and fertilizer respectively for the production process represented by indexes $hijk$, then

$$\sum_{hijk} a_{hijk}^m x_{hijk}(t) \leq k_m(t), \quad m = \ell, f.$$

Where $k_\ell(t)$, $k_f(t)$ are exogenously determined trend values in total potential utilization of labor and fertilizer respectively in the region.

Uncertainty (Flexibility) Constraints

Let $\bar{\beta}_i$ be an "upper flexibility coefficient" and β_i be a "lower flexibility coefficient" for the i^{th} crop combination. Then

$$\begin{aligned} \sum_{hjk} x_{hijk}(t) &\leq (1 + \bar{\beta}_i) \sum x_{hijk}^*(t-1) \\ - \sum x_{hijk}(t) &\leq - (1 - \beta_i) \sum x_{hijk}^*(t-1). \end{aligned}$$

Here the flexibility of increasing or decreasing the acreage of a crop is found above and below by certain percentage changes over the preceding year.

One may ask how these behavioral constraints can in any sense reflect real behavior. One finds the answers in farmers' imperfect knowledge of demand for their outputs. Farmers are well acquainted with the fact that if aggregate supplies of their commodities increase, prices received will fall. But they cannot know in advance what aggregate supplies will be, not only because of weather, but also because they cannot forecast the behavior of all other producers. Therefore, they are cautious in changing their production patterns in response to current profit expectations.

Acreage Allotments

Let $a_i(t)$ be the allotment in year (t) for the i^{th} crop or if there is no allotment set $a_i(t) = \infty$. Then

$$\sum_{hjk} x_{hijk}(t) \leq a_i(t)$$

Summary of Constraints

The various constraints given above can be summarized by the following constraint notation

$$Bx(t) \leq \Gamma Bx^*(t-1) + k(t)$$

in which the rows of B correspond to the various constraints and contain the zero, one, or b or " $b_{ijk\ell}$ " coefficients contained in them, and in which the diagonal elements of the Γ coefficients are zero for the fixed factors, exogenous variable factors and allotments and otherwise are the $1+\alpha$ or $1\pm\beta$ coefficients of the investment and uncertainty constraints, all nondiagonal elements being zero.

The Recursive Programming Formulation

From the above we may form the following dual recursive programming problems.

$$\pi^*(t) = \max_{x(t)} [G^T p(t-1) - A^T q(t-1)]^T x(t)$$

subject to

$$Bx(t) \leq \Gamma B^*(t-1) + k(t),$$

and

$$\rho^*(t) = \min_{r(t)} [\Gamma Bx^*(t-1) + k(t)]^T r(t)$$

subject to

$$B^T r(t) \geq G^T p(t-1) - A^T q(t-1).$$

The model has initial conditions $x(1939)$ and exogenous variables $p(t)$, $q(t)$ and $k(t)$, $t=1939, \dots, 1958$. The model is solved recursively for $t=1940, \dots, 1958$.

Derived Supply of Agricultural Outputs

Let $Y(t)$ be the vector of estimated supplies of agricultural commodities of the region. Then

$$Y(t) = Gx^*(t), \quad t = 1940, \dots, 1958.$$

Derived Demand for Agricultural Inputs

Let $V(t)$ be the vector of estimated derived demands for inputs, then

$$V(t) = Ax^*(t), \quad t = 1940, \dots, 1958.$$

In particular, among the elements of $V(t)$ are the derived demands for labor by type.

Productivity Indexes

Let

$y_m(t)$ be the output of the m^{th} commodity at time t .

$$\text{Then } Y_m(t) = \sum_{hijk} y_{hijk}^m x_{hijk}^*(t)$$

Let

$V_n(t)$ be the input of the n^{th} input at time t .

$$\text{Then } V_n(t) = \sum_{hijk} a_{hijk}^n x_{hijk}^*(t)$$

Then the average input/output ratio is $V_n(t)/Y_m(t) = \phi_{nm}(t)$.

Or if one wants an average input/output ratio derived for several inputs we have

$$\phi'_{nm}(t) = \sum V_n(t)/Y_m(t)$$

where the summation is over some range of n .

Seasonal Distribution of Labor

Let γ^s_{hijk} be coefficients describing the time distribution of labor requirements for the given process. Then

$$\gamma^s_{hijk} a^n_{hijk} = a^{ns}_{hijk}$$

would give the requirement of input n during time subperiod s for process P_{hijk} .

By appropriate constructions these coefficients could be used to derive the seasonal distributions of labor (or any other input) demand as a function of time that depended on the derived process levels $x^*(t)$. This changing pattern would therefore be related to all the components of the model.

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THE SHORT RUN REVISITED

By LOUIS DE ALESSI*

The theory of the firm has been the subject of voluminous literature. Nevertheless, some ambiguities and inconsistencies still persist. In particular, the traditional approach to the short run as a period in which the quantities of some inputs cannot be varied¹ requires clarification. The failure to recognize explicitly that the adjustment of a firm to a change in market conditions depends upon the costs and receipts associated with the adjustment leads to a number of ambiguities, including some confusion regarding the regions of the production function that are empirically relevant.

Section I contains a brief statement of the traditional short-run theory of the firm; then, as a first approximation, the main implications regarding the paths of prices and of input proportions in the short run are derived from higher-level economic hypotheses. Section II indicates how the relevant predictions regarding the short-run behavior of the firm may be derived from the wealth-maximizing (stock) approach, avoiding some shortcomings of the traditional profit-maximizing (flow) approach. Section III extends the analysis to the "uneconomic" regions of the production function. Section IV contains a few concluding remarks.

I

Consider a competitive firm whose production function²

$$(1) \quad Z = f(a_1, a_2)$$

is hypothesized to be a single-valued, continuous function with continuous first- and second-order partial derivatives; all variables represent flows per unit time.³ The parameter Z° defines a particular output isoquant, conforming to the usual requirement, with slope at a point equal to $-f_1/f_2$.

Let the firm purchase inputs a_1 and a_2 in perfectly competitive markets at constant prices p_1 and p_2 . Total costs of production are given by the linear equation:

$$(2) \quad C = p_1 a_1 + p_2 a_2,$$

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¹ For example, see [11, p. 41].

² The following exposition, equations (1) through (3), is standard in the literature. For example, see [6, Ch. 3].

³ The traditional production function relates rates of input utilization to rates of output. As A. A. Alchian [1] has suggested, however, planned volume (V) of output may be a crucial variable; this point, together with some shortcomings of the flow relative to the stock approach in the analysis of the firm, will be examined in subsequent sections of this paper.

and the parameter C° defines a particular isocost with constant slope equal to $-p_1/p_2$.

Well-known first-order conditions for cost minimization subject to an output constraint require that the input-output combination be on the locus of points (least cost path) where:

$$(3) \quad \frac{f_1}{f_2} = \frac{p_1}{p_2},$$

that is, where the ratio of the marginal products of the inputs is equal to the ratio of their prices.⁴ In Figure 1, the least cost path is shown by OE .

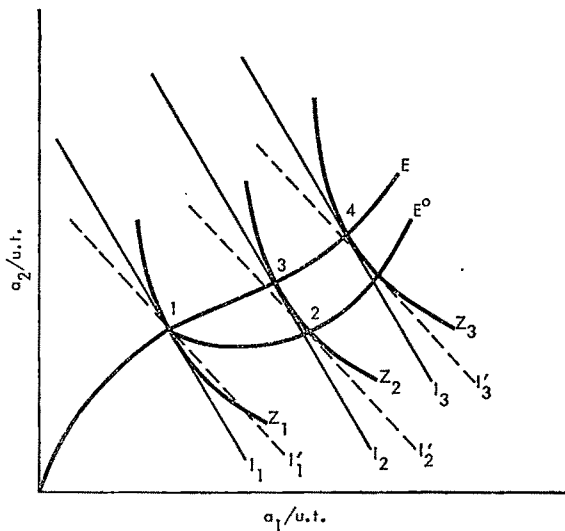


FIGURE 1

Given the output demand function, the long-run equilibrium conditions for a profit-maximizing firm are easily obtained. Traditional statements of the theory then turn to the main problem at hand, the response of the individual firm to changes in circumstances (e.g., an increase in the demand for the output of the firm) that disrupt the equilibrium.⁵ Let T measure the period from the instant of time some disturbance occurs to the instant of time a given adjustment is completed, where the

⁴ Fulfillment of second-order conditions implies that output isoquants are concave from above over the relevant range [6, p. 51].

⁵ For the sake of brevity, the analysis in this paper is usually limited to the consequences associated with an increase in the demand for the output of the firm. The analysis, however, can be easily extended to include the consequences of a decrease in demand, of changes in the input supply function(s), and of changes in the firm's production function due to technological innovations.

adjustment may involve a change in input proportions as well as a change in the absolute quantity and in the form of the inputs utilized. The usual procedure is to consider adjustment periods or runs of progressively longer duration, where the longer the run, the more inputs the firm "can" vary and the greater the variation permitted in the quantity of any given input. In the two-input case, one short run may thus be shown by holding a_2 fixed at the initial equilibrium level (e.g., point 1 in Fig. 1) and allowing a_1 to vary.

The purpose of distinguishing between runs of different length presumably is to explain the empirical observations that, the shorter the period T , the higher is the cost of the change in output and the fewer the inputs that are varied. It follows that economic theory, *inter alia*, must yield implications consistent with this evidence. By defining the short run as the time period in which some inputs cannot be varied, the desired implications regarding costs are obtained, e.g., [5, pp. 111-15]. This solution, however, avoids a crucial theoretical issue. Implications regarding the proportions in which factors are varied in a given market situation must also be derived from the theory. In particular, the statement that some inputs are held fixed during a given period must be interpreted as a falsifiable proposition implied by economic theory. The phenomenon in question may be identified (defined) as a short run, but the definition in no sense substitutes for the hypothesis.

All factors of production are variable over any given time interval greater than zero. The rate at which different inputs in fact are varied over time in response to some change in market conditions must depend upon the relative costs and receipts of all the alternative production strategies technologically available to the firm.

As a first approximation, hypothesize that the closer an output program is moved to the present (the shorter the period T) the greater are the costs [1]. Applied to the sellers of the inputs, this proposition implies that as shorter T 's are considered the input supply curves decrease (shift to the left) and the greater is the cost to the buyer of varying any given input. That is,

$$(4) \quad p_i = p_i(T), \quad p'_i < 0, \quad (i = 1, 2);$$

substituting⁶ into equation (3) yields:

$$(5) \quad \frac{f_1}{f_2} = \frac{p_1(T)}{p_2(T)}.$$

⁶ Although the analysis developed in this paper is not dependent upon the validity of the following conditions, it may be presumed that eventually $p'_i = 0$, and that $p''_i > 0$ over at least part of the interval where $p'_i < 0$.

Equation (4) does not deny that each firm purchases its inputs competitively. The input supply curve facing the individual firm is still hypothesized to be perfectly elastic at the price associated with a particular T ; however, the shorter the period the higher is the price intercept.

As shorter periods T are considered, not only do supply functions decrease, but the rates of shifting differ among at least some of the inputs. The ratio $p_1(T)/p_2(T)$ increases, remains the same, or decreases depending upon whether the rate of change in p_1 with respect to T is greater than, equal to, or less than the rate of change in p_2 with respect to T (in all cases, all isocosts shift to the left as shorter T 's are considered).

Each firm is thus hypothesized to consult a family of sets of budget constraints, where each set contains all possible alternative budget constraints of a given slope associated with the particular run contemplated by the firm for the (possibly partial) adjustment. Presumably one family of sets is applicable to expansion and another is applicable to contraction. Given the iso-product map derived from the traditional production function, a least cost path exists for each input price ratio associated with each adjustment period T . Each short-run least cost path is discontinuous at the original equilibrium point (e.g., $1E^\circ$ in Fig. 1), since the firm presumably can continue to produce the current output at the current least cost.

Each short-run least cost path yields a short-run total cost curve. If diminishing returns prevail, the total cost curve (TC) for an increase in output during a particular short run T° would be similar to TC° in Fig. 2.⁷ The longer the adjustment period T , the closer the short-run total cost curve approaches the long-run TC . Thus, the long-run TC (e.g., TC in Fig. 2) is the boundary of all the short-run TC curves.

Economic theory also asserts that the shorter the time interval, the smaller is the price elasticity of the market demand for a commodity. Thus, even in the case of a firm selling its output under purely competitive conditions, different output prices will prevail during at least some of the different adjustment periods considered by the firm. The form of the function relating the demand price per unit of output to T presumably has the same general properties attributed in equation (4) to the input price functions. It follows that a unique total revenue curve and a

⁷ Note that the behavior of the individual firm is at issue. Thus, the *schedule* of input prices (where each price relates to a particular "run") is impounded in *ceteris paribus*. If expansion or contraction of all firms in the industry affects the schedules of input prices (input supply functions facing the industry are not perfectly elastic), then the firm's cost curves will shift accordingly.

The discontinuity in TC° is associated with the approximation developed in this section and the two-dimensional nature of the diagram. A more complete statement of the appropriate cost surface is presented in Section II.

unique total cost curve may be associated with each run,⁸ and the usual criteria may be used to derive the profit-maximizing program for each period.

The analysis developed in this section implies that the firm is induced to seek full adjustment over time rather than instantaneously, and to vary inputs in different proportions as longer T 's are considered. In particular, the firm may *choose* to hold one or more inputs fixed during periods of partial adjustment. For example, suppose that a firm initially

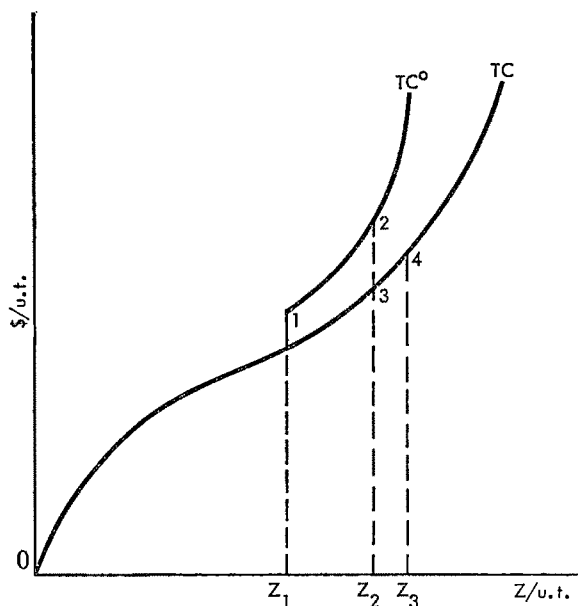


FIGURE 2

is producing under conditions of long-run equilibrium (e.g., point 1 in Figs. 1 and 2), and that the demand for its output increases. Given the elasticity of this demand over time and the family of all short-run relative input prices, the profit-maximizing program during a particular short run may be that associated with the input price ratio shown by I'_2 (Fig. 1), and with point 2 (Figs. 1 and 2) in particular. In this event, the firm would choose to hold a_2 fixed for a time. The long-run equilibrium output, for an unchanged *schedule* of input prices, would still be along OE (e.g., point 4 in Figs. 1 and 2). It should be noted that, in the absence of any information regarding the relative costs of varying inputs

⁸ This approach yields the familiar implication that firms will specialize in the speed (time elapsed) of their responses to changes in market conditions.

in different proportions over different time periods, there is no a priori reason for predicting which factor, if any, will be held constant.⁹

It seems generally recognized that adjustment to a change in market conditions is not a free good. So far, the main assertion contained in this paper is that the extent of the adjustment depends upon the relevant costs and receipts. The traditional statement that the quantities of some inputs cannot be varied in the short run implies a denial of the higher-level economic hypotheses that demand curves are negatively sloped (the lower the cost of changing the input mix, *ceteris paribus*, the quicker the full adjustment) and that supply curves are positively sloped (the quicker the full adjustment sought, *ceteris paribus*, the higher the cost of changing the input mix).

Similarity with traditional analysis, however, has been maintained by choosing the particular set of isocosts associated with a particular run, relating the short-run least cost path to the price per unit of output associated with that run, and identifying the profit-maximizing input-output combination.

II

The isoquant-based theory of the firm yields decision rules in terms of flows: under long-run competitive equilibrium, at the margin the rate of expenditures on all inputs is equal to the rate of receipts from the sale of the output, where presumably the rates in question remain constant indefinitely. When a disturbance occurs, *ceteris paribus*, the new long-run equilibrium flows may be derived and used to predict the long-run response of the firm. Under these circumstances, the present value of the constant rate of outlay given by each isocost and the present value of the constant rate of receipts associated with each output isoquant will yield present value total cost and total revenue curves with the same shape as the total cost and the total revenue curves given by the profit-maximizing model of traditional theory. Thus application of the usual marginal rules to the stock (wealth) and to the flow (profit) models would yield the same predictions regarding the long-run behavior of the firm. As Alchian and others have emphasized, however, the crucial concept is the *wealth* effect of the alternative strategies considered by the firm; this must be so, since the firm can always alter the time pattern of the flows by either lending or borrowing.

During the process of adjusting to a given disturbance, the pattern of flows for a given firm may be expected to vary from period to period.

⁹ This conclusion does not seem to be inconsistent with observation. Without allowing empirical evidence to intrude, it might be granted that in some cases the short-run cost of varying the quantity of some types of labor is greater than the cost of varying some types of plants or equipment, while in other cases the opposite cost relationship prevails. For an example of empirical evidence inconsistent with the classical treatment of labor as a purely variable factor, see [9].

Predictions regarding the behavior of the firm in the (traditional) short run are usually obtained by examining progressively longer "representative" short runs of unspecified duration. Similarly, the short-run least cost paths derived in the preceding section may be useful pedagogically in deriving implications with respect to the nature of the short-run cost curves, and, in addition, to the short-run changes in input proportions. However, it must be recognized that the decision of the firm to adopt a particular input-output program during a particular time period can only be predicted by discounting to the present the flows associated with the alternative input-output programs for different periods, and then choosing the wealth-maximizing sequence of programs. That is, a rational firm must consider a multiperiod horizon in determining the input-output rates in a specific period; in particular, a firm owning resources must decide the intertemporal allocation of such resources.

A firm responding to a change in market conditions may be viewed as producing the adjustment involved in addition to the other product (Z) under consideration. The present value cost, C_p , of the adjustment may be hypothesized to be a function of (1) x , the rate at which the adjustment is undertaken, (2) V , the total planned adjustment, (3) T^* , the length of the interval between the time when the disturbance occurs and the time when the adjustment is begun, and (4) m , the length of the interval between the time when the adjustment is begun and the time when the adjustment is completed:¹⁰

$$(6) \quad C_p = f(x, V, m, T^*), \quad C_p \geq 0, .$$

where $f_x > 0$, $f_{xx} > 0$, $f_V > 0$, $f_{VV} < 0$, $f_{xV} < 0$, $f_{T^*} > 0$ [1]. The total quantity of the adjustment undertaken and its schedule over time would then be determined at the margin in conjunction with the present value of the receipts from the adjustment. In this construct, the long run may be defined as the time period in which deferred changes in the quantity (and form) of each input used by the firm would not lead to a lower input price to the firm in present-value terms.

That is, the individual firm is hypothesized to adjust the quantity and the form of each input it uses until the present value of the marginal stream of outlays is equal to the present value of the marginal stream of receipts for each input. The firm chooses the least-cost mesh of the alternative input adjustments subject to a set of output constraints related to the elasticity over time of the demand for the firm's output. The wealth-maximizing solution thus yields the total planned change in the quantity and form of each input, the rate at which the change is under-

¹⁰ The T^* used in this paper corresponds to the T used by Alchian [1]. Following Alchian, m is a dummy variable whose value is determined by the values assigned to x and to V , where $V = \int_{T^*}^{T^*+m} x(t) dt$ and x may vary over time.

taken, the instant of time when the change is begun, and the time period taken to complete it.¹¹ *Inter alia*, this approach yields information regarding the time period during which a specific input is held fixed and the schedule at which each input adjustment is phased into the production process.

The procedure outlined in the preceding paragraph would yield one point on the (flow) iso-product map for each period. The input-output program presumably will vary from period to period as the firm completes its adjustment, and may involve holding the rate of utilization of one or more inputs fixed during a given time period.¹²

These comments suggest that a particular input-output combination may appear to be irrational in terms of the flows prevailing in that period, and yet be the rational choice in terms of the wealth-maximizing model. Put more strongly, the flows prevailing in one time period do not provide, by themselves, sufficient information to determine the choice of the output program for that period [1]. Economists have obtained propositions regarding the short-run behavior of the firm from a model designed to predict long-run behavior under constant flows, but they have done so only at the cost of ignoring some relevant portions of economic theory.

III

The production function of a firm, according to one definition, "... shows the (maximum) quantity of product it [the firm] can produce for given quantities of each of the various factors of production it uses" [5, p. 123]. This definition has been taken to imply that a firm would not use additional quantities of a given input¹³ beyond the economic region;¹⁴ that is, output isoquants outside the relevant ridge lines would be straight lines parallel to their respective axes [3, p. 304]. A second definition of the production function rests on the minimum factor quantities necessary for given outputs. On this definition, the segments of the output isoquants outside the ridge lines are taken to vanish [3, p. 304].

¹¹ In the case of one type of labor, for example, the present value of the hiring and training costs would be related to the planned number of workers to be added to the labor force, the rate at which such individuals are to be hired and trained, and the instant of time when the hiring and training programs are to begin.

¹² J. Hirshleifer has suggested that a firm may choose to hold some inputs fixed in the short run because of uncertainty regarding the permanence of the initial disturbance [7, p. 250]. The argument developed in this paper, although reinforced by the introduction of uncertainty, suggests that some inputs may be held fixed under conditions of certainty.

¹³ As M. Friedman has pointed out, however, this presumes that the cost of discarding some units of this input is zero [5, pp. 130-131].

¹⁴ The economic region is frequently defined (Rule I) as the range over which the marginal products (*MP*) of all inputs are greater than zero (and, possibly, equal to zero for some inputs); G. H. Borts and E. J. Mishan do so initially [3, p. 300]. Friedman [5, p. 130] and others,

Recently, Borts and Mishan have argued for a re-interpretation of the first definition. If a factor is taken to be fixed (and indivisible) during a given time period, and if the firm sells its output under monopolistic conditions, then output isoquants supposedly have meaning in the uneconomic region for the fixed factor and are straight lines parallel to the axis in the uneconomic region for the variable factor.¹⁵ Borts and Mishan conclude that "... it is logically inadmissible to construct a diagram in which in both uneconomic regions the iso-product curves are uniquely determined" [3, p. 307].

If "uneconomic" regions exist,¹⁶ the flow analysis developed in Section I of this paper implies that a firm would not operate within them.¹⁷

including Borts and Mishan later in the same article [3, p. 305], have noted that the maxim of rational behavior for a firm (Rule II) is to use such a combination of factors that the average product (AP) to each input separately is falling (or at least remains constant).

As Friedman and others recognize, these two rules yield the same answer only if the production function is homogeneous of degree one; then the locus of points (ridge line) where the AP of one input is maximum and the ridge line where the MP of the other input is equal to zero coincide. If the production function is homogeneous of degree less than one, then the ridge lines for AP maximum are outside the ridge lines for $MP=0$; if the production function is homogeneous of degree greater than one, the order of the ridge lines is reversed.

The two rules, of course, are not mutually exclusive. As Friedman concludes, "The point not to be exceeded is the point of vanishing (marginal) returns; the prudent man will seek to exceed the point of diminishing (average) returns" [5, p. 130].

If production functions are taken to be homogeneous of degree less than one, with degree one as the limit, satisfying the marginal conditions (operating in the range where the output isoquants are concave from above) is sufficient to insure that the AP of each input is either decreasing or constant.

Some economists, e.g., [10, p. 177], have suggested that the derivative of output, dZ (taken with respect to one of the inputs, when all inputs vary proportionately), first increases (increasing returns to scale) and then decreases (decreasing returns to scale). In the range of increasing returns to scale, application of rules I and II for rational behavior implies that the segments of the negatively sloped output isoquants outside the ridge lines for AP maximum would be in the "uneconomic" region of the production function.

Production functions yielding increasing returns to scale throughout are taken to be empirically irrelevant, since evidence purporting to support such functions has failed to allow for changes in the planned volume of output.

It may be useful to examine the form of the production function implied by Alchian's reformulation of the cost function. If planned volume (V) of output is held constant, the production surface with respect to the rate (x) of output exhibits decreasing returns to scale throughout. If the rate of output is held constant, the production surface with respect to V exhibits increasing returns to scale throughout. If V and x are taken to vary proportionately (the usual case, as suggested by Hirschleifer [7] and apparently accepted by Alchian [2, Ch. 21]), the production envelope at first exhibits increasing returns to scale and then decreasing returns to scale.

¹⁵ Borts and Mishan [3, p. 307] argue that (i) if both factors are indivisible in the short period, the firm is restricted to a point within the economic region; (ii) if both factors are variable during the period in question, all choices must fall within the economic region.

¹⁶ This paper is not concerned with the empirical question of whether "uneconomic" regions exist.

¹⁷ As noted earlier, second-order conditions for profit maximization imply that the firm would operate in the range where iso-product curves are concave from above. Moreover, a firm (at least a competitive firm) would operate beyond the region of increasing returns to scale if such a region existed.

Since it has been suggested that a noncompetitive firm using an "indivisible" factor may in fact operate in one of the uneconomic regions in the short run, the point deserves consideration.

The first issue to be examined is that of indivisibility.¹⁸ Why should a factor be indivisible in the short run? To take a standard example, consider a firm using tractors as one of the inputs. Following Friedman [5, pp. 131-32], suppose that tractors come in two sizes, with the tractor of size II being in some relevant sense "twice" the tractor of size I. Tractors presumably come in these two sizes, given the demand function for tractors, due to the lower unit cost associated with producing a larger planned volume of output (V) of each of the fewer models relative to the cost of producing a smaller V of each of a broader range of models [1]. A firm using tractors of size I, following some change such as an increase in the demand for its output, may well prefer to shift to an intermediate-size tractor at an intermediate price.¹⁹ If the cost of an intermediate-size tractor produced to order is sufficiently high, however, the firm may choose to acquire more of the standard sizes. That is, the choice to acquire some inputs in lumps can be predicted, at least in some relevant cases, by cost considerations alone. It may seem that this argument, if acceptable, applies to "indivisibility in acquisition" and not to "indivisibility in use" [8, pp. 231-33]. But "divisibility" of all inputs in acquisition implies "divisibility" of all inputs in use, since a firm, at some cost, may always choose to modify the structure of its assets or change the form of its inputs.²⁰ The theory must be capable of predicting the revealed choice of a firm in the short run, under specified circumstances, to vary some inputs in discrete quantities rather than in infinitesimally small quantities; it is not helpful to cloak in the name of indivisibility what is really a plea of ignorance.²¹

¹⁸ The term "indivisibility" has been used to cover a variety of phenomena. For example some economists have used it to describe a factor which cannot be varied during a given interval of time, e.g., [3, p. 304]; some economists have also noted that "... indivisibility is necessary for economies of size or scale" [3, p. 305]. This paper is concerned with indivisibility as a short-run phenomenon.

¹⁹ The "half-size tractor" to drive the "half-size tractor" [5, p. 132] apparently would not be a limit here. In any event I fail to see why, at some suitable price, the "half-size man" (in the technologically relevant sense) would not be forthcoming.

²⁰ The distinction between fixed and variable costs is also ambiguous. Whether a firm chooses to vary a particular cost depends upon the relative gains and losses of doing so. At some cost a contract can be broken (as any lawyer will gladly admit), a private radio network substituted for a telephone, and so on.

The only costs which legitimately (i.e., implied by economic theory) cannot be varied are "sunk" costs; such costs, of course, are irrelevant to the decision process.

²¹ The preceding statements are not inconsistent with an alternative meaning of indivisibility. As Alchian suggests, the term may be interpreted to cover the phenomenon whereby more durable "dies" result in more than proportional increases in output potential [1, p. 29]. In this sense indivisibility is simply the name given to the sign of certain partial derivatives; it does not explain anything.

The second issue is whether a firm would ever operate in the uneconomic region. The usual argument asserts that a competitive firm "... can never produce in the uneconomic region. For if the price falls below the lowest average variable cost (equals highest average return to the variable factor, given the factor price) it will incur negative quasi-rents"²² [3, p. 305, fn. 2]. A firm facing a negatively sloped demand, however, apparently may do so: "All that is required in order to encompass the uneconomic region ... is to regard some fixed amount of a factor ... as being indivisible during the period in question" [3, p. 304].

If the concept of indivisibility is rejected as a short-run crutch, must output programs within the uneconomic region also be rejected? Given the argument developed in Section II, the answer seems to be negative. As the firm considers alternative adjustment periods, the present value cost of varying one input may be sufficiently greater than the present value cost of varying the other input(s) that the firm may choose to operate in one of the uneconomic regions during some limited time period. If wealth, rather than profit, is used as the relevant criterion, a competitive as well as a monopolistic firm may choose to operate for a time in the uneconomic region—particularly if it is granted that uncertainty may exist regarding the permanence of the change in circumstances inducing the adjustment in output.²³

The preceding comments suggest that, for analytical purposes, it is necessary to show the uniquely determined iso-product curves in each uneconomic region.²⁴ Whether the firm will operate within any such region during a particular short run can only be determined *after* allowing for the appropriate cost and revenue considerations. Furthermore, although at most one such region would be relevant in the two-input case, $n-1$ regions may be relevant in the n -inputs case.

The concept of a priori fixed, indivisible inputs is misleading. Among other things, such an approach masks the implication that a competitive firm may find it profitable to operate for a time within the uneconomic region.²⁵

²² It should be noted that Borts and Mishan are working with a linearly homogeneous production function. Their statements would not be necessarily correct, even in the context of flow analysis, if, for example, the production function in question were homogeneous of degree less than one.

²³ R. H. Coase's suggestion that an input be considered "... as a right to perform certain (physical) actions" [4, pp. 43-44] is a promising point of departure for further investigations in this area.

²⁴ E.g., if the present-value cost of not using some resources is zero, the least cost path for these resources outside the relevant ridge lines would then be straight lines parallel to their respective axes.

²⁵ As Borts and Mishan point out, there is nothing necessarily uneconomic about operating in the uneconomic region.

IV

The traditional approach to the short run as a period in which some inputs cannot be varied is theoretically inadmissible. The substitution of a definition for the corresponding falsifiable hypothesis implied by economic theory masks some empirically relevant issues. In what proportion are inputs to be varied in the short run? Which inputs, if any, are to be held constant? Which regions of the production function are empirically relevant? Why?

The production function must specify which input-output combinations are technologically relevant; cost and revenue information alone can determine which combinations are economically relevant. If it is granted that variation in the input mix is not a free good, then the proportion in which inputs are varied in response to some change in market conditions will depend upon the relative costs and receipts of all the alternative production strategies technologically available to the firm. Moreover, traditional flow analysis yields propositions regarding behavior of the firm in the short run only at the cost of some ambiguities. The wealth-maximizing model suggests that, if "uneconomic" regions exist, they are relevant for competitive as well as for monopolistic firms.

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FORECASTING ERRORS AND BUSINESS CYCLES

By JOHN A. CARLSON*

Evidence has accumulated showing that aggregated forecasts of sales by business men have had systematic errors [9, 10, 11, 14, 16, 18, 26, 27]. In periods of rising sales, business forecasts have tended to understate actual sales. In periods of falling sales, the forecasts have been too high. In other words, changes in sales have typically been underestimated.

Such empirical findings, however, are incompatible with the hypothesis of rational expectations recently developed by Mills [24, 25] and Muth [28]. The rational-expectations argument follows from the assumption that businessmen are maximizing creatures. When it costs a businessman something to make forecasting mistakes, he has reason to avoid them. Thus, if past forecasts have shown systematic errors that can be corrected in some mechanical way, the rational businessman will make the necessary adjustments, and any bias will disappear from his forecasts.¹

What are we to believe, the empirical results or the logic of the theorists? On the one hand, if the theory of rational expectations is relevant to real-world anticipations, then the reported data must have been distorted from what businessmen really expected.² On the other hand, if we accept as a fact the persistence of systematic errors, then businessmen have been either unwilling or else unable to correct for these errors.

We shall take the position in this paper that aggregated forecasts of sales should tend to underestimate changes, no matter how carefully individual forecasts have been prepared. Section I shows that this con-

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¹ This argument has been disputed. Bossons and Modigliani [2, 3] claim that expected profit-maximizing decisions may be perfectly consistent with systematic forecasting errors; and as Theil has shown [32, sect. 3.3], an adjustment that removes bias from forecasts can increase the error variance. If these cases hold, the "rational" businessman may choose to leave his forecasts unadjusted.

² Underestimation of change as revealed by a regression of forecasted levels on actual levels can be reconciled with rational expectations [28]. This particular "regression bias," however, depends upon where one places the assumed error term that links actual and forecasted values. Furthermore, if rational expectations are assumed, there is no such bias to account for the findings that forecasted extrapolations of past changes are smaller than actual extrapolations [3]. Pashigian [31] finds, on the basis of a statistical test with OBE-SEC data, that he cannot reject the rational expectations hypothesis, but his results suggest that the type II error (accept when hypothesis is false) is greater than it would have been had he tested the hypothesis that changes in sales are underestimated by some moderate percentage.

clusion follows from generally accepted macroeconomic theory and from the hypothesis that sales expectations are adaptive. Section II surveys a few studies of sales expectations and finds that the theoretical formulations in Section I are fully compatible with available expectations data.

The argument about rational expectations may be relevant to simple microeconomic situations,³ but we reject the notion that feedback from aggregated individual decisions can be readily enough perceived by the individual decision makers for them to make appropriate adjustments in their forecasts. Section III reports an experiment with a classroom economy in which systematic forecasting errors continued for many periods despite the fact that the structure of the economy was relatively simple.

I. Changing Sales and Forecasting Errors: A Theoretical Link

The typical explanation of the theory of national income determination discusses why an economy should adjust toward its equilibrium level. Once the rationale for the "stability" of aggregative equilibrium has been presented, subsequent focus of theoretical attention is on positions of equilibrium. Theories of the impact of fiscal policy, monetary policy, or shifts of various assumed behavioral equations become exercises in comparative statics. The idea of disequilibrium fades from conscious thought.

In this section we intend to dwell on the possibility of disequilibrium as a useful concept in the theory of business cycles. In so doing we find that the phenomenon of systematic errors in forecasting sales is a direct implication of the theoretical formulation. Our belief that this is a useful way to view the business-cycle process is supported by empirical evidence examined in Section II.

Equilibrium

The essence of our argument can be seen most clearly with a very simple macroeconomic model. Net national product is determined by the output decisions of producers who anticipate selling the goods produced. Suppose capital goods are made to order. Planned net investment then equals the production of new plant and equipment (less capital consumption) plus planned additions to inventories. It follows that net national produce Y_t must equal expected sales by producers of consumer goods E_t plus planned net investment \bar{I}_t :

$$(1) \quad Y_t \equiv E_t + \bar{I}_t$$

³ The author has conducted market experiments in which the participants have had to make supply decisions in anticipation of the market price. In the experiments price adjusted to clear the market in accordance with a demand function not revealed to the participants. In these markets, very rapid convergence did occur even when the structure allowed for an explosive cobweb pattern [5].

It will also be useful later to express (1) in the form:

$$(1a) \quad Y_t - \bar{I}_t \equiv E_t$$

We shall postulate that actual consumption expenditures are a function of net national product and that the marginal propensity to consume is less than one. While any simple analytical function of Y_t is likely to depart from observed values of C_t , the introduction of a stochastic error term will not alter our basic conclusion. We shall therefore retain a deterministic function and assert:

$$(2) \quad C_t = C(Y_t) \text{ such that } dC_t/dY_t < 1$$

Equation (1) states that expected sales and investment plans determine Y_t . Equation (2) then indicates what the resulting level of actual sales will be. Nothing in these two equations guarantees that actual sales will be equal to expected sales. Suppose, however, that sales are forecasted correctly:

$$(3) \quad C_t = E_t$$

This assumption is equivalent to the familiar condition that saving must equal investment. To see this, we can substitute for E_t from (1a) and then manipulate (3) into the form:

$$(4) \quad Y_t - C_t = \bar{I}_t$$

The value of Y_t that satisfies equation (4) is frequently referred to as the "equilibrium" value of net national product and will be denoted by \bar{Y}_t . The value of \bar{Y}_t depends not only on the consumption function but also on \bar{I}_t . Since investment plans may change from period to period as may parameters of the consumption function, we shall refer to \bar{Y}_t as a "temporary equilibrium" to remind ourselves that the achievement of equilibrium does not guarantee that the same level of net national product will be maintained indefinitely [13, 30].

Disequilibrium and Forecasting Errors

A temporary equilibrium exists when actual consumption equals the level anticipated by businessmen. We now consider what happens when these two quantities are not equal. Suppose net national product Y_t is above the temporary equilibrium level \bar{Y}_t . From the assumption that the marginal propensity to consume dC_t/dY_t is less than one, it follows that:

$$C_t < Y_t - \bar{I}_t \quad \text{when} \quad Y_t > \bar{Y}_t$$

After substitution for $Y_t - \bar{I}_t$ from (1a) the preceding statement can be rewritten as:

$$(5) \quad C_t < E_t \quad \text{when} \quad Y_t > \bar{Y}_t$$

By a similar line of reasoning, we can state:

$$(6) \quad C_t > E_t \quad \text{when} \quad Y_t < \bar{Y}_t$$

Assumed Reactions to Disequilibrium

The remaining task is to relate statements (5) and (6) to the phenomenon of business cycles. Our contention is that the expansion phase of a business cycle is usefully viewed as a sequence of periods in which the economy is below a temporary equilibrium level.⁴

When sales have been underestimated in one period (and $Y_t < \bar{Y}_t$), we hypothesize that business forecasters will have higher expected sales in the next period. This hypothesis can be written:

$$(7) \quad E_{t+1} > E_t \quad \text{when} \quad C_t > E_t$$

Statement (7) is a form of adaptive-expectations hypothesis but without a parameter that specifies how much of an adjustment is to be made in expected sales when a forecasting error occurs [29].

When sales have been overestimated, we hypothesize that sales expectations will be adjusted downward. Symbolically:

$$(8) \quad E_{t+1} < E_t \quad \text{when} \quad C_t < E_t$$

We further assume:

$$(9) \quad Y_{t+1} > Y_t \quad \text{when} \quad Y_t < \bar{Y}_t$$

If $\bar{I}_{t+1} \geq \bar{I}_t$, statement (9) follows directly from (1) and (7). If planned investment drops, statement (9) assumes that such a drop does not offset the increased sales expectations.

On the basis of a similar argument we assume:

$$(10) \quad Y_{t+1} < Y_t \quad \text{when} \quad Y_t > \bar{Y}_t$$

The theoretical link should now be apparent. Statements (6) and (9) together imply that sales will be underestimated during expansions of business activity. During the early part of the expansion the temporary equilibrium itself may shift up with increasing planned investment. This, of course, helps to sustain the expansion.

When the economy eventually overshoots its temporary equilibrium the process will be reversed, unless there is some new stimulus to aggregate demand. By statement (5) sales will be overestimated. During the ensuing contraction there should be a sequence of periods in which net

⁴ The author is preparing a paper in which it is argued that this is the approach Hicks had vaguely in mind in his theory of trade cycle [17] but that he never successfully broke away from a dependence on his second-order difference equation, which was based on the assumption that aggregate supply always equals aggregate demand. It should be noted that the lag structure adopted here is in the Lundberg-Metzler tradition [22, 23].

national product falls and sales are consistently overestimated. This argument follows from (5) and (10).

An Example

To help clarify these points, imagine the following hypothetical situation:

Expected sales of consumer goods=2000

Planned additions to inventories=10

Planned net additions to capital=30

In this case production of consumer goods would be 2010, and net national product Y_t would be 2040.

Now suppose that the consumption function is:

$$(11) \quad C_t = 500 + .75Y_t$$

Given the planned net investment of 40, we substitute from (11) for consumption in equation (4) and find that the temporary equilibrium value \bar{Y}_t is 2160. We thus have actual Y_t of 2040 far below its temporary equilibrium. According to statement (6), actual sales should exceed expectations. From equation (11), actual consumption is $500 + .75(2040) = 2030$, which does lie above the expected sales of 2000.

Under these circumstances, what sort of reactions would we expect from the producers? By hypothesis, they will revise upward their sales expectations. Suppose they are conservative and anticipate sales to be about 2020 (a regressive forecast). If planned net investment in capital goods remains unchanged at 30 and if the planned additions to inventory goes up to 30 (in order to make up for the unexpected depletion in period t), then the production of consumer goods will rise to 2050, and net national product to 2080. With the assumed consumption function, sales will be 2060. This implies a forecasting error of 40; and the error would be even greater if investment in equipment were higher.

This underestimation of sales is attributable to the fact that production is below its temporary equilibrium level and not simply to the apparent conservatism of the forecasters. To see this, suppose that businessmen are much more optimistic and estimate that sales will soar to 2120. With a planned net investment of 60, consisting of 30 in inventories and 30 in capital goods, net national product would reach 2180 and the consumption function would call for sales of 2135, still in excess of the forecasted level. This example, of course, requires that goods are available and that capacity exists.

II. Relevant Real-World Data

Studies of sales anticipations have revealed several persistent attributes. Without cataloging all of the findings, we shall note three as-

pects of businessmen's sales forecasts that have relevance to the model under discussion here.

(1) Changes in sales are generally underestimated. This, of course, is the phenomenon for which an explanation has been offered. (2) Forecasters generally have an optimistic outlook, i.e., errors have been greater when sales are falling. (3) Forecasts have sometimes been regressive, i.e., businessmen do not invariably extrapolate changes in sales; they often appear to expect a reversal of the recent trend.

Change Understatement and Level Optimism

Ferber [9] [10] [11] and Hultgren [18] present details of the railroad shippers' forecasts. All three of the foregoing attributes emerge dramatically from these data. Modigliani and Sauerlender [26] find that the systematic errors (understatement of change) occur not only in the railroad shippers' data but also in surveys by Dun and Bradstreet, by *Fortune*, and by the Department of Commerce and the Securities and Exchange Commission.⁵ Of these, only the *Fortune* surveys lack a general upward bias to the level of forecasted sales. In other words, while changes in sales are underestimated, the errors are usually much greater when sales are falling than when they are rising. In this connection Ferber has postulated that business forecasters possess a "permanent optimism," i.e., a strong belief in an upward trend in sales.

One other quarterly survey of anticipations was begun in the second quarter of 1959 by the Office of Business Economics (OBE) in the Department of Commerce. The anticipations and corresponding actual sales (and inventories) of U.S. manufacturers of durable goods and non-durables are published quarterly in the *Survey of Current Business*. Foss [12] discusses the survey and provides some preliminary analysis of the data through the fourth quarter of 1961. Additional information is provided by Lovell [21].

An indication of the general optimism of sales forecasters is conveyed in an observation by Foss [12, p. 236]: every seasonally adjusted forecast of sales six months ahead is above the sales of the most recently completed quarter. We have carried the data through 1966 and this record is still unbroken.

The period from 1959 to 1966 can be divided into six quarters in which

⁵ Cooper and Simon [7] suggest that there must be either a statistical or psychological bias introduced into some of these survey results. Lovell [20] has proposed another possible source of error in the railroad shippers' forecasts. If the shippers do have an internal sales forecast, then errors in their derived carloading forecasts can result from the fact that not everything the shippers sell is shipped by rail. As support for this hypothesis, Lovell finds, in the cement industry for the period 1947-1956, that sales anticipations derived from reported carloading forecasts are more highly correlated with actual sales than are forecasted carloadings with actual carloadings [20, p. 218].

sales fell and 23 in which sales rose over the preceding quarter. Falling sales occurred in the last two quarters of 1959, presumably because of a steel strike and in the four quarters from early in 1960 to early in 1961. The average decline in sales over these six quarters was 1.57 billions of dollars per quarter. For the same six quarters the three-month forecasts indicated that the average expected decline was only .55 billions of dollars per quarter. Over the 23 quarters of rising sales the increase averaged 2.44 billions of dollars per quarter, and the average three-month forecast was for a rise of 2.10 per quarter. Thus, there is some indication that changes in sales are still being underestimated, although the errors are small compared with earlier data. In keeping with Ferber's "permanent optimism" hypothesis, the errors tend to be greater when sales are falling.

Regressiveness

Regressiveness has been analyzed in detail by Bossons and Modigliani [1] [2]. As they use the term, regressiveness refers in our notation to the following situation:

$$\begin{aligned} E_t < C_{t-1} & \text{ when } C_{t-1} > C_{t-4}, \text{ or} \\ E_t > C_{t-1} & \text{ when } C_{t-1} < C_{t-4} \end{aligned}$$

Since surveys are usually conducted quarterly, we shall say that a forecast for quarter t is regressive if the trend in sales over the last three quarters is expected to be reversed in the forthcoming quarter. The phenomenon of regressiveness is most evident in both the railroad shippers' forecasts and the Dun and Bradstreet data.

One reaction to these findings is reproduced here because the views expressed may foster a spurious objection to the expectations hypothesis presented in Section I of this paper. Hart and Kolin write [15, p. 261]:

Now if expectations are really regressive in this sense, we must infer that the whole course of a business upswing is a series of agreeable surprises, which keep things going up despite a tendency of business to reverse the upswing by decisions to cut back inventory, staffing, procurement, and the like. Admitting that it is hard to systematize the use of business-cycle evidence in this connection, we assert that our preference for nonregressive models is not sentimental. Rather, it reflects the large body of facts which lead most analysts of fluctuations to give much weight to "cumulative" elements in expansion and contraction, and to doubt the inherent stability of the business process. In the light of these facts, how can expectations simultaneously be regressive and be linked up with the decision processes which generate business fluctuations?

These remarks give the impression that we must choose between "regressive" tendencies and the view that business cycles are cumulative

processes. This is simply not true. The confusion arises because Hart and Kolin are comparing E_t with C_{t-1} instead of with E_{t-1} . If sales have been rising for a number of quarters and we find regressiveness, i.e., $E_t < C_{t-1}$, this need not imply $E_t < E_{t-1}$. As long as businessmen's expected sales rise over expected sales of the preceding quarter, an expansion can well continue despite regressive forecasts. The example in Section I included just such a case. Similarly there could be regressive expectations in every period of a decline and at the same time we might find $E_t < E_{t-1}$ throughout.⁶

A Test of Adaptive Sales Expectations

The hypothesis that sales expectations in the aggregate are adjusted adaptively is a key part of our theoretical link between forecasting errors and business cycles. In order to classify all possibilities, including cases in which forecasts happen to be correct or there is no change in the level of the forecast, we need to modify (7) and (8) slightly. Restated, our hypothesis takes the form:

$$(7') \quad E_{t+1} > E_t \quad \text{when} \quad C_t \geq E_t$$

$$(8') \quad E_{t+1} \leq E_t \quad \text{when} \quad C_t < E_t$$

Our choice of location of the weak inequalities is based on the hypothesis of "permanent optimism." If a forecast has been correct and the forecasters are generally optimistic, their sales expectations will be revised upward. By a similar line of reasoning, we assume that a forecast of no change in sales will follow an overestimate of sales rather than an underestimate. The negation of the hypothesis includes either of the following two cases:

$$(12) \quad E_{t+1} > E_t \quad \text{when} \quad C_t < E_t$$

$$(13) \quad E_{t+1} \leq E_t \quad \text{when} \quad C_t \geq E_t$$

Available data can now be put into cells of a 2×2 contingency table in order to test our expectations hypothesis. Table 1 contains a classified count of forecasts from the seasonally adjusted series that Hart [14] has

⁶ Nerlove [29, pp. 216-19] points out how regressive and adaptive expectations may be reconciled but he does not relate it to cumulative processes. In the appendix we show how Metzler's inventory models can incorporate adaptive expectations. Certainly, if the output of consumer goods is based on E_t in period t , it should have been based on E_{t-1} in period $t-1$. The relationship between investment in capital goods and expected sales of consumer goods is less clearcut. If the desired stock of capital goods is always proportional to expected sales and producers invest just enough to reach the desired stock, then planned investment depends directly on the difference $E_t - E_{t-1}$. This may be a naive formulation of the accelerator but it suggests that the size of E_t relative to E_{t-1} can influence investment decisions as well as decisions to change production. There is an evident need for more research into the link between stated expectations and actual decisions. A notable attempt is in the latter part of the paper by Modigliani and Sauerlender [26]. See also [4, 8, 19, 21].

TABLE 1—RAILROAD SHIPPERS' QUARTERLY FORECASTS SEASONALLY ADJUSTED, 1927-1939
NUMBER OF FORECASTS IN EACH CATEGORY

| | $E_{t+1} > E_t$ | $E_{t+1} \leq E_t$ |
|----------------------------------|-----------------|--------------------|
| Underestimates $C_t \geq E_t$ | 17 | 3 |
| Overestimates $C_t < E_t$ | 4 | 25 |

Source: Hart [14], Table A-1, pp. 232-33.

constructed from the railroad shippers' forecasts. Even with these highly regressive forecasts, a significant preponderance of cases conforms to our hypothesis.⁷

TABLE 2—DEPARTMENT OF COMMERCE QUARTERLY SURVEY OF SALES EXPECTATIONS
SEASONALLY ADJUSTED 1959-1966 NUMBER OF FORECASTS IN EACH CATEGORY

| Manufacturers of Nondurables | | |
|--------------------------------|-----------------|--------------------|
| | $E_{t+1} > E_t$ | $E_{t+1} \leq E_t$ |
| $C_t \geq E_t$ | 19 | 0 |
| $C_t < E_t$ | 3 | 7 |
| Manufacturers of Durable Goods | | |
| | $E_{t+1} > E_t$ | $E_{t+1} \leq E_t$ |
| $C_t \geq E_t$ | 14 | 0 |
| $C_t < E_t$ | 5 | 10 |

Source: U.S. Dept. of Commerce, Office of Business Economics; and March, June, Sept., and Dec. issues of the *Survey of Current Business*. The forecasts for the fourth quarter of 1963 have been omitted because of a modification of the series at that point.

Table 2 contains a similar classification of more recent data. The seasonally adjusted sales forecasts of manufacturers of durable goods and nondurables have been considered separately. These data also tend to confirm our hypothesis. The pattern is again highly significant statis-

⁷ See *Tables for Testing Significance in a 2×2 Contingency Table*, compiled by D. J. Finney, *et al.*, Cambridge: University Press, 1963. These are tabulated on the basis of the hypergeometric distribution. Let the null hypothesis be that the probability of $E_{t+1} < E_t$ is independent of the relationship between E_t and C_t . With the railroad shipper's data, we note that of the 29 observations with $C_t < E_t$, 25 were followed by a lower forecast. According to the tables, at a .005 significance level, we can reject the null hypothesis if 9 or fewer of the 20 observations of $C_t \geq E_t$ were followed by $E_{t+1} < E_t$. In this sense 3 observations may be viewed as highly significant.

tically. Interestingly, the only adjustments contrary to the hypothesis occur because of the optimistic outlook of the forecasters. In none of these quarters were sales expectations lowered following an underestimate of actual sales.

III. *Experimental Evidence*

An advocate of the use of rational expectations in connection with sales forecasts, Mills [25, p. 34], has written:

In other words, if the economists' theory implies that a variable is generated by a particular process then he should not assume that expectations of the variable are formed in a way inconsistent with this process. This is particularly important if, as in the usual model, the variable is generated by a relatively simple mechanism that an intelligent decision maker, who is familiar with the series, can estimate.

A classroom economy developed and used by the author provides an interesting vehicle for examining Mills' contention.⁸ We shall present only a brief sketch here. For those interested, much greater detail is available elsewhere [6].

The basic plan of the classroom economy is to install students as managers of hypothetical firms producing an all-purpose consumer good, and ask them to make decisions for a sequence of periods. At the beginning of a period, a firm has a certain stock of capital equipment. The production function calls for fixed proportions of equipment and labor. Since the labor supply is assumed to be perfectly elastic, the production decision boils down to a judgment about how much of the firm's available productive capacity to utilize. At the beginning of a period, the managers must also decide how much new equipment to order for future use. Since equipment is assumed to deteriorate, the level of net investment in equipment can be negative as well as positive. There is also a rule that a firm's stock of equipment may not be expanded by more than a certain percentage in any one period.

In both experiments in which data on sales expectations were systematically collected, there were twenty firms. Two units of capital were required for each unit of output. Capital deteriorated at a rate of 2 per cent per period and could not be expanded at a rate of more than 4 per cent per period. Letting K_t stand for the stock of capital equipment at the beginning of period t , X_t for the production of consumer goods, and I_t for net investment in equipment, the two decision variables X_t and I_t were constrained as follows:

⁸ This course was taken by candidates for a Masters Degree in Industrial Administration at Purdue University during the Spring semester, 1965. One class was taught by William Breen and the other by the author. In each class there were 37 students and 20 firms. For each group of 20 firms, 17 had two managers and 3 had only one manager.

$$X_t \leq .5K_t$$

$$-.02K_t \leq I_t \leq .04K_t$$

The production and investment decisions create income, and income in turn generates demand for consumer goods. The consumption function, which was not revealed to the class except to the extent that it could be inferred from the national income accounts, took the following form:

$$C_t = 720 + 12t + .7(X_t + I_t)$$

where C_t denotes "potential sales." In any period C_t is a linear function of national income, but from one period to the next the function shifts upward.

Each firm shares equally in the market. If all firms have enough available for sale, actual consumption equals potential sales. If some firms have too little available, they lose sales that period. If a firm has more available than its share of the potential sales, then the unsold goods are carried over as inventories until the next period. Since prices do not change, this is a pure quantity-adjustment model.

The classroom economy also specifies a number of cost parameters. The most important to consider here are the cost of holding inventories and the cost of holding one unit of productive capacity for one period. The cost of capacity is determined by the depreciation and interest charges. The larger this capital cost is relative to the cost of holding a unit in inventory, the greater is the incentive to use capacity fully and temporarily build up inventories when demand falls below full-capacity production.

Two classroom economies designated Economy A and Economy B were given precisely the same instructions and had the same parameters, with the exception of the cost of holding inventories. In Economy A it paid to produce a marginal unit for inventory only if that unit could be sold within five periods. In Economy B, the critical interval was ten periods. Given the level of production, a change in expected sales must be matched by an opposite change in planned additions to inventory. If inventory costs are relatively inexpensive, then production may not be quickly adjusted to any revised sales estimates; but if managers are concerned about undesired inventory changes, production will be changed directly, if possible, with any change in expected sales, and the income effect of these decisions may vitiate the forecasts.

In the two classroom economies, a depression normally lasted three or four periods and expansions typically took from five to eight periods. It took about six or seven periods for a preceding peak in the level of production and investment to be surpassed. With these conditions, our theory suggests that the managers of firms in Economy A were bound

to make systematic underestimates of changes, since the rational policy was to be very much concerned about inventory buildup.

There was slightly more chance that the managers of firms in Economy B might forecast correctly and then not adjust production so as to thwart the sales forecasts. Since it was almost always rational to use capacity fully in this economy, a knowledge of capacity, a good guess as to net investment, and a fair approximation to the consumption function would enable a manager to make accurate forecasts. Beginning in period 10, firms submitted sales forecasts. When they made their production and investment decisions for a period, the managers were to record their forecasts of potential sales for the current period and for one additional period into the future.

One striking difference between the experimental data and sales forecasts by U.S. manufacturers is that the classroom forecasts are clearly extrapolative with some sensitivity to changes in the rate of change in sales and are adaptive only in the same sense that the sales expectations posited by Metzler [23] are often adaptive.⁹ An explanation is that in the classroom economy forecasters can be reasonably sure that, if their firm needs to increase or decrease production, most other firms do too. Sales can be extrapolated with confidence, since the managers have experienced the effect on sales from their own typical output decisions. In the real world there is much less certainty of any feedback on sales when a "typical" firm responds to a felt need to expand or contract its operations. It is also harder to tell in which direction the temporary equilibrium might be.

Despite their awareness of the interdependence between production and sales, the managers in the classroom economy rarely anticipated the full impact. The cases of underestimation of sales were almost always found during expansions, and overestimates generally occurred in contractions. To show the extent of this, we have prepared Table 3 in a manner similar to Tables 1 and 2. The difference is that we have related changes in Y , as posited by equations (9) and (10), to positions of disequilibrium ($C_t \neq E_t$).

The pattern for these experimentally generated data is, like the real-world data, in conformity with our hypothesis. In this case what contradictions there were occurred within a period or two of turning points. It is also interesting to note that most of the observations that do not

⁹ The relationship between adaptive expectations and the expectations function assumed by Metzler is examined in the appendix to this paper. The expectations data generated in the classroom economy are analyzed in [6]. The data in both economies had a remarkably close fit to approximately the following function:

$$E_{t+1} - C_t = 7 + .7\Delta C_t + .2\Delta^2 C_t$$

where $\Delta C_t = C_t - C_{t-1}$,
 $\Delta^2 C_t = \Delta C_t - \Delta C_{t-1}$

TABLE 3—TWO-WAY CLASSIFICATION OF FORECASTS GENERATED EXPERIMENTALLY IN TWO CLASSROOM ECONOMIES

| | Economy A | |
|---------------------------------|-----------------|--------------------|
| | $Y_{t+1} > Y_t$ | $Y_{t+1} \leq Y_t$ |
| Underestimate $C_t \geq E_t$ | 15 | 1 |
| Overestimate $C_t < E_t$ | 2 | 10 |
| | Economy B | |
| | $Y_{t+1} > Y_t$ | $Y_{t+1} \leq Y_t$ |
| Underestimate $C_t \geq E_t$ | 13 | 0 |
| Overestimate $C_t < E_t$ | 5 | 9 |

conform to our hypothesis are cases of increasing production after an overestimate of sales. The upward shift of the consumption function imparted growth into the system. The recognition of a growth trend in the classroom economy may have led the participants to prefer an error on the optimistic side, an experimental re-creation perhaps of Ferber's "permanent optimism." One thing these experimental results demonstrate is that some of the special difficulties encountered in actual surveys are not necessary for a systematic bias in the forecasts. There was no possibility of seasonality. There was a single product and no price changes. Hence, no ambiguity could arise from these sources.

Most of Mills' criteria for rationality in expectations were met by the classroom economy. The generating mechanism process was simple. There were no shifts in spending because of net foreign demand, government spending, or varying psychological propensities of consumers. The consumption function contained no stochastic component. The students were familiar with the series since national income data were reported after every period. They had plenty of time to think about the data since only one decision was made each time the class met, and there were a number of assigned problems asking them to analyze various aspects of the economy. The students were intelligent; and yet they had not settled on the long-run equilibrium path in almost forty periods.

In one sense we agree with Mills. Forecasts are not independent of experience. A decision maker who becomes aware of systematic errors in his forecasts is likely to make some adjustments in his forecasts if it is profitable to do so; but as long as the production decisions are related to

the forecasts and changes in sales are induced by changes in production and income, there is no guarantee that the forecasts will lose their bias.

There is some evidence that the managers in the classroom economies did change their forecasts. They became more attuned to changes in the rate of change in sales as an indication of turning points. In Economy A, in particular, the cycles were occurring with increasing frequency as managers were trying to avoid past mistakes. Perhaps these classroom economies would eventually have settled onto the equilibrium path, but this is by no means certain. If it proved so difficult to settle onto a steady growth path when there was a simple structure and relatively complete information, then it is not surprising, accepting our theory, that systematic forecasting errors occur in an economy whose structure is not easy to perceive and whose decision makers are frequently changing.

The proponents of rational expectations emphasize that forecasting errors will occur, but that the mathematical expected value for the errors ought to be zero and not be autocorrelated. A similar point should be underscored with regard to our claim about systematic forecasting errors. We generally expect the average forecast of a change in sales to be in error; but there is no reason why managers with sufficient information and insight cannot come up with unbiased forecasts. One firm in Economy A was managed by two students who did perceive what was going on more clearly and earlier than anyone else. Because of their understanding of the overall economic process, their firm was the runaway winner in terms of accumulated net worth. But if all managers had behaved as these two, the sales pattern would have been altered and a different set of decisions and predictions would have been more nearly optimal.

IV. *Conclusion*

The theoretical framework developed in this paper is consistent with a broad class of consumption functions and investment functions. A key expectations hypothesis has also been confirmed by available data. Thus, the oft-noted tendency of businessmen to underestimate changes in future sales does not require a statistical or psychological explanation. It is to be expected on the basis of economic theory.

APPENDIX: METZLER'S INVENTORY MODELS WITH ADAPTIVE EXPECTATIONS

In our description of the business cycle process we have stressed the nature of reactions to disequilibrium and have not tied ourselves to any rigid sequence or functional statement of the expectations hypothesis. We shall now assume a linear adaptive-expectations function and examine how it differs from the expectations function used by Metzler [23].

Let C_t denote actual sales and E_t expected sales. In parametric form a linear adaptive-expectations function can be written:

$$(A.1) \quad E_{t+1} = E_t + \rho(C_t - E_t) \quad 0 < \rho < 2$$

Metzler's expectations function has the form:

$$(A.2) \quad E_{t+1} = C_t + \eta(C_t - C_{t-1}) \quad -1 \leq \eta \leq 1$$

It is crucial to our description of business cycles as cumulative affairs that ρ be positive. The postulated upper limit of two is arbitrary.¹⁰ It corresponds roughly to Metzler's upper limit of one on η . When $\rho=1$ and $\eta=0$, the two hypotheses come to the same thing: naive expectations.

When $-1 \leq \eta < 0$, businessmen are predicting a reversal in the direction of the most recent change in sales. Let us denote such a forecast as *regressive*. When $0 < \eta \leq 1$, sales are expected to change again in the same direction as the most recent change. This sort of forecast may be called *extrapolative*.

Given the last forecast and the most recent change in sales, values of the coefficient of adaptive expectations ρ can be selected to yield either regressive or extrapolative forecasts. Suppose that $C_t > C_{t-1}$ and $C_t > E_t$ or that both inequalities are reversed. In those cases $0 < \rho < 1$ implies regressive forecasts and $1 < \rho$ implies extrapolative forecasts. Since our theory claims that $C_t > E_t$ during expansions and $C_t < E_t$ during contractions, the two functions can frequently be used to generate the same qualitative forecasts. It follows that the expectations assumed by Metzler are also adaptive much of the time.

At turning points, or whenever the cases specified above do not hold, the relationship is reversed. When $C_t > C_{t-1}$ and $C_t < E_t$ or when $C_t < C_{t-1}$ and $C_t > E_t$, $1 < \rho$ produces regressive forecasts, and extrapolative forecasts occur with $0 < \rho < 1$. Thus, not all forecasts of direction of change in sales generated under one hypothesis can be reproduced under the other if the coefficients of expectation are assumed to be constant over the cycle.

To show how the formal models differ we shall retain all of Metzler's assumptions and then substitute (A.1) for (A.2). Assuming the expectations function (A.2), Metzler has developed three different dynamic sequences corresponding to three different assumptions about inventory policy. We designate these below as cases 1, 2, and 3. They correspond to Metzler's third, fourth and sixth sequence, respectively.

Actual sales, or consumption, is assumed to be a linear function of current income Y_t :

$$(A.3) \quad C_t = A + bY_t \quad 0 < b < 1$$

The constant A in this function plays the same role as Metzler's autonomous investment v_0 .

¹⁰ In one sense these limits are not arbitrary. The function (A.1) can also be written as a series expansion:

$$E_{t+1} = \rho \sum_{n=0}^{\infty} (1-\rho)^n C_{t-n}.$$

Any wider limits than $0 < \rho < 2$ and this would not be a convergent series.

Y_t is generated by two components, production in anticipation of sales E_t and planned net investment in inventory I_t :

$$(A.4) \quad Y_t = E_t + I_t$$

Case 1: Passive Accumulation of Inventories

If businesses let inventories rise or fall depending on whether sales have been overestimated or underestimated and make no effort to offset these changes, then:

$$(A.5) \quad I_t = 0$$

Equations (A.2)–(A.5) correspond to Metzler's third model. By substitution they reduce to the following second-order difference equation:

$$(A.6) \quad Y_t - (1 + \eta)bY_{t-1} + \eta bY_{t-2} = A$$

As in all of the models considered in this appendix the equilibrium solution is $A/(1-b)$. The stability conditions for (A.6) require:

$$-(1 + b)/2b < \eta < 1/b$$

Since Metzler postulates $0 < b < 1$ and $-1 \leq \eta \leq 1$, instability is impossible.

Changing to adaptive expectations, the system of equations (A.1), (A.3)–(A.5) reduces to the following first-order difference equation:

$$(A.7) \quad Y_t - (1 - \rho + \rho b)Y_{t-1} = \rho A$$

The stability conditions can be expressed:

$$0 < \rho < \frac{2}{1 - b}$$

The two models have certain similarities. Both can give rise to a damped cobweb approach to equilibrium. In Metzler's model this may occur when forecasts are regressive ($\eta < 0$); for this to occur with (A.7) expectations must be strongly adaptive ($1/(1-b) < \rho$). Both have a portion of the parameter space in which there is a steady approach to equilibrium. The main difference is that (A.6) is a second-order difference equation. The characteristic equation can have complex roots and give rise to a cyclical pattern. This cannot occur with (A.7).

Case 2: A Desired Level of Inventories

Metzler has shown that if businesses try to maintain some fixed level of inventories, planned investment in inventories will always be equal to the difference between actual and expected sales in the prior period.

$$(A.8) \quad I_t = C_{t-1} - E_{t-1}$$

The four equations (A.2)–(A.4), (A.8) represent Metzler's fourth sequence. The resulting difference equation is:

$$(A.9) \quad Y_t - (2 + \eta)bY_{t-1} + (1 + 2\eta)bY_{t-2} - \eta bY_{t-3} = A$$

and the stability conditions are:

$$-\frac{1+3b}{4b} < \eta < \frac{1}{2b}$$

If we substitute adaptive expectations (A.1) for (A.2) in the foregoing model and manipulate the equations to obtain a function of Y , there is a new difference equation:¹¹

$$(A.10) \quad Y_t - (1 - \rho + b + \rho b)Y_{t-1} + bY_{t-2} = \rho A$$

with the stability conditions:

$$0 < \rho < \frac{2(1+b)}{1-b}$$

The characteristic equation of (A.10) has complex roots if

$$\frac{(1 - \sqrt{b})^2}{1 - b} < \rho < \frac{(1 + \sqrt{b})^2}{1 - b}$$

Therefore, cycles can arise with $b > 0$.

In Metzler's model unstable cycles occur if $b > .5$ and η is sufficiently large. The larger the value of b , the smaller the range of values of η that result in a damped approach to equilibrium. Just the opposite is true with adaptive expectations. The larger the value of b , the greater may ρ be without having instability. Actually, within the postulated ranges $0 < \rho < 2$ and $0 < b < 1$, instability cannot arise with adaptive expectations in this model.

Case 3: A Desired Ratio of Inventories to Sales

Suppose now that businesses seek to maintain a constant ratio of inventories to expected sales. In that case it is necessary to add to equation (A.8) the difference between the desired level of inventory in period t and the desired level in period $t-1$. Planned investment in inventories then becomes:

$$(A.11) \quad I_t = C_{t-1} - E_{t-1} + \alpha(E_t - E_{t-1})$$

Metzler's sixth and most complicated model consists of equations (A.2)-(A.4), (A.11). These reduce to the following third-order difference equation:

¹¹ Both (A.10) and (A.13) below can be obtained by substitution, but the appropriate trick is not readily apparent. A more straight-forward method is to rewrite the system of equations in matrix form by using the operator E , i.e., $EY_t = Y_{t+1}$, and solve by matrix inversion. For example, with this procedure one obtains:

$$Y_t = \frac{\rho A}{E^3 - (1 - \rho + b + \rho b)E + b}$$

which is equivalent to (A.10).

$$(A.12) \quad Y_t - [(1 + \eta)(1 + \alpha) + 1]bY_{t-1} + (1 + 2\eta)(1 + \alpha)bY_{t-2} - (1 + \alpha)\eta bY_{t-3} = A$$

In this model, stability requires

$$\alpha < \frac{3(1 - b)}{2b}$$

no matter what the coefficient of expectations. There is then the further restriction on the coefficient of expectations:

$$-\frac{1 + 3b + 2\alpha b}{4b(1 + \alpha)} < \eta < \frac{1 - (1 + \alpha)b}{(1 + \alpha)b[2 - (2 + \alpha)b]}$$

Both ends of this range approach -1 as α approaches $3(1 - b)/2b$.

The corresponding model with adaptive expectations contains equations (A.1), (A.3), (A.4), (A.11). By substitution, we obtain a second order difference equation:

$$(A.13) \quad Y_t - (1 + b - \rho + \rho b + \alpha \rho b)Y_{t-1} + (1 + \alpha \rho)bY_{t-2} = \rho A$$

with the stability conditions:

$$\begin{aligned} 0 < \rho < \frac{1 - b}{b\alpha} & \quad \text{for } \alpha > \frac{(1 - b)^2}{4b} \\ 0 < \rho < \frac{2(1 + b)}{1 - b - 2\alpha b} & \quad \text{for } \alpha < \frac{(1 - b)^2}{4b} \end{aligned}$$

Metzler observed that the introduction of a desired ratio of inventory to sales severely restricts the range of parameter values for which the system will tend to equilibrium. The same conclusion holds with adaptive expectations. Unlike Metzler's model, stability is still possible with large values of α and b , but this restricts ρ to an implausibly small range. The destabilizing potential of this inventory policy is not appreciably mitigated by adaptive expectations.

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THE DEMAND FOR HIGHER EDUCATION IN THE UNITED STATES, 1919-1964

By ROBERT CAMPBELL AND BARRY N. SIEGEL*

This is a study of the demand for higher education in the United States for the period since World War I. The study is based upon aggregate enrollment data and it uses the rather common economic variables of income and price (tuition) to explain the movements of demand during the period. To anticipate our conclusions, we have found that these two variables explain some 87 per cent of the variation of demand for higher education as we have measured it. In addition, we have found that demand responds positively to changes in income and negatively to changes in price. We believe that these results have important implications for the theory of educational demand and for the conduct of national educational policy.

I. Theoretical Basis of Demand Estimates

Our results make sense only if we can put them in touch with a reasonable theory of educational demand. There are two not necessarily mutually exclusive approaches to such a theory. One approach views the decision to enroll in an institution of higher education as an investment decision, while the other views it as a current consumption decision. Both of these approaches can be integrated for the purposes of this paper.

The investment approach¹ to the theory of educational demand asserts that an individual will purchase a college education if the present value of the expected stream of benefits resulting from the education exceeds the present cost of the education. The expected stream of benefits includes two elements: the additional lifetime money income resulting from higher education and the additional social and intellectual amenities which a person might expect to receive as a result of having gone to college. The first benefit makes a college education similar to a producer durable, while the second causes it to resemble a consumer durable, whose yield is real rather than monetary.

The cost of a college education must also be viewed rather broadly. It includes direct money outlays in the form of tuition and fees, books,

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¹ Now widely adopted. See in particular T. W. Schultz [7] and Gary Becker [1]. A useful defense of the approach is given by Mark Blaug [2].

differential living costs, and other outlays incident to going to school. An enrollment also entails indirect financial burdens in the form of opportunity costs, as measured by the loss of income incurred while in school. Finally, there are nonmonetary costs which include such things as the burden of study and, for some students, the pain of being away from home.

As with other investments, purchase of an education entails risks. The expected stream of benefits may fail to materialize, either because the student may fail to complete his education or because expected post-educational opportunities may not materialize.

An individual facing an enrollment decision presumably makes his decision in the light of the expected benefits and costs involved in an enrollment. For any given set of benefits and costs there will be an implied rate of return on the educational investment. The rate of return will be composed of two parts, one relating to the stream of monetary benefits and costs and another relating to the nonmonetary benefits and costs. Conceptually, these two elements are incommensurate; hence, we must imagine that the individual places some sort of monetary valuation upon the real part of the stream of benefits and costs. Whether or not individuals actually do this is, of course, an open question. Indeed, it is difficult to imagine how, in principle, such valuations can be made. The value to the individual of the stream of expected real benefits should equal the minimum compensation the individual would accept in order to induce him to give up those benefits but still undertake his education. In a similar vein, the real costs of an education may be valued by the maximum amount of money a person would pay to avoid them, while still expecting to receive the future stream of benefits. Such a procedure implies that real benefits can be sold and that relief from real costs can be bought. It is hard to see how this can be done. Fortunately, it is not our purpose here to measure the expected rate of return on an educational investment. Nevertheless, since such a concept is crucial to the investment approach, we shall assume that individuals act according to estimates of such a rate of return, with due allowance for the riskiness of their investments.²

Ordinary investment theory would have the individual compare his expected rate of return with some appropriate interest rate. An education will be purchased if the expected rate of return exceeds the rate of interest. The education will not be purchased if the interest rate exceeds the rate of return. An aggregate demand schedule for enrollments may then be derived by arraying individuals according to their expected rates of return (from highest to lowest). The total number of enrollments

² Mark Blaug also argues for such an assumption [3]. This article appeared after we had completed this paper.

demand will equal the aggregate of all enrollments for which the rate of return exceeds the rate of interest. Variations in the rate of interest will lead to inverse variations in the number of enrollments demanded.

This theory of enrollment demand depends heavily upon the assumption that loan capital will be available to all those who wish to purchase an investment. Although the situation is changing rapidly, this assumption is a bad one. The supply of credit for educational lending is still highly imperfect, and most individuals must depend upon personal or family resources, personal loans, or gifts and scholarships. For this reason, we should expect the actual demand schedule for enrollments to be below the schedule described above, since there will be many profitable educational investments which cannot be made for lack of finance. We should also expect aggregate demand to be relatively insensitive to changes in market rates of interest.

Variations in enrollment demand are related to variations in the factors affecting the expected rate of return. A general rise in expected money income, or a reduction in the uncertainty of acquiring the income, from educational investments, should increase the demand for higher education. An increase in the cost of educational investments, either in the form of an increase in opportunity costs or in the form of increased direct money outlays, should lead to a reduction in educational demand. Finally, because people must substitute personal for borrowed resources, we may expect the demand for higher education to vary directly with disposable family or household income.

Turning to the consumption approach to educational demand, we have already argued that a college education can be considered as a consumer durable, yielding a stream of future services over the lifetime of the individual. It is also true that an enrollment may bring current consumption benefits. These benefits include the many social, intellectual, and athletic activities available in most colleges and universities in the United States. We cannot directly measure the value of these benefits, but we may suppose that it is in some approximate sense measured by the outlays an individual would have to make to buy a substitute bundle of goods and activities outside of college. This method of valuation implies that the cost of current consumption benefits varies directly with the prices of consumer goods in general.

The consumption approach supplements and enriches the investment theory of educational demand in several ways. First, it allows us to offset part of the current costs of enrollment with a positive current benefit. Second, it allows us to strengthen the predicted effect of income upon the demand for enrollments. If both future and present consumption benefits from education are normal goods, an increase in income should lead directly to an increase in enrollment demand. This income effect is

quite separate from the earlier effect mentioned in connection with the lack of loan capital.

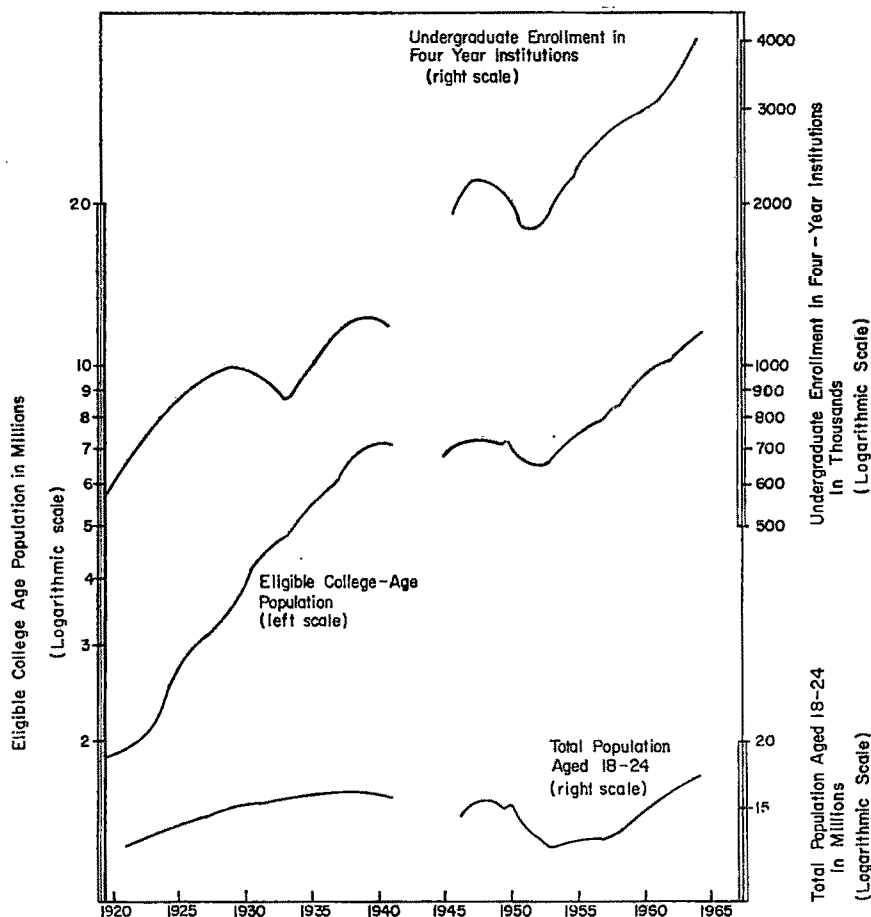
Finally, the consumption approach allows us to argue that educational demand is sensitive to the money costs of an enrollment relative to current consumer goods prices. For, consider a rise in the price level of all consumer goods relative to current tuition charges and other enrollment costs. Such a rise implies a reduction in the cost of the future consumption benefits from an education relative to the cost of present consumption goods in general. Conventional demand theory suggests that such a reduction will lead to the substitution of future consumption (through increased enrollments) for present consumption. An additional effect, working in the same direction, arises from the fact that the current consumption benefits from enrollments are valued by their substitutes among other consumer goods. The increase in consumer goods' prices raises the value of the current consumption benefit from enrollments. Since the value of the current consumption benefit offsets the money and real costs of an enrollment, the net cost of enrollments should fall. This, in turn, works to raise the expected rate of return from education and, hence, to increase enrollment demand.

In summary, our theory argues that, for a given population, enrollment demand should vary positively with expected money and real yields from education, positively with income, positively with the consumer price index, and negatively with the money and real costs of education. We now proceed to the empirical specification of the demand model.

II. *Measurement of Demand*

Enrollment data provide the most obvious source of information on demand. It is customary to express enrollment as a ratio of some relevant population group in order to measure the intensity or pervasiveness of demand for higher education within the relevant group. The U.S. Office of Education, for example, publishes widely used ratios of enrollments of various types (undergraduate, graduate, etc.) to both the 18-21 and the 18-24 year old age groups [8, p. 76]. While such ratios may be useful for some purposes, they are not useful for ours. We need a measure which relates enrollments to those in a position to choose to go on to college at any particular point in time. People lacking a high school diploma or its equivalent, for example, are rarely in a position to enter an institution of higher education of any sort. Neither is it possible for people who are institutionalized, immobilized for reasons of health, or who are members of the armed forces to go to college, even if they possess high school diplomas. It is for these reasons that we have chosen to measure demand with the ratio of undergraduate enrollment to those in

CHART 1. UNDERGRADUATE ENROLLMENT IN FOUR YEAR INSTITUTIONS, ELIGIBLE COLLEGE AGE POPULATION, AND TOTAL POPULATION AGED 18-24 1919-1964 (WORLD WAR II YEARS EXCLUDED)



Sources: See Appendix.

the 18-24 year age group who possess high school diplomas and who are not in the armed forces. We shall call this group the *eligible college age population*³ or, for short, the "eligibles."

Chart 1 plots the movements of enrollments, eligibles, and the 18-24 year population on a semilogarithmic scale for the years 1919 to 1964. For reasons which will be explained below, we have chosen to use resident

³ We use the 18 to 24-year old age group because it seems to contain about 80 per cent of the undergraduate population. See A. Cartter and R. Farrel [4, p. 120]. We were not able to obtain data by age of the institutionalized population. For our methods of estimating the "eligibles," see the appendix at the end of the paper.

undergraduate degree enrollment in four-year institutions as our measure of demand. Movements of the enrollment ratio as we would measure it can be inferred from the vertical distance between the top two series. The ratio published by the Office of Education is measured by the vertical distance between the top and the bottom series. The chart shows that the ratio of enrollment to the total 18-24 year group has risen continuously and extensively since 1919, whereas the ratio of enrollment to the eligible college age population has had no trend during the same period.⁴ The rise in the Office of Education ratio is evidently due to the increase over the period in the proportion of people in the 18-24 year group who have completed a high school education. We do not know if this long run increase in demand for high school diplomas is derived from the demand for higher education, or from a combination of factors including the demand for higher education. We do believe, however, that the demand for high school diplomas is sufficiently complex to warrant a separate study. Here we confine ourselves to the problem of the demand for higher education among those who are in a position to make a choice for or against going to college, i.e., among those we have defined as the eligible college age population.

Although there was no trend in the ratio of undergraduate enrollment to the eligible group during the 1919-64 period, Chart 1 does reveal that the ratio fluctuated during the period. It began to fall in the late 1920's and remained depressed during the 1930's. After World War II the ratio increased and fluctuated around its level for the 1920's. While the absence of trend in the ratio suggests that there has been no basic change in the taste for higher education among those in a position to choose to go to college, the fluctuations in the ratio strongly suggest the existence of a relationship between the demand for higher education and the economic variables discussed above.

III. *Formulation of the Empirical Demand Model*

Ideally, a statistical test of the theory of enrollment demand would require data on both educational costs and finance as well as expected differential income streams for those eligible to enroll. We do not have all the necessary data, even to an approximation, in the form of continuous time series. We do have, however, some data which can be related to elements in the earlier described model.

The financial costs of investing in college enrollment can be represented by an index of tuitions, deflated by the consumer price index.⁵

⁴ B. A. Jaffe and Walter Adams [5] have found that there has been no trend for a full century in the proportion of people going on to college after high school. Their findings are based upon backward projections from 1940 and 1960 census data on educational levels achieved by various age groups, a method quite distinct from ours.

⁵ It is assumed that other financial costs will tend to vary directly with the consumer price index.

Since this is equivalent to introducing the familiar relative price variable of demand analysis, it should also help to explain the demand for enrollments as present and future consumer goods. Similarly, consideration of both the producer and consumer good aspects of an enrollment requires data on both the amounts and sources of funds used to purchase enrollments. Since we have limited information on such matters for the period of time of the study, we have chosen to use estimates of real disposable income per household. We assume that other financial resources vary with this measure.⁶

The model also calls for some measure of opportunity costs. We have not included such a variable primarily because it is difficult to find a satisfactory measure. Income from jobs available to high school graduates provides, to be sure, a measure of opportunity costs. To some extent, this measure is already included in our household income variable. Nevertheless, it is still not clear that the availability of such jobs works consistently in the direction of reducing enrollment demand. Students who work draw their incomes from jobs very similar to those available to young high school graduates who do not enroll in college. Hence, an increase in such opportunities may well work to increase, as well as to decrease demand. Because of this ambiguity, we have decided to exclude opportunity costs from the test of the model.

A formal statement of the model is given by equation (1):

$$(1) \quad N_t = f_1(Y_{Ht}, P_t, E_t),$$

where N_t is undergraduate degree enrollment in 4-year institutions in year t , Y_{Ht} is real disposable income per household in year t , P_t is average real tuition in year t , and E_t is the number of 18–24 year old eligibles in year t .

For equation (1) to provide a test of our demand model, certain conditions must be satisfied. The most important is that enrollments not be constrained by institutional restrictions—e.g., entrance requirements beyond the minimum requirement of a high school diploma. Since N_t is the aggregate of undergraduate enrollments in 4-year institutions, we believe that we can assume such constraints away. In the United States, there is probably an institution of higher education for virtually anyone who both possesses a high school diploma and has the necessary financial resources.⁷

⁶ Note, we have not adjusted our income concept for size of family or size of household. While it is true that larger families with given incomes will have more difficulty in sending their children to college, we believe that much of this difficulty is resolved by sending children to less costly schools and by throwing a larger burden of finance upon the student himself. Studies of aggregate enrollment demand are therefore not likely to pick up a large effect from variations in family size. On the dubious statistical relationship between family size and family support for students, see John Lansing, Thomas Lorimer, and Chikashi Moriguchi [6, p. 32].

⁷ See M. Blaug [3, pp. 171–72] for a similar opinion.

In addition we assume: (a) that the enrollment demand function is homogeneous, and (b) that eligibles over the period of the test have not, on the average, changed their tastes for higher education.

Since we wish to study the *ratio* of enrollments to eligibles, we can convert equation (1) into the general form

$$(2) \quad \frac{N_t}{E_t} = R_t = f_1(Y_{Ht}, P_t).$$

Note that equation (2) requires f_1 to be homogeneous of degree one in E_t , i.e., that changes in E_t do not carry with them compositional changes in the population of eligibles which might affect R_t . Among such changes, for example, would be relative increases in eligible individuals who have strong preferences for higher education. We believe that our statistical results support this assumption and turn now to the estimation of the demand function.

IV. *Estimation of the Demand Function*

Chart 2 presents on a logarithmic scale the raw materials for our test: (1) the ratio of undergraduate enrollment in 4-year institutions to eligible 18–24 year olds; (2) real disposable income per household; and (3) an index of tuition costs deflated by the consumers' price index. Note that only nine observations for the period 1927–1963 appear in the Chart. While we have more complete data on the enrollment ratio and real disposable income per household, ready estimates of tuition costs are available only for the nine years indicated.⁸

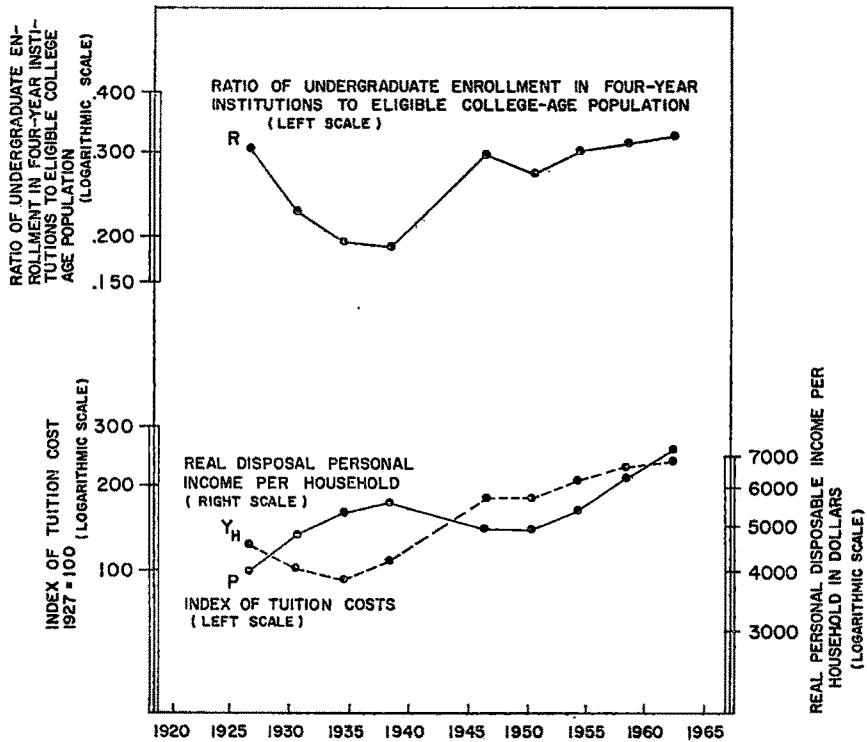
Inspection of Chart 2 appears to support our hypotheses about the behavior of the demand for education. The enrollment ratio tends to vary directly with variations in real household income and inversely with the index of relative tuition costs. In the 1950's and 1960's the enrollment ratio approximated its value in the 1920's. Without our hypothesis concerning the importance of price, this would be puzzling behavior. Real household income was much higher in the 1950's and 1960's than it was during the 1920's, and one would have expected higher income to have a positive effect on the ratio. We believe that the absence of an upward trend in the enrollment ratio during 1919–64 was due to the negative influence of price offsetting the positive influence of income on the demand for higher education.

Although only nine observations⁹ were usable for regression analysis, we feel these years to be sufficiently representative of the period as a whole to justify such an analysis. Our regression took the form:

⁸ See the appendix for the sources and procedures used in the various estimates.

⁹ The years were 1927, 1931, 1935, 1939, 1947, 1951, 1955, 1959, and 1963.

CHART 2. RATIO (R) OF UNDERGRADUATE ENROLLMENT IN FOUR-YEAR INSTITUTIONS TO ELIGIBLE COLLEGE AGE POPULATION, INDEX (P) OF TUITION COST, AND REAL PERSONAL DISPOSABLE INCOME PER HOUSEHOLD (Y_h) FOR SELECTED YEARS 1919-1964



Sources: See Appendix.

$$(3) \quad R_t = b Y_{Ht}^\alpha P_t^\beta \text{ or taking logarithms,}$$

$$(3a) \quad \log R_t = \log b + \alpha \log Y_{Ht} + \beta \log P_t$$

The regression tests our hypotheses (1) that the income effect upon R_t is positive (that $\alpha > 0$) and (2) that the price effect is negative (that $\beta < 0$). The results are consistent with these hypotheses (standard errors are in parentheses):

$$(3b) \quad \log R_t = .7425 + 1.2036 \log Y_{Ht} - .4404 \log P_t$$

(.3702)* (.1942)** (.1506)***

Coefficient of Multiple Correlation = .9316

$$F = 19.701$$

$$F_{.01} = 10.92$$

* Not significant, using $t_{.05}$ in a two-tailed test.

** Significantly greater than zero, using $t_{.005}$ in a one-tailed test.

*** Significantly less than zero, using $t_{.025}$ ($= 2.447$) in a one-tailed test.

Unfortunately, our results do not permit a verification of our belief that R_t was trendless during the 1919–64 period because of the offsetting effects of income and price. Although α and β possess the necessary signs, the rather large standard errors associated with their estimated values rules out a precise prediction of R_t . At the same time, while we have not verified our hypothesis, we certainly have not rejected it.

V. *Two-Year College Enrollments*

The estimated demand function—equation (3b)—relates the ratio of undergraduate enrollment in 4-year institutions to the eligibles in the 18–24 year old age group. The calculations exclude both graduate students and students enrolled in 2-year institutions. It is perfectly legitimate to exclude graduate students, since both the age levels and eligibility requirements for graduate work differ sharply from the age levels and eligibility requirements for undergraduate work.

The exclusion of enrollments in 2-year institutions, however, does not have equal legitimacy. To be sure, a high school diploma is not necessary for matriculation in many of these institutions and vocational training, not ordinary college work, is the objective of many students in these institutions. Nevertheless, we are uncomfortable with the exclusion of enrollments in 2-year institutions, since an enrollment in a 2-year institution can be a substitute for an enrollment in the freshman and sophomore level in a 4-year institution.

Since there is no firm basis for separating students who seek vocational training from those who use a 2-year institution as a substitute for a 4-year school,¹⁰ these students can be treated in only two ways: (a) we can exclude them from the enrollment ratio, as we have done, or (b) we can include them in the ratio and seek an interpretation of the change in the ratio.

If enrollments in 2-year institutions are included in the numerator of the enrollment ratio, a definite upward trend appears. As Table I indicates, most of the trend appears after World War II, a period during which the number of junior colleges and community colleges expanded rapidly.

If we choose to regard the bulk of enrollments in 2-year institutions as substitutes for enrollments in the first two years of 4-year institutions, the theory outlined above can easily explain the resulting upward trend in the enrollment ratio. Two-year colleges are usually low cost institutions—in terms of living costs, tuition, and fees—relative to most competing 4-year institutions. In addition, their closeness to students' homes and their flexible programs make it relatively easier for students

¹⁰ The *Digest of Educational Statistics* [8] contains a breakdown for recent years, but not for earlier ones.

TABLE 1—ENROLLMENT RATIOS FOR UNDERGRADUATES, SELECTED YEARS 1919-64

| Year | 2- and 4-Year Institutions (1) | 4-Year Institutions Only (3) | Difference Between (1) and (2) |
|------|--------------------------------------|------------------------------------|-----------------------------------|
| 1919 | .328 | .323 | .005 |
| 1927 | .320 | .305 | .015 |
| 1931 | .246 | .225 | .021 |
| 1939 | .201 | .179 | .022 |
| 1947 | .332 | .299 | .033 |
| 1951 | .309 | .276 | .033 |
| 1952 | .321 | .277 | .044 |
| 1953 | .339 | .290 | .049 |
| 1954 | .348 | .299 | .049 |
| 1955 | .356 | .305 | .051 |
| 1956 | .377 | .323 | .054 |
| 1957 | .382 | .326 | .056 |
| 1958 | .382 | .326 | .056 |
| 1959 | .371 | .316 | .055 |
| 1960 | .368 | .311 | .057 |
| 1961 | .372 | .310 | .062 |
| 1962 | .384 | .317 | .067 |
| 1963 | .393 | .326 | .067 |
| 1964 | .395 | .326 | .069 |

Sources: See Appendix.

to acquire part-time employment. Finally, the presence of many low quality students in these institutions reduces competition for able students. The combination of easier financial burdens plus less intensive competition reduces the risk of not completing schooling anticipated by many students in the first 2 years of college. Lower costs, easier work opportunities, and reduced risk all work in the direction of increasing the demand for higher education.

VI. Conclusion

Our empirical results are easily summarized in two propositions: (1) Since 1919 there has been no trend in the ratio of enrollments in 4-year institutions to the eligible population; and (2) the fluctuations in the ratio which have taken place are associated with fluctuations in disposable income per family and an index of tuition as deflated by the consumer price index. The income and price elasticities of demand (using the enrollment ratio as a measure of demand) are $+1.20$ and $-.44$. Although both of these elasticities were statistically significant when tested against the null hypothesis, we are well aware that our small sample of observations has prevented us from making reliable estimates of the elasticity coefficients. It follows that the results of the study are less useful for forecasting purposes than they might have been with a larger sample. We do believe, however, that the results substantiate, at

least in part, the model of educational demand we postulated in the paper. Unfortunately, however, the model we used was a hybrid investment-consumption model, and our results cannot be used to separate out the contribution of each of these elements to the demand for higher education. Such a separation is, in our minds, a useful area for further research.

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APPENDIX: SOURCES AND METHODS OF STATISTICAL ESTIMATES

1. *Eligible College Age Population*. With certain exclusions, this group was estimated from 7-year cumulative totals of high school graduates, beginning with the seventh year prior to the year for which the estimate was required. *Historical Statistics of the United States, Colonial Times to 1957* (1960) and the *Statistical Abstract of the United States* (both published by the U.S. Department of Commerce) provided data for most of the years. Since graduation data appeared biannually for most of these years, it was necessary to interpolate for the missing years. The elements of the cumulative totals were adjusted for death rates. Armed service high school graduates in the 18-24 age group were estimated for 1940 to 1964 by taking the ratio of the 7-year cumulative total to the total 18-24 year age group and multiplying the result by the number of 18-24 year servicemen (calculated from the *Current Population Report*, P-25, #98, U.S. Bureau of the Census). Since age breakdowns of servicemen prior to 1940 are not available, the ratio was multiplied by the total number of enlisted men for these years, on the assumption that this group comprised the bulk of servicemen in the appropriate age group. Official data on the age and educational composition of the armed forces are extremely sketchy, especially for earlier years. Nevertheless, perusal of *Selected Manpower Statistics* (Directorate for Statistical Services, Office of the Secretary of Defense, 19 February, 1965) will show that our assumptions are warranted for the years since 1948. As noted in the

text, we did not have estimates of the institutionalized population in the appropriate age group. More important, we did not exclude people in the 18–24 age group who had already received college degrees. We do not think that the year-to-year variation in the enrollment ratio will be much affected by the inclusion of this group. In any event, we had no hard data on the age distribution over time of college graduates.

2. *Enrollment Data.* *Historical Statistics* and the *Digest of Educational Statistics* [8] provided the basic sources for our estimates. It was necessary to splice together earlier and later enrollment estimates in order to put them on a comparable basis. Those presented in *Historical Statistics* for 1919–53 were based upon cumulative estimates for the academic year. Those presented in the *Digest* were for Fall enrollments, 1939, and 1946–64. The Fall enrollment figures were “blown up” by a factor relating the Fall and cumulative enrollments in four common years. In addition, in order to derive undergraduate enrollments it was necessary to estimate graduate student enrollments for a number of years.

The quality of the pre-World War II enrollment figures has long been suspect by students of higher education. Jaffe and Adams [5], for example, chose to work with backward projections of census data rather than enrollment data for this reason. We did not do so because such a method would not have yielded enough years to be useful in estimating the demand function. In any event, it is striking that our method comes to much the same conclusion about trends in the enrollment ratio that Jaffe and Adams came to. Our confidence in the usability of the official enrollment data has been increased by the Jaffe-Adams study.

3. *Tuition.* *The Fact Book on Higher Education* (Washington: American Council on Education) publishes various indexes on tuition and other student costs in looseleaf form. We constructed an index weighted by the number of students enrolled in public and private institutions and based upon the indexes of tuition and fees presented on page 263 (dated March 1964) of the *Fact Book* for 99 private and 33 public institutions. As noted in the text, only 9 years of estimates are available from this source for the period of the study.

4. *Disposable Income per Household.* Based upon data in *Historical Statistics*, *Statistical Abstract*, and the *Annual Report of the President's Council of Economic Advisers*, 1965 (Washington, 1965). We would have preferred to use estimated income per family, especially in those families where the head is 35–55 years of age and income exceeded a specified minimum. Such detailed income distribution data is not available for the pre-War period, except for 1929 and 1935–1936.

INCOME DISTRIBUTION AND PLANNING FOR PUBLIC INVESTMENT

*By A. MYRICK FREEMAN III**

The purpose of this paper is to examine the implications of introducing income redistribution as a policy goal in public investment planning and project selection. The model was initially developed to apply to water projects, but it can be generalized to apply to any federal investment where benefits and costs are tangible and beneficiaries do not bear the full costs of the project.¹

The basic criterion for evaluating public investment is the change in social welfare which results. The benefit-cost criterion which is normally used is based on the assumption that the change in national income is a good measure of the change in social welfare. When projects are designed to maximize net benefits and when all projects with positive net benefits are undertaken, the increase in national income will be at a maximum. But the benefit-cost criterion is sufficient to achieve maximum social welfare only if one of two conditions is met. Either equal increments to income must have the same welfare significance no matter to whom they accrue (the distribution of income does not matter); or there must exist a separate tax and transfer system or some other means for achieving the desired or "proper" distribution of income.

The basic assumptions of this paper are that income distribution does matter, and that there is not and will not be a completely effective tax and transfer system capable of achieving the desired distribution of income. I assume that there is a widely held value judgment which

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¹ If beneficiaries bear full cost, there is no subsidy and no redistribution problem of any consequence. The list of federal programs which do involve subsidies and redistribution effects is long. It includes urban renewal, the several agricultural income and price support systems, and the various manpower, labor retraining, and educational programs. If the kind of analysis that is outlined below is applied to these programs, it seems likely that some may be found to have "undesirable" distribution effects, while others which might not pass the benefit-cost test might be shown to have positive redistribution effects which outweigh the efficiency cost. At the very least any attempt to apply this model to one of these programs would produce information about the distribution of benefits and costs which would be very interesting in its own right. For earlier efforts to evaluate the distribution effects of public investment in water resources see [4], [2], and [3]. The latter two utilize explicit welfare criteria.

enables us to say that society prefers a more equal distribution of income, *ceteris paribus*, over a less equal distribution, at least for small changes from the present distribution. I also assume that there are barriers to the increased utilization of the tax and transfer system. It does not matter whether these barriers are taken to be increasing dead-weight or administrative costs or some form of ethical or political aversion to "confiscation" and "give-aways."

Granted these assumptions, maximizing the net benefits of federal investments, as normally defined, does not necessarily yield the maximum increment to social welfare. Of two projects with the same net benefits, the one which through its distribution of benefits and costs moves farther toward the desired distribution of income will be preferred. Not only will selection of existing alternative projects be influenced by redistribution considerations, but also the design of a project might be different if its purpose is to maximize social welfare rather than national income. In the following sections the nature of the design problem will be illustrated, a social welfare function will be assumed, and a project design and selection criterion will be derived.

I

The nature of the problems that arise when income distribution effects must be considered can best be seen by studying a model project. Assume a proposed project to construct facilities to produce and deliver an intermediate good, X . The scale of the project can be measured in units of its annual supply of X which is assumed to be a uniform stream known with certainty over the life of the project. The cost of the project is:

$$(1) \quad C = C(X).$$

The units of measurement in this and subsequent equations are annual equivalents of the actual streams of costs, etc., calculated by using the appropriate discount rate.

Let X be the only variable factor in the production of Q :

$$(2) \quad Q = Q(X, F),$$

where F is the fixed factor. If P_q is the competitive price of the final product, Q_x is the marginal physical product of X , and VMP_x is the value of marginal product of X , then:

$$(3) \quad VMP_x \equiv P_q Q_x.$$

Since there is only one variable factor of production, VMP_x defines the demand curve for X . The demand curve can also be expressed in the inverse form. If P_x is the price of X ,

$$(4) \quad P_x = P_x(X).$$

In conventional benefit-cost analysis the benefit of the project is taken to be the maximum aggregate willingness to pay for X on the part of the producers of Q . Assume that the producers are profit maximizers. The benefit is given by:²

$$(5) \quad B = \int_0^X P_x(X) dX = \int_0^X P_q Q_x dX.$$

The increment to national income, or the net benefit, is:

$$(6) \quad N = B - C.$$

This is maximized when:

$$(7) \quad P_x = VMP_x = MC_x \quad \text{and} \quad B(X) - C(X) \geq 0$$

where MC_x is the marginal cost of producing X . Strictly speaking only the equality of marginal cost and value of marginal product need hold in the first equation. This equality defines an efficiency maximizing or optimum scale of project, X^* , and provided this scale of project is chosen, the output could be supplied at any price less than VMP_x . The only problem would be one of rationing the optimum quantity among users whose demands would sum to more than X^* .³

The choice of a price for X , or more generally of the degree of repayment to be required, can be made independently of efficiency considerations. The level of repayment can be regarded as a matter of income distribution policy. If Y is aggregate income to project beneficiaries,

$$(8) \quad \Delta Y = Y_2 - Y_1 = B - R$$

where R is the repayment obligation. Let \bar{P}_x be an arbitrary price which is or might be established by the authorities, and which is not necessarily consistent with (4). Authorities can therefore express repayment as a lump sum, R , or in terms of project revenues, $\bar{P}_x X$.

The relationships among the aggregate variables can be illustrated in Figure 1. Project scale as measured by X is plotted on the horizontal axis and various quantities in dollar terms are plotted against the vertical axis. The demand curve, D_x , is also the VMP_x curve. The marginal cost curve is a long-run curve. The optimum scale, X^* , is given by the intersection of the MC_x and D_x curves. The two vertical lines at X_1 and X_2

² Assume secondary or spillover benefits are zero. If secondary benefits exist they can be incorporated here and in the later section, the only problem being measurement.

³ This is not meant to minimize the potential of price or user charges to achieve a rational allocation of resources.

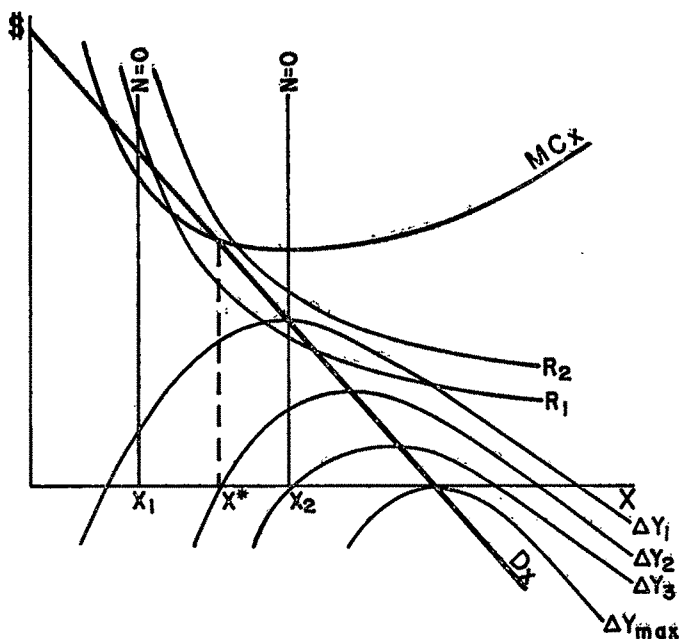


FIGURE 1. INCOME, EFFICIENCY, AND PROJECT SCALE

mark the range of possible project scales within which net benefits are positive, i.e., the benefit-cost ratio is greater than one.

The concave curves marked \bar{Y} are isoincome curves. Their shapes and properties were deduced by taking the total differential of (8) with respect to \bar{X} and \bar{P}_x and setting it equal to zero.⁴ Each curve represents those combinations of \bar{X} and \bar{P}_x which will maintain the same level of aggregate income to project beneficiaries. The maximum possible income occurs when \bar{X} is provided free and its marginal product is driven to zero, assuming, of course, the \bar{P}_x is limited to nonnegative values. Figure 1 also contains isorepayment curves which are defined by the condition that:

$$(9) \quad R \equiv \bar{P}_x \bar{X} = k.$$

⁴ Substitute $R = \bar{P}_x \bar{X}$ in (8). Then:

$$\begin{aligned} \Delta \bar{Y} &= f(\bar{X}, \bar{P}_x) \\ d\Delta \bar{Y} &= \frac{\partial \Delta \bar{Y}}{\partial \bar{X}} d\bar{X} + \frac{\partial \Delta \bar{Y}}{\partial \bar{P}_x} d\bar{P}_x = 0. \end{aligned}$$

The slope of a curve is:

$$\frac{d\bar{P}_x}{d\bar{X}} = - \frac{\partial \Delta \bar{Y} / \partial \bar{X}}{\partial \Delta \bar{Y} / \partial \bar{P}_x} = \frac{P_x(\bar{X}) - \bar{P}_x}{\bar{X}} = \frac{VMP_x - \bar{P}_x}{\bar{X}}.$$

The slope of each curve where it cuts D_x is zero.

As can be seen in Figure 1, if the most efficient project scale is chosen, incomes can be increased as repayment is lowered to zero. Beyond this incomes can be increased only by increasing project scale. At any given R , incomes increase with project scale up to a point; and if \bar{P}_x is fixed, income to the group is at a maximum if project scale is expanded to the point where \bar{P}_x is equal to the demand price $F_x(X)$ given by the inverse demand function (4). To choose from among these possibilities, the planner needs some form of ranking function.

II

Stephen Marglin has considered the problem of project design when income redistribution objectives are present [5, pp. 62-86]. He assumes that the redistribution ranking function compares projects on the basis of their contribution to the aggregate income of a group or region. The group is worthy in some sense, presumably because of low present incomes. In other words larger increments to group income are preferred. He also assumes that the price, \bar{P}_x , is established by a higher authority on the basis of prevailing attitudes toward giving away goods, subsidies, and transfer payments [5, pp. 69-70]. There are three ways in which the criterion for design can be expressed.

The first alternative is to maximize net benefits, N , subject to the constraint that the income to the region, ΔY , be equal to some target level. Alternatively, the constraint could be on N with the objective of maximizing the income to the group. In either case the constraint and the fixed value for \bar{P}_x are sufficient to determine optimum project design as can be seen in Figure 1. The third alternative is to weight efficiency benefits and project incomes, the weighting system to be chosen by the planners. The criterion becomes the weighted sum of N and ΔY . Again, given the weights and \bar{P}_x a unique solution is obtained.

But as presented by Marglin, the design problem seems artificially formulated. The redistribution constraints or weights are variables to the planner but the price is not. Furthermore, the solution to the problem is extremely sensitive to the values of the constraints (or the weights). Marglin suggests that the design procedure might be iterative. The marginal opportunity cost of satisfying the constraint (the Lagrangian term) can be determined, and if judged to be too high, the constraints can be lowered and the project redesigned [5, p. 78]. However, this process involves some rather poorly defined subjective judgments regarding weights and opportunity costs. If subjective judgments are to be made concerning the welfare values of incomes to groups and the opportunity costs of achieving constraints, these judgments should be made explicit and within the framework of a well-defined welfare function. The purpose of the next section is to propose a more general form of

design criterion which is derived from a social welfare function and which will provide guidance in the determination of repayment policy as well as project scale.

III

Planning and selecting projects on the basis of their impact on the distribution of income implies that there is a social welfare function capable of evaluating alternative distributions. Planning could be improved if the welfare function could be specified and incorporated explicitly in the project ranking function. It is assumed here that such a welfare function has been provided to planners and reflects society's assumed preference for more equality of income distribution. What would it look like? To be realistic and to reflect our apparent desire for greater, but not complete, equality of income distribution, it would have to have either an unusual form, or be accompanied by some sort of constraint which prevents the achievement of equality.

Let us assume that total social welfare or social utility is the sum of the contributions of each individual to social welfare and that each contribution to social welfare as opposed to individual's utility is a function of that individual's income alone. The social marginal welfare functions for all individuals are alike, cardinal, and downward sloping functions of individual income.⁵ In other words increments to incomes for different persons are weighted or given different values according to the initial levels of individual incomes. If W is aggregate social welfare, and Y_i is the net income of the i th individual,

$$(10) \quad W = W(Y_1, Y_2, \dots, Y_n) = \sum_{i=1}^n W(Y_i).$$

It obviously follows that social welfare is at a maximum, given the size of aggregate income, when all incomes are equal. But if there is some constraint on the use of taxes and transfers, equality will not be achieved. This constraint could be due to some ethical or moral disutility involved in the taxing process, or due to incentive costs or administrative and deadweight costs of taxation. Whatever the explanation of the constraint, it prevents achievement of income equality through taxes and transfers. In that case, the social welfare function has relevance for project design; and projects can be chosen which will add to social welfare by increasing aggregate income, by redistributing it, or by some combination of both.

The choice of the form which the social welfare function should take is a value judgment; and the economist has no special competence in

⁵ This borrows from Meade's suggestion that individuals could be assigned social welfare weights [6, Ch. 7]. The problem of social welfare functions and project planning is discussed by Eckstein [1, pp. 440-48].

making such judgments. Eckstein has suggested two possible courses of action for economists who want to get on with the important policy issues.

The economist can . . . feel free to perform experiments in policy evaluation using specific objective functions, treating the results as free of absolute normative significance. For example, he can assume a certain shape for the marginal utility of income functions. He may choose to use a form of the function that has been implicitly produced by the political process. The effective marginal rates of the personal income tax at different income levels can be interpreted as implying a marginal utility of income curve. If the government is assumed to act on the principle of equimarginal sacrifice, then marginal effective tax rates can be the basis for deriving a measure of the government's notion of marginal utilities of income [1, pp. 447-48].

The suggested approach is appealing. But the case for using the marginal effective tax rates is wrong. It is clear that the government does not really act on the principle of equimarginal sacrifice. If the government did act on this principle, all incomes above some level (assuming identical social marginal welfare schedules) would be taxed away, and no taxes would be levied on incomes below that level. More importantly there is no unique correspondence between the social welfare function and the tax schedule. Under our assumptions the tax rate structure would be determined not by the social welfare function alone but by the combined effects of it and whatever constraints there were against full use of the tax-transfer system.

Figure 2 represents a society of two individuals. The social welfare function produces a set of isosocial welfare curves, W_1 , W_2 , etc. Point B represents the initial income positions of the two individuals before redistribution. If there is no barrier to using taxes and transfers in pursuit of the social optimum, the line ABC is the "transformation curve" for income distribution, and the optimum is complete equality. The marginal tax rate on income above Y_1 is one. If redistribution is impossible for institutional reasons, the transformation curve is $E'BF'$ and the marginal tax rates are zero. If there is an increasing deadweight or other cost to using taxes and transfers, the transformation curve is EBF . Complete equality of incomes is not the optimum, and the optimum marginal tax rates depend on the marginal social cost of transferring income as well as on the welfare function.

In summary, an increasing marginal effective tax rate does imply a downward sloping social marginal utility schedule, if there is an increasing cost to using taxes for redistribution. However, the shape of the marginal tax schedule has no more claim for recognition as a social welfare function than any other assumed function. The ensuing discussion would be easier to follow if a particularly simple form for the social wel-

additional assumption that individuals have constant marginal utilities of income in order to make (5) and (8) valid measures of B and ΔY .

It is clear that this criterion is a marked improvement over those proposed by Marglin because it explicitly takes into account the original levels of income of the beneficiaries as well as the way the benefits are distributed within the group. But it also requires more information. Planners have to know the initial income distribution and how increments to income will be distributed. They also must be provided with a value for a . For convenience assume that project net incomes are distributed in proportion to the original incomes of the beneficiaries, and that each individual's share of income is known:

$$(13) \quad Y_{i1} = h_i Y_1$$

where

$$\sum_{i=1}^n h_i = 1$$

and

$$(14) \quad \Delta Y_i = h_i \Delta Y.$$

To the extent that beneficiaries do not pay the full cost of the project, there is a subsidy from another group presumably taxpayers, to the beneficiaries.⁶ The subsidy is:

$$(15) \quad S = C(X) - R.$$

And if the distribution of the subsidy cost is known, an individual's share of this cost is given by:

$$(16) \quad S_j = t_j S \quad \text{where} \quad \sum_{j=1}^m t_j = 1.$$

Equation (12) can be simplified if it can be assumed that S_j is very small relative to Y_{j1} , the initial income level of the payer. Then the social marginal welfare of income can be treated as constant over the relevant range and the welfare cost of raising a subsidy is:

$$(17) \quad \Delta W = \sum_{j=1}^m t_j S Y_{j1}^{-a} - bS \quad \text{where} \quad b = \sum_{j=1}^m t_j Y_{j1}^{-a}$$

i.e., the marginal social cost of raising the subsidy is b .

⁶ The subsidy may be paid by other groups besides taxpayers. For example, in the case of reclamation projects some of the subsidy may be paid by the consumers of electric power produced from multipurpose projects, or from other projects in the same river basin. Such subsidies arise when the prices charged for power exceed the costs and the difference is used to reduce the repayment obligation of farmers.

The design problem under these conditions is to maximize the contribution to social welfare made by the project or:⁷

$$(18) \quad \text{maximize } \Delta W = \sum_{i=1}^n \int_{Y_{i1}}^{Y_{i2}} Y_i^{-a} dY_i - bC(X) + b(R).$$

The two policy variables are X and R . The first order conditions for a maximum are:

$$(19) \quad VMP_x \cdot Y_2^{-a} \sum_{i=1}^n h_i^{1-a} - bMC_x = 0$$

and

$$(20) \quad Y_2^{-a} \sum_{i=1}^n h_i^{1-a} - b = 0.$$

The interpretation of (20) is straightforward. The marginal social cost of the subsidy must equal the marginal social value of an extra dollar of income divided among project beneficiaries in the way given by (14). When this condition is substituted in (19), the optimum scale of project for redistribution purposes is shown to be the efficient scale of project as well.

$$(21) \quad VMP_x - MC_x = 0$$

But this happy result stems from the lack of a constraint on R . R must be able to take on negative values, if need be to assure that incomes are raised or lowered to the optimum level.⁸ Note also that the efficient scale of project could have negative net benefits. Thus if

$$(6) \quad N = B - C$$

were the criterion, it would not be built. But such an apparently undesirable project might nevertheless make a positive contribution to social welfare as measured by (18).

If partial repayment is required because of institutional or legal factors, and R is an increasing function of X , there is only one policy variable. The condition for a maximum of (18) is:

$$(22) \quad Y_2^{-a} \sum_{i=1}^n h_i^{1-a} \frac{1}{b} = \frac{MC_x - R_x}{VMP_x - R_x}$$

⁷ This assumes that there are no secondary regional effects either in the region of the project or elsewhere; or that if there are such effects they are distributionally neutral. Otherwise additional terms for secondary gainers and losers would have to be added.

⁸ Equation (20) can also be interpreted as determining the optimum repayment policy for projects.

where R_x is the first derivative of required repayment with respect to project scale.⁹ When the R associated with the efficient project scale exceeds the optimum R , i.e., when the marginal social value of income to beneficiaries is greater than the marginal social cost of the subsidy, further redistribution in favor of beneficiaries is desirable. If R cannot be lowered, project scale must be altered to satisfy (22). MC_x must exceed VMP_x . Planners must build projects that are oversized compared with the results of maximizing (6) or (18). And the social marginal welfares cannot be made equal. In other words, if simple dollar-for-dollar transfers from taxpayers as a group to beneficiaries as a group cannot be used to drive the social marginal welfares to equality, project scale becomes the operating variable.¹⁰ At the efficiency optimum the marginal dollar in costs produces one dollar in benefits, and at a given R , one dollar of income. At a larger scale of output, the marginal dollar of taxes given a social value, for example, of one dollar produces less than one dollar of income, say \$.90. If the marginal social value of an extra dollar to beneficiaries at this point is \$1.11 (because they, roughly speaking, are a lower income group), the social value of the extra \$.90 is just \$1.00. The marginal social cost of a dollar of taxes is equated to the marginal social value of the less than a dollar of income produced at a larger scale of output. This conclusion that oversize projects should be built is independent of the form and parameters of the marginal social welfare schedule. It follows from beneficiaries having lower incomes and there being a binding constraint on R .¹¹

There are some reservations that must be entered concerning the optima that result from the solutions to the model. First, the marginal conditions of (19) and (20) and (22) apply to group "weighted" averages rather than to social marginal welfare changes for all individuals. A low mean income for a group could mask substantial intragroup inequality. It has been assumed that the within-group distribution of benefits cannot be affected by planners. This is consistent with the general assumption barring increased use of taxes and transfers. It accurately describes the situation for many types of projects which are likely to involve distribution considerations, for example, reclamation and flood control.¹²

⁹ If R is fixed as a constraint, independently of project scale, the R_x terms drop out. The conclusion is unchanged.

¹⁰ Project design, as opposed to financial arrangement, is affected by the introduction of redistribution criteria only when \bar{P}_x (or R) is constrained above the value given by (19) and (20). Marglin's assumption that price was given is consistent with our results [5, pp. 69-70].

¹¹ There is another way to explain this oversize. Resources are, in effect, valued differently depending on who owns them. The welfare weighting system as applied to project design is analogous to using two price sets. The marginal cost curve drawn in Figure 1 reflects the market prices for resources used in construction. The resources raised by taxation have a lower social opportunity cost. This lowers the "social" marginal cost curve.

¹² The provision in Reclamation Law limiting the size of farms on federal projects to 160

The second reservation concerns the requirement to build oversized projects. Building oversized projects involves an unmet opportunity cost or cost of insisting on repayment above that defined by (20). This cost is in the form of net benefits foregone. For example, assume $R=0$, and let ΔY_2 be the income of beneficiaries at the optimum scale, and ΔY_3 be the income from an oversize project with output X_2 . Figure 1 shows the essentials. If the efficiency maximizing scale of project were built, the beneficiaries could be made as well off as under the oversize project by a lump sum transfer equal to ΔY_3 minus ΔY_2 . This payment would be raised from the taxpayers and would be equal to the area under the demand curve between X^* and X_2 . This payment would be smaller than the alternative burden on taxpayers which would result from building the oversize project. This latter burden is the area under the MC_x curve between the two alternative scales of the project. The unweighted opportunity cost to society of building at X_2 is the reduction in net benefits from the maximum obtainable, or the area bounded by MC_x , D_x , and the vertical line through X_2 .¹³

When the constraint is binding and oversized projects are built, the result is really a second-best solution in the sense that if one set of marginal conditions (in this case on social welfare contributions) cannot be met, the optimum solution requires divergence from another set of marginal conditions as well. The situation resulting from maximizing (18) is not the most desirable outcome since by reducing project scale and using transfer payments the income of one group could be increased without reducing that of the other group. The second-best solution resulting from this model can only be justified if a third element of social welfare is recognized. If outright grants and transfer payments are deemed undesirable because they are "destructive of moral fiber" or whatever, society must turn to more subtle ways of altering the existing distribution. There is ample evidence that we, as a nation, prefer to hide our subsidies and grants behind a facade of respectability. The requirement of repayment for irrigation projects as currently administered could be interpreted as such a facade for hidden redistributive devices. This preference for covert rather than overt redistribution is an economic good in that it cannot be satisfied without cost.

Although the social welfare function was here assumed to have a particular form for purposes of exposition, the results hold for any welfare

irrigable acres could be viewed as an attempt by policy makers to affect the within-group distribution of benefits. Aside from the opportunities for evasion, the acreage limit does not achieve this purpose. If a farmer is required to sell excess land holdings he will still capture the benefits since they will be capitalized in the price of the land.

¹³ Counting the opportunity cost in the design criterion (18) would result in a smaller, but still oversized, project. The degree of oversize would depend on which group, taxpayers or beneficiaries, was assumed to bear this cost, and the social marginal utility of income to that group.

function with diminishing social marginal welfare or utility of income to individuals. Maximizing such a social welfare function subject to the constraint that incomes cannot be equalized through lump sum taxes and payments requires that projects and programs be expanded beyond the point where marginal benefits equal marginal costs.¹⁴ The degree of oversize which is required for the social optimum depends on the relative income positions of the subsidy payers and the beneficiaries. If the tax used to finance the subsidy falls primarily on high income individuals, the social cost of money raised by taxes will be low and a greater degree of oversize will be optimum.

The degree of oversize will also depend on the form and slope of the social marginal utility schedule. Of course, the choice of welfare function is a value judgment; but it is one which must be made before this criterion can be applied. A value judgment implying a steep social marginal utility schedule means that redistribution benefits are given relatively greater weight in the criterion, and that the degree of oversize will be greater.

IV

The model presented here represents an attempt to follow through on a recent suggestion by Eckstein, namely to "... interpret the desires of the policy people ... and express them in an analytical form as an objective function," and to get on with "... the establishment of decision-models which will reveal explicitly what actions will maximize the achievement of specified objectives" [1, p. 445]. The federal government is moving rapidly ahead with a wide range of programs which combine investment in human and natural resources with subsidies to beneficiaries. Evaluation of these programs with conventional benefit-cost techniques seems to me to miss the point, since there is implicit in the public discussion of these programs the notion of a social welfare function embodying income distribution in some way. The social welfare function used here and the design criteria based on it are simply meant to formalize these apparently widely held ideas, and to examine their implications for project planning.

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¹⁴ If beneficiaries have lower incomes, i.e., have higher social marginal utility of incomes.

MARXISM-HORVATISM: A YUGOSLAV THEORY OF SOCIALISM¹

A Review Article

By BENJAMIN WARD*

A certain complacency tends to creep into Western economists' evaluations of avowedly Marxist works. After all, we are in possession, at least relatively, of the truth, in the form of a correct analysis of the price system, a variety of freely competing theories of growth and an open-minded willingness to adapt our models to the burgeoning flow of empirical results (which are mostly produced under the auspices of the Western analytic framework). From this vantage point it is convenient to treat our Eastern colleagues much as we do our students: those who show a willingness to learn are to be encouraged; the others should be urged to change their majors.

No one, I think, who reads Branko Horvat's book with any care will find it possible to maintain this paternalistic attitude. For this is a serious and original, even though avowedly Marxist, rethinking of some major economic problems of contemporary society. The arguments are cast in a language that is almost wholly intelligible to a "purely" Western economist, and the results present an integrated picture of the economic decision process in an optimal social regime. Naturally enough this regime is essentially socialist; not surprisingly, it bears a more than casual resemblance to Yugoslavia. What is surprising is that it carries a more than expected measure of plausibility—at least to this Western reader.

The next section provides an abstract of Horvat's argument. The language is mine, and a good deal is omitted so as to concentrate attention on the main features of the optimal regime. The sections after that contain commentary on various parts of Horvat's theses. The concluding section includes a description of some Yugoslav reactions.

I. Horvatism

Despite Marx's tremendous intellectual accomplishment, including unusual prescience as to future trends in the development of capitalism, there are two major trends in the twentieth century which were incorrectly or incompletely foreseen by him. The first of these is State capitalism, which may be taken ei-

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¹Branko Horvat, *Towards a Theory of Planned Economy*. Belgrad: Yugoslav Institute of Economic Research, 1964. Pp. viii, 244. 87.5 new dinars. A slightly modified English translation of *Ekonomika teorija planske privrede*, Belgrad, Kultura, 1961, pp. x, 315, 1500 old dinars.

ther as the highest stage of capitalism or the lowest stage of socialism. Marx correctly foresaw the rise of large-scale organizations. Indeed this rise is inevitable, meaning by inevitability that large-scale organizations are both feasible forms of organization and of superior efficiency to the petty capitalist organizations of a century ago. What Marx failed to see was the class conflict that is inherent in bureaucratic systems of control. Once the bureaucracy is established as a dominant system of economic control, a class is created with an interest in preserving its power and relative affluence against outsiders. As noted above it makes little difference whether such a society calls itself socialist or capitalist. It does, however, make some difference whether it is essentially one big bureaucracy or not, and whether it developed out of capitalist preconditions. Weber pointed correctly both to the efficiency properties of rational bureaucracy and to the importance of rational capitalist calculation as an element in promoting that efficiency. As a consequence of the absence of a developed capitalist rationalism, a bureaucracy imposed on an underdeveloped country will be especially inefficient. However Ludwig von Mises pointed to a vital aspect of bureaucracy which Weber ignored, namely the absence of a basis for rational calculation within a large-scale organization that does not contain internal markets. In addition there are the usual problems of bureaucracy: information absorption and distortion within the hierarchy, the aggregative and therefore rough-and-ready nature of the key highly centralized decisions of the top leadership, the tendency to "pass responsibility up and work down," etc.; all of which make very large bureaucracies very inefficient. The tendency toward concentration thus produces at first a higher and more efficient stage of society, but also contains the seeds of its own destruction as still further growth tends to produce unwieldiness rather than rationality.

The second modification of the classical Marxist picture of social development leads to the assertion of the inevitability of worker control, inevitable again in the sense that it represents a feasible social form whose efficiency is superior to that of the historically preceding form. As to the first aspect of inevitability there is a clear trend toward increasing worker participation in management. Partly as a consequence of the workers' natural inclinations there has been some attempt to establish factory committees in every major revolution from 1848 through the Russian and other revolutions around World War I to those of Poland and Hungary of 1956. Partly as a consequence of the needs of state capitalist government some measure of worker control has been introduced widely in nationalized industries and in Great Britain, Germany, and the United States, among others, during wartime. Partly as a consequence of the initiative of enlightened capitalists, from profit sharing to production conferences it has served to increase efficiency and therefore profits. This broad trend strongly suggests both feasibility and superior efficiency. However it was not likely that full-scale worker control of industry would come first to a developed state capitalist society. What was needed was, first, a revolution to sweep away traditional attitudes and authority in a less developed country, and then a developing trend toward workers' management which preceded the entrenchment of the new bureaucracy. Hence

Yugoslavia provided the first instance of this higher form of society, which realizes for the first time the ultimate Marxian aim of free associations of workers.

So much for background. The next step is to survey the problems of planning economic development in the contemporary context.

To maximize welfare one should maximize the total volume of consumption of any generation, subject to a similar maximization with respect to any other generation. But over a generation (say 30 years) consumption maximization is equivalent to production maximization. This is because the limited absorptive capacity of all economies sets short-run limits to the productivity of investment. Most economies can bring the marginal productivity of investment to zero within half a generation, in the sense that no discarded project is capable of producing a net addition to social income over this period. For nearly all economies this implies a steady increase in the savings ratio over at least the first half-generation. However, the future consumption benefits within the generation are so much greater than the early foregone consumption that rational, goods-oriented consumers will almost certainly choose to increase savings up to absorptive capacity. Thus optimal savings and optimal investment will coincide under this policy.²

If a society had already chosen, in some sense, to get on the maximum growth path this would imply that the marginal disutility of saving is zero. Hence it would decide to stay permanently on this path, since with consumption increasing it is highly unlikely that the marginal disutility of saving would increase. The saving decision must be a collective decision because, among other reasons, a consumer faced with his own present-future tastes and the interest rate does not have enough information to make his decision, because his future real consumption depends on the behavior of other consumers. Consumers in an economy that is off the maximum growth path would surely, if faced with a referendum representing the present-future choices outlined above, choose to get on the maximum growth path.

In an optimal regime, getting on the maximum growth path is only part of the battle. One must also make allocative decisions relating to alternative uses of resources. Two interesting aspects of this class of decisions relate to investment choice. The first is that individual decisions, such as might be made by an enterprise or by a planning board with respect to resources available this year, will be made in the face of a positive interest rate. This is true even though the overall marginal productivity of investment is zero because of the partial, short-run context of these decisions. For example, output this year could be increased by putting engineering students to work in factories. The other point is that once society has chosen the maximum growth path the supply of capital over that generation is fixed by that decision. This implies that interest, which may be nonzero for decentralized decision making, is a rent.

The most useful way to treat factors of production when dealing with the problem of designing an efficient economic system is to think of them as the collection of influences on the productivity of labor. It is also useful to distinguish the four traditional factors—monopoly, labor, capital, and entrepreneur-

² For Horvat's illustrative calculations see table in fn. 4.

ship—because of differences in the nature of their impact on productivity. The term “monopoly” is substituted for the traditional “land” to reflect its modern generalization to the surplus over the minimum supply price of a factor. If land is socially owned its minimum supply price is zero, suggesting the Single-Tax (but not alone) as a useful means of controlling income distribution without distorting efficient outcomes.

The search for a welfare economics based on the notion of Pareto optimality has proved a failure. It becomes a defense of the status quo on efficiency grounds, because of the income redistribution that occurs as an inevitable consequence of any readjustment. Hence the controversy and equivocation over the case of increasing returns. Though the broad lines of marginalist evaluation of alternatives remain as useful guides, the full-cost principle is the most satisfactory practical rule of thumb for decentralized decision making by enterprises.

When the above is combined with knowledge of the dangers and inefficiencies of bureaucracy it seems clear that markets are the best “planning authority” for the overwhelming majority of economic decisions. Enterprises striving to maximize profits are the most useful means for organizing the allocative decisions that are not essentially collective in nature.

In an optimal regime enterprises will be controlled by the workers, who will follow the above rule and receive wages as the usual reward for their contribution to productivity. The fourth factor, entrepreneurship, consists in the supply of supervision, coordination, and decision making under uncertainty, combined with Schumpeterean innovative activity. Profit is the supply price of this activity, which in the optimal regime is carried out by the workers’ collective. Since entrepreneurship is diffused throughout the society any particular collective is not entitled to the full profits generated by the market operations of the enterprise.

The optimum distribution of income would be egalitarian, because of the declining marginal utility of income, if income distribution were independent of output. But because of this dependence the rule must be: the most egalitarian distribution of income consistent with maximum production. As societies develop, skewness of the distribution tends to decrease and uneconomic motives to substitute for the individualistic, materialistic incentives of capitalist and underdeveloped societies. It seems likely that before an economy has been on the maximum growth path for many decades the condition attached to the distribution rule will become rather insignificant.

The main features of organization in the optimal regime have already been mentioned. A market economy with socialized means of production is operated by profit-maximizing workers who pay interest-rent, and some portion of profits, to the state for the privilege of operating social capital. They may invest too, but will pay an interest charge on new as well as old capital. They are also charged depreciation in proportion to the amount of capital they operate, and may sell part of their capital to other enterprises, provided they pay back all remaining depreciation charges. A planning authority corrects the market mechanism where necessary to increase economic welfare, by dealing with such problems as externalities, market adjustment failures and income

distribution. These basic organizations will be supplemented by a number of others, such as banks, communes (territorial governments), and industrial associations.

Yugoslavia comes closer than any other existing country to satisfying the conditions for an optimal regime.

II. *Investment Productivity*

The heart of Horvat's policy argument, on which a good deal of the rest depends, is the assertion of the feasibility and desirability of maximizing production over the lifetime of a generation. However a serious problem arises right at the start in giving meaning to the criterion he proposes. Perhaps his clearest statement of the criterion is: "Maximization of welfare of *every* generation means maximization of the total volume of consumption in the lifetime of *any* generation consistent with the similar maximization of *any other* generation" (p. 199; italics his). Nowhere are this criterion or its implications spelled out formally. It appears not to give a decision with respect to investment proposals which increase the consumption of one generation and reduce that of some other generation. However for Horvat's purposes this is not a fundamental weakness. Here as elsewhere in the book Horvat is interested in developing a practical rule for planners. His views as to the actual structure of development alternatives makes the range of indecisiveness of the criterion much less than it appears *in abstracto*. A practical rule can probably be constructed which will satisfy the requirements of his theory and which is not *prima facie* unacceptable. For example consider the following rule: (1) Ignore all investment projects to be implemented in the next or later generations; (2) adopt all investment projects which increase net consumption over this generation and the next. This is obviously not an optimal rule, but two things can be said for it: (1) Its allocative failures are likely to be concentrated in the distant future so that present uncertainty offers little opportunity to improve the decisions before the next plan (a point emphasized by Horvat), and (2) the marginal consumption-productivity (over two generations) of investment is zero for projects implemented in this generation, a key feature of Horvat's approach.³

A much more interesting part of Horvat's theory is the assertion that in effect the opportunity cost (in terms of consumption) of increasing the ratio of investment to output is near zero up to a very high ratio. He argues that a good estimate of the typical marginal capital output ratio is about three, that that ratio tends to be fairly constant with increasing investment ratios up to some ratio near the capital saturation point, provided the investment ratio is not increased by more than one or two percentage points per year, and that capital saturation (absorptive capacity) tends to occur at an investment ratio of around 35 per cent. Consequently a country with a current investment ratio of 15 per cent might rise to the long-run-saturation investment-ratio-point in

³ Unless the actual structure of development alternatives is that hypothesized by Horvat, a rule like this could produce results that were strongly biased against the consumption of some (probably the earlier) generations. In this sense too it is a "practical" and not a "theoretical" rule.

ten years or so, after which output will grow steadily at about 10 per cent per year. If this is true, the investment ratio may be increased without ever producing fewer consumer goods than in the base year and with a growth rate of consumption such that in no year is the consumption level more than a year or two behind the level that would have been achieved had there been no increase in the investment ratio. And long before the initial generation "disappears," say 25 years later, society is consuming far more than it would have under the 15 per cent ratio, as well as growing much more rapidly, and providing a far larger capital base for the next generation.

If the assumptions are true the conclusions do follow.⁴ As with other parts of his analysis Horvat is leaning rather heavily on recent Yugoslav experience, where investment ratios of around 30 per cent have been producing a growth of output of almost 9 per cent for nearly fifteen years. Furthermore, one can find support for at least part of the argument in a recent analysis of prospects during the seven years 1963-70 for some fifty developing countries [6, Annex A] [7, p. 718]. Upper-limit growth possibilities on the average for these countries are estimated at about 6 per cent. Marginal capital output ratios are estimated at 3.2 and the marginal savings ratio at potentially 24 per cent, with investment growing potentially at 10 per cent. Under favorable circumstances and with strongly growth-oriented policies the countries of the underdeveloped world on the whole are apparently, according to an independent estimate, capable of carrying out the early portion of Horvat's program.

It is difficult to get a clear empirical insight into the relation between the size of the investment ratio and the size of the marginal capital output ratio. There is some weak tendency in the data on which the above study was based, which mostly covers about five years during the late fifties, for higher values of the former and the latter to be associated, but there are also a number of exceptions. Data for NATO countries over the years 1950-63 show some tendency for rising investment ratios (and especially for those which are above 20 per cent and rising) to be accompanied by higher marginal capital output ratios, several of them being well above 4.0 [19, Table VI-6]. The Eastbloc countries (excluding Rumania and Albania) had marginal gross capital output ratios of 5.6 (unweighted average) over the period 1951-64, with high in-

⁴The following table (Horvat, p. 190) shows how this works. Pattern I shows consumption and output when the investment ratio is maintained at 15 per cent, Pattern II when the investment ratio increases by one percentage point per year and Pattern III when it increases by two percentage points per year. The underlying assumptions are as stated in the text.

| Year | Pattern I | | | Pattern II | | | Pattern III | | |
|------|-----------|----------|-------------|------------|----------|-------------|-------------|----------|-------------|
| | Output | \$ in | Consumption | Output | \$ in | Consumption | Output | \$ in | Consumption |
| | | per cent | | | per cent | | | per cent | |
| 0 | 100 | 15 | 85 | 100 | 15 | 85 | 100 | 15 | 85 |
| 1 | 105 | 15 | 89 | 105 | 16 | 88 | 105 | 17 | 87 |
| 2 | 110 | 15 | 94 | 111 | 17 | 92 | 111 | 19 | 90 |
| 3 | 116 | 15 | 98 | 117 | 18 | 96 | 118 | 21 | 93 |
| 4 | 122 | 15 | 103 | 124 | 19 | 100 | 126 | 23 | 97 |
| 5 | 128 | 15 | 108 | 132 | 20 | 105 | 136 | 25 | 102 |
| 6 | 134 | 15 | 114 | 140 | 21 | 111 | 147 | 27 | 108 |
| 7 | 141 | 15 | 120 | 150 | 22 | 117 | 161 | 29 | 114 |
| 8 | 148 | 15 | 126 | 161 | 23 | 124 | 176 | 31 | 121 |
| 9 | 155 | 15 | 132 | 173 | 24 | 132 | 194 | 33 | 130 |
| 10 | 162 | 15 | 138 | 187 | 25 | 140 | 216 | 35 | 140 |

vestment ratios. Furthermore, the capital output ratios tended to rise with the investment ratios within this period [10, pp. 890-92].⁵ However, the Soviet Union has apparently had an unusually low incremental capital output ratio despite its relatively high investment ratio [5, p. 357]. Finally, Horvat notes the very large improvement in the postwar Yugoslav ratio and asserts that this is attributable in large measure to improved ability to implement the rapid growth policy. Unfortunately, the change is too easily associated with the stagnation of industrial growth during the period of adjustment to the Cominform blockade to be generalizable. These data *prima facie* call for a good measure of skepticism about capital coefficient constancy with increasing investment ratios, but by themselves are probably insufficient to reject the thesis. There are examples, such as Yugoslavia itself, where high investment productivity and high investment ratio appear to go together, and even where the former is low it may be argued that institutions for economic decision making were inefficient, representing a lower stage of social development.⁶

III. *Macroeconomic Control*

Suppose for the moment that Horvat is right and declining investment productivity as a result of decreasingly attractive alternative projects is not a hindrance to his policy. One question that still must be answered is whether the economy can be moved to and kept at the zero-productivity point.

First the point must be identified. The argument that the point is located at an investment ratio of about 30 to 35 per cent is based on the fact that Soviet-type economies and Yugoslavia have tended to operate near this level, and the assertion that though their investment productivity has not suffered dramatically as a consequence, they have occasionally pushed their investment activity beyond the point of positive return. That is, no empirical support is offered for this part of the thesis. Probably at the margin all the Eastbloc countries, including the Soviet Union, have often been well beyond the zero-investment-productivity point, but they have also probably been missing some good bets for productive investment.

Identification of the point is made more difficult because of the uncertainty surrounding investment alternatives under conditions of very rapid growth. At an 8 per cent growth rate, output doubles every 9 years, so that well within, say, a half-generational planning timespan new projects will have to be integrated with a capital stock over half of which is as yet nonexistent. This suggests that a large margin of error will be attached to investment plans and may lower investment productivity as well as make the sought-for state hard to identify.

A notable if not unexpected feature of the Chenery-Strout study [6] [7]

⁵ These data were recalculated from official statistics by Thad Alton and Associates.

⁶ Together with Horvat we are assuming that these aggregates provide a meaningful basis for policy discussion. For example, because of the variability of the capital output ratios across sectors, Horvat's thesis may hold for a country only if it is willing to accept a structure of investment that may be undesirable on other grounds. Nevertheless for the sort of global orientation toward development policy that Horvat has in mind this level of aggregation appears to be rather useful.

is its finding that the effective constraint to further growth in a large number of underdeveloped countries is balance of payments. Horvat ignores foreign trade, a serious failing from the point of view of the plausibility of his arguments. Its effect in the present context is implicitly to assume that every country will be able to obtain aid of a sufficient magnitude to move the economy out to the absorptive-capacity constraint. For a Marxist, especially, that is a rather naive assumption.⁷

A final question has to do with the instruments for achieving Horvat's goal. In his optimal regime, state policy will assure a high level of aggregate demand. The problem is that we are still in an early stage of our understanding of short-run macro-economic control techniques, and less developed countries with their greater structural rigidities are probably much harder to control than, say, the United States. At high growth rates control problems are more serious, the risks of overheating are large, and the cost in output foregone during adjustment can be fairly high. This question is essentially dismissed by Horvat who seems to be at least as good a Keynesian as he is a Marxist.

IV. *The Optimum Rate of Savings*⁸

The really striking thing about Horvat's calculations (see the table in footnote 4 above) is their implication of a near zero opportunity cost, in terms of aggregate consumption, for higher growth up to a very high rate. In Patterns II and III, after a decade one gets more consumption forever than in Pattern I, so there is no long-term cost to choosing maximum growth. In the short run there are a few years in which consumption increases lag behind in Patterns II and III, but the lag never exceeds a year or so, is usually less, and absolute consumption never falls.

This peculiar consequence of the compound interest formula has been known for some time. A variant of the thesis was, for example, offered by Peter Wiles some years ago [24].⁹ But so far as I know Horvat is the first writer who both uses this consequence to defend maximum growth of output and tries to offer an integrated theory to support the realization of the goal. The undoubted power of compounding to produce amazing increases in output over a generation or two lends added interest to the question: Given that Horvat has the facts right, is his policy proposal desirable?

To part of the question the answer in terms of the usual assumptions is a simple "yes." Suppose (1) that a society's decisions are governed by the morally appropriate social welfare function, (2) society is on the maximum growth path, and (3) is in an equilibrium state. Then clearly it should stay on that path. For in equilibrium the marginal disutility of saving is equal to the mar-

⁷ Horvat also ignores population growth. However when economies start growing steadily at ten per cent, a population growth rate of three per cent or so will not cause great alarm, though problems of adaptation to the changing factor mix may arise.

⁸ Issues surrounding this question are most lucidly analyzed by Sen [22]. Sen deals with Horvat's maximum-output proposal explicitly, as the proposal was initially presented in 1958 [14]. Horvat has recently returned to its defence [16].

⁹ A very early absorptive-capacity growth model was developed by the Soviet economist, G. Fel'dman [11]. A translation of his paper is available [23] and Domar has analyzed it [8].

ginal utility of investment (in terms of additional consumption produced by the investment). The marginal utility of investment is zero on the path and with consumption rising the disutility of saving will not rise. Hence society should stay on the path.

Objections may be raised against this position on three grounds. First there are some who argue that over certain ranges the marginal utility of income rises, in which case the marginal disutility of saving may in principle rise with income. Not an important objection, I think, especially since this phenomenon cannot dominate a broad range of incomes. Secondly, and more importantly, if one believes that Pareto optimal savings can be generated by independent market decisions of households, it seems clear that the argument breaks down, because the second condition above seems in fact never to occur in societies where this form of savings mobilization dominates. Thirdly, and still more importantly, it is not clear, except in the most simplistic cases, that conditions (1) and (3) can be given an intelligible meaning when one does not believe that independent decisions are consistent with a Pareto optimal savings rate.

The argument against independent-decision optimality is strong and is based essentially on the individual's inability to determine the future availability of goods to himself without knowing the intentions of others, and on his interest in a future that extends beyond his own lifetime. Future prices depend not only on the individual's saving decision, but on the current intentions of all other potential savers. The market gives highly incomplete information on the productivity of current saving and on expected future prices. As to the second point, an individual may not be willing to save a dollar now to produce three dollars after he is dead, but may be willing to do so if someone else agrees to do the same. Then, in effect, he has produced six dollars of income for future generations at a cost of a dollar of saving to himself and a dollar of current loss to "his" generation.¹⁰

Thus a collective decision seems necessary to achieve Pareto optimality. The trouble is we have no joint decision processes which produce Pareto optimality in the absence of consensus. The referendum that Horvat proposes can in principle produce a result which makes a large minority extremely unhappy. A political equilibrium does not in general have economic optimality properties.

The remaining part of the question is in a way easier to deal with. If the cost of moving to the maximum growth path is very small and the gain is very great, then presumably most everyone will want to do it. That is, Horvat might argue that up to now people have not been sufficiently informed as to the costs and benefits of this choice, so past empirical data are irrelevant, and the benefits from choosing maximum growth are so great that objections of the kind raised above are swamped.

Where does all this leave us? It leaves me, at least, in the following position: (1) The attractiveness of compounding is very great and creates a predisposition toward higher growth; (2) available data indicate very crudely that

¹⁰ These are Sen's arguments, *q.v.* Reference might also be made to James Buchanan's argument that condition (1) may be unintelligible even (rather, especially) with independent-decision optimality.

underdeveloped countries typically can raise their gross investment ratios substantially, for example to 20 per cent or more, with little cost in terms of gross investment productivity, provided an adequate (which usually means substantial) availability of foreign aid is assured. (3) This could get growth rates of output to above five per cent but in most cases (on the average over a long pull) probably not too much above that rate. (4) In terms of economic control the above will not be easy to achieve and for higher rates the costs, including the risks of dramatic readjustments, will be excessively high in most cases. (5) Socialist experience, including the Yugoslav, suggests that these risks tend to be shifted by the planners and their bosses to the consumption sector, and that this produces a very different consumption timepath than the neat monotonic patterns in Horvat's table.

V. Neoclassical Planning Theory

A problem of interpretation makes the evaluation of Horvat's decision calculus a needlessly difficult job. He is very critical of the received western doctrine in the area of resource allocation, generally ending up by dismissing the theories as valueless toying with trivial cases. However, this is frequently followed by the proposal to use a much cruder version of the same theory, whose validity perforce is proved only for even narrower and less realistic situations. A striking instance is his detailed criticism of marginal cost pricing, followed by the claim that profit maximizing is the best way to deal with enterprise allocation decisions.

There is, I assume, nothing inconsistent in this. Rather, Horvat is simply saying that in practice the fancier aspects of economic theory can be ignored because data and administrative problems do not allow control or even observation of such fine distinctions. However, given that this is his purpose, the attacks on economic theory are needlessly polemical.

Essentially Horvat accepts the current western doctrine that the market system works effectively and should be left to do the job except where paternalism, monopoly, and neighborhood effects occur, or a bad distribution of income results. In these cases Horvat and Milton Friedman recognize in principle the legitimacy of state intervention. Nevertheless there is a real difficulty with much of Horvat's argument which is probably a result of the form of presentation. He is extremely casual both in his factual assertions and in his discussion of proposed remedies for the failures of the market. Probably most economists would agree with his preference for the real market over administrative simulation of markets. But no one will be convinced by the brief comment he devotes to it after a harangue on the inadequacies of marginal cost pricing. His position in a nutshell seems to be: The market knows best, except in cases where society knows best. However, he devotes little attention to problems of decision making in cases where the market fails. He seems to be saying that where the market fails the economist does too. "The examination of how social valuations are arrived at falls outside the field of economics" (p. 32). This important class of decisions is turned over to the planners, along with a general social criterion, but no guidance is offered as to what to do with them.

In discussing factors of production, Horvat fails to mention one of the most useful attributes of market allocation: the assignment of distinct functional returns to the various factors. Perhaps there is good reason for this omission, since under worker management the separation of profits and wages is no longer performed by the market. Surely this can cause serious administrative difficulties, but they go unmentioned, along with those that would be associated with his proposed tax on nonwage quasirents. His discussion of entrepreneurship as a factor seems to be more a doctrinal than an economic theory, and seems to be aimed mainly at proving that society, being in part the entrepreneur, has a right to a share of enterprise profits.

Within their generational time horizon planners must choose which specific projects to approve and when to schedule their implementation. This is a most difficult and fundamental type of decision but Horvat's highly aggregative growth theory, his incompletely specified maximization problem (see p. 513 above), and his essentially polemical discussion of the interest rate offer little assistance in solving it. The informational activity that will be needed to produce the optimal ten or fifteen year bill of investments is surely beyond the capacity of any existing agency. Or will the market determine most of this? Presumably in most underdeveloped countries the answer to the latter question is: "no," which means placing the economy's future in the hands of an assertedly extremely inefficient bureaucracy. But even where it is "yes," what assurance is there that worker managers will behave in the appropriate ways? It is not even clear that they will be profit maximizers. The first sentence of the English foreword begins: "This is a book of a planner for . . . planners. . . ." Surely a theoretical framework to deal with these problems is called for, and would be more valuable even to a theoretical planner than attempting to shoot down once again Frank Knight's views on the interest rate.

VI. *Horvat as a Marxist*

In his value theory Horvat manages to produce more or less Marxian results from more or less neoclassical assumptions. He defines the marginalist calculus as a tautological system which is incapable of being falsified, so presumably he accepts it as a logically consistent, if in principle uninterpreted, theory. The labor theory of value, as a specification or special case of the marginal calculus, is capable of being falsified. This is a tenable position, sanctioned in fact by Schumpeter, though it is not the only tenable position. Surely there are a variety of ways in which marginalism can be given a real world interpretation, in the sense that some of its propositions can be so interpreted as to be conceivably falsifiable as a result of empirical test. However, there seems to be no logical or methodological reason why Horvat's approach need be rejected.

An advantage of his approach is that the long, bitter, and largely empty discussion of the applicability of Marxian price and value schemes to socialism is bypassed. Taking Marx's *en passant* ". . . I have never built a 'socialist system' . . ." [18, p. 357] as text, Horvat can point to the usefulness of the labor theory in some contexts and go on to the tautological and hence ideologically neutral marginalist calculus as his starting point. The next step is to

provide a new specification of that calculus to apply to the planned economy.

The maximum growth policy has an important implication for this calculus. Once the optimal amount of investment over the generation is chosen, decisions still remain as to the timing and more specific properties of individual investment projects. Some proportion of these decisions will be decentralized and a nonzero interest rate will probably enter into the calculations of the decentralized units. But since variations in this rate will not change the amount of capital forthcoming over the generation (though it can lead to changes within shorter time periods), and since the marginal productivity of investment over the generation is zero, all such interest charges are in the nature of a rent, from the point of view of the generation, and will be captured by the state. Thus one famous Marxian dictum is satisfied. Horvat's approach is novel in detail though the strategy is a hoary one.¹¹

The state captures all other nonwage quasirents too. This leaves labor and entrepreneurship as factors receiving functional shares. But the latter is a kind of labor and in the optimal regime accrues only to laborers, so in a general sense value is exclusively created by, and functionally allocated to, labor. Hence, though pricing is conventional, the moral content behind labor value theories is satisfied in Horvat's scheme.

If all Eastbloc economists were to accept this approach they would no doubt find it a good deal easier to discuss alternative price systems. However, Horvat has not made much use of the opportunity he has created. He does not discuss the administrative problems associated with price regulation in either a socialist or a state capitalist economy, aside from asserting that a market mechanism, based on profit maximizing as the incentive, does the best job in most cases. Nor does Yugoslav experience suggest that this problem will not be very troublesome in the socialist stage of society. Perhaps some more sophisticated versions of the theory will prove to be useful after all.

Horvat's sociological Marxism is most clearly exemplified in the twin propositions that state capitalism and worker management are the successively higher postcapitalist stages of society. The superior-efficiency aspect of their "inevitability" can be questioned in both cases. On the first of these assertions especially there is a gap in Horvat's argument. There is a good deal of discussion of the efficiency of bureaucracy as compared to small scale organization, but essentially the claim to superior efficiency of state capitalism rests on the assertion that, as a consequence of planning, growth rates are doubled or trebled. As empirical descriptions these figures are somewhat exaggerated but, quite aside from this, it may well be that higher growth is not really a consequence of bureaucracy but of the use of macro-economic instruments of control in order to maintain a high level of aggregate demand. If this is true, then the simple extrapolation of past trends toward greater concentration may generate false predictions. Apparently concentration ratios in the United States have tended to be relatively constant for three or four decades, larger size of organizations being roughly compensated by the growth of markets [2, p. 206-7].

¹¹Theodor Hertzka [13] is the earliest author I know who marries Marx with neo-classical economics by going to the point of capital saturation. Horvat's innovations are to claim a speedier arrival time and to assert that the saturated state can be maintained in the face of rapid technological change.

And the developing movement toward less centralized decision making in the Eastbloc without a simultaneous move toward worker management¹² opens the door to speculation that this trend is being aborted without a change in stage and with an increase in efficiency.

As for the second proposition, I, at least, find convincing the evidence that worker participation in enterprise decision making has been riding a strongly increasing trend in the twentieth century. What is not convincing is that "full" worker management possesses superior efficiency or that it is a very likely development. First, there are powerful political forces arrayed against the optimal regime, more even than socialists used to find arrayed against other types of socialism, for not only the capitalists but the trade unions are threatened by this development, as Horvat points out quite clearly. Secondly, there is really no mechanism for the collapse of state capitalism, especially since total bureaucratization does not seem a particularly plausible end result for that regime, but also because buying off of the workers is an accepted bureaucratic-capitalist strategy. Compromise is a poor substitute for deepening class conflict. Thirdly, the superiority of his optimum regime is not established. Administrative problems with the scheme have already been mentioned. But even if Horvat is right about the optimum growth path, he ought to be able to convince the state capitalists that they should adopt it, for it is a genuinely Pareto optimal solution in the sense that in principle everyone can gain from moving to that policy.

Finally, there is at least one possible alternative "next higher" stage which can substitute for state capitalism. There is no time to discuss it here, but it might be called "regional monopolism" and emphasize the great and apparently still growing relative differences among regions of the world. Standing in the way of the eradication of these differences are some of the world's most powerfully entrenched institutions, the affluent and militarily powerful nation states.¹³ Paths to a "revolutionary transformation" of this collection of institutions are perhaps somewhat easier to find (nuclear war, for example). At any rate, Horvat's optimum regime offers no answer to the problems posed by this situation. Indeed, the example of Yugoslavia suggests that regional conflicts are not well mediated by that approximation to the optimal regime even when regional interests are not defended by sovereign states. The synthesis just does not emerge here.

VII. *Conclusion*

Horvat's views have come under sharp criticism in the Yugoslav press.¹⁴ His use of "bourgeois" concepts and the "ahistorical" application of norma-

¹² So far at least. However, Gregory Grossman has argued [12, p. 55] that political pressures for more worker autonomy within the enterprise will probably follow decentralization.

¹³ Had he recognized the similarities between the Soviet Union and the United States on this dimension, Paul Baran would perhaps qualify as an adherent to this position by virtue of his well-known work on the political economy of growth [4].

¹⁴ These comments are based on two review articles [9] [20], a discussion of Horvat's theories of value and of planning, as presented by him to a conference in Yugoslavia [17] [21], and a discussion of his high-growth thesis as applied to Yugoslavia by a number of economists in the report of a conference [1].

tive theory to the discussion of such economic phenomena as prices have come under especially heavy fire, as has his use of a lone quote in throwing over the traditional Marxian value formalisms. It would seem that, in the early sixties no less than in the early fifties, Yugoslav ideologues are not prepared to accept attempts at "bridge building between the labor theory of value and the utility theory of value."¹⁵ However, in response, Horvat gives not an inch; indeed, there is a strong flavor of the "critique of the Gotha Program" in the tone of his reply to Pirec [15, pp. 272-74].

The combined proposal for more decentralization of allocative decision making with an increase in the already very high investment ratio also failed to strike a responsive chord in the Yugoslavia of early 1963, when a slackening in the growth rate led many economists to support a relatively greater emphasis on consumption plus decentralization. Probably this portion of Horvat's thesis would have found a more sympathetic audience two years earlier; perhaps also two years later. In this case, too, Horvat was not prepared to concede, and his own unmodified views have been published in several places.

Horvat's work represents a big step forward as compared to current Marxist writing with which I am familiar. One finds in Horvat neither the ritualistic obeisances to the classics and heroes of Soviet ideologues, nor the vapid emotionalism of many socialist humanists, nor the angry negativism of much of the American New Left. The arguments are usually incisive, intellectually challenging, but nonetheless couched in an aggressive style that Marx himself might have appreciated. True, attention is devoted to a discussion of the two highest stages of society (with even a glimpse or two into the more distant future), but, his Yugoslav opponents to the contrary, this does not qualify as utopianism, for Yugoslavia is assertedly a socialist country, an optimal regime. If Marxists who disagree with Horvat were to present alternative positions in as constructive a vein, Marxism might once again become a living body of thought.

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COMMUNICATIONS

Market Structure and the Employment of Scientists and Engineers

In the December 1965 *American Economic Review*, I reported results unfavorable to Professor Schumpeter's conjectures regarding the relationship between monopoly power and technological innovation [4, pp. 1117-21]. An analysis of 48 narrowly defined manufacturing industries revealed no significant tendency for the output of the industry leaders' 1954 patented inventions to increase with the industries' 1950 four-firm concentration ratios, *ceteris paribus*. Here I shall extend the analysis, utilizing data from a new and more comprehensive sample.

I. The Data

The hypothesis tested is that industrial inventive and innovative effort, measured by the employment of technical engineers and scientists, increases with the concentration of market power, other relevant variables such as total industry employment, technological opportunity, and product characteristics being held constant. Three dependent variables were used. One is the number of technical engineers E_i plus natural scientists S_i employed in 56 manufacturing industry groups during 1960. The second is the number of natural scientists S_i only (averaging 14 per cent of total technical employment $E_i + S_i$). Estimates of both E_i and S_i were obtained from a five per cent sample of the 1960 Census of Population [6].¹

The third dependent variable requires more explanation. Although production engineers, quality control mathematicians, and similar personnel are undoubtedly responsible for some inventions and innovations [1][5], the prime source of new industrial technology is presumably the formally organized research and development laboratory. But only 45 per cent of U.S. manufacturing industry's scientific and engineering employees during 1960 were engaged in formal R&D work [8, p. 25]. Furthermore, only 57 per cent of (nonaircraft) industry's R&D effort was supported with private funds, and hence subject to the market incentives to which Schumpeter's market structure hypotheses are solely applicable [7, p. 65]. Consequently, a third estimated private research and development employment variable $RD_i = (F_i)(P_i)(E_i + S_i)$ was defined, where F_i is the proportion of the i th industry's scientific and engineering work force engaged in formal R&D and P_i is the fraction of the industry's R&D financed privately.²

¹ The engineering employment variable excludes sales engineers, while the scientific employment variable includes mathematicians. Three "not specified" catchall groups were excluded from the sample. In addition, the aircraft and shipbuilding industries were left out because their technical activities lie for the most part outside the private market sector, and newspaper publishing was excluded because of its peculiar product and market characteristics.

² Data on F_i and P_i were obtained for 13 and 19 broader industry groups respectively from [8] and [7]. These ratios were applied, after a few adjustments based upon supplemental knowl-

The definitions of the 56 industry groups sampled (covering nearly all of U.S. manufacturing industry for 1960) were governed by the availability of data. Classification problems forced the Census Bureau to use a heterogeneous system in aggregating the occupational data by industry. The industry groups ranged from narrow clusters of one or a few four-digit S.I.C. industries (i.e., synthetic fibers) to quite broad two-digit sectors (i.e., electrical machinery, equipment, and supplies), although most were at the three-digit level of aggregation. It was therefore necessary to use a weighted average index of concentration as the independent variable of primary relevance to the hypothesis tested. The concentration index C_i of the i th industry group is the average (weighted by value of shipments) of the 1958 four-firm concentration ratios for all four-digit S.I.C. industries included in that group [9, pp. 11-42].

To take into account interindustry differences in scientific and technological opportunity, dummy variables are defined for four broad classes: general and mechanical (G&M), electrical, chemical, and traditional. Each industry group was assigned to one of these classes after analysis of the group's product technology characteristics.³ Choices involving the electrical and chemical classifications were straightforward, but it was difficult to avoid some arbitrary judgments in distinguishing between industries with G&M as opposed to traditional product technologies. In borderline cases, the G&M classification (used as the intercept class in all regressions) was favored.

Three additional dummy variables were used to distinguish durable from nondurable goods industries, consumer from producer goods industries, and industries characterized by local or regional markets from those essentially nationwide in scope.

II. The Model and Tests

If Schumpeter's conjectures are correct, one would expect the absolute differences in technical employment associated with concentration to be greater for large industries than for small. Past experience with similar data has shown that the error terms of untransformed linear regressions on the industry size variable are heteroscedastic, and that observations from extremely large industries can dominate the regression estimates. These theoretical and statistical considerations call for either a multiplicative or ratio specification of the model. In my 1965 article a multiplicative model was chosen to test the concentration hypothesis. With the present sample variables, the model can be written:

$$(1) \quad (E_i + S_i) = aN_i^\alpha C_i^\beta \left(\prod_j 10^{\delta_j D_{ij}} \right) u_i;$$

edge, to the 56 narrower industry groups. The resulting estimates are crude, although there is no reason to suppose that the estimation errors are systematic.

³ The classifications conform closely to those found significant in [4, pp. 1103 and 1107]. A table giving the industry classifications and other raw data used in the study is available upon request from the author.

where N_i is total 1960 employment of the i th industry (scaled in thousands), D_{ij} is a dummy variable with the value of 1 if the i th industry is in the j th technology or product characteristic class and zero otherwise; and u_i is the error term. The dummy variables in this model relate to the slope of the regression hyperplane with respect to both industry size N_i and concentration C_i . This specification implies that as C_i tends to zero, $(E_i + S_i)$ also tends to zero, no matter how high δ_j and N_i are—that is, no matter how large the industry is and how favorable the technological opportunities confronting the industry are. But it seems more reasonable that, in a field as enriched as, say, the electronics industries are with opportunities opened up by the advance of science, many scientists and engineers would be employed even if the industry structure were atomistic. While the specification of equation (1) might serve adequately over the range of concentration values actually observed (the lowest was 10 per cent), the use of an alternative ratio model appears preferable on a priori grounds:

$$(2) \quad \frac{(E_i + S_i)}{N_i} = c + \sum_j d_j D_{ij} + g C_i + v_i.$$

In this form, the influence of interindustry differences operates independently of the concentration level. Even when the industry is atomistic ($C_i \rightarrow 0$) it can have a significant fraction of its work force assigned to scientific and engineering tasks.

Because it appears more consistent with a priori knowledge, I am inclined to favor model (2) over model (1). But since the case for model (2) is not ironclad, and since the conclusions depend to some extent upon the choice of models, I shall first summarize the tests of model (1). Regressions were run in logarithmic form for the three dependent variables on N_i alone; on N_i with C_i ; and on N_i with all dummy variables, with and without C_i . When the concentration variable was introduced into regressions of $\log(E_i + S_i)$, $\log S_i$, and $\log RD_i$ on $\log N_i$ without dummy variables, its regression coefficients were in all cases positive and highly significant, and it made an incremental contribution to the percentage of variance explained (100 R^2) of 25.5, 15.9, and 22.4 percentage points respectively.

The concentration regression coefficients remained positive and significant when all dummy variables (and also subgroups of the dummies) were introduced, as shown in the following equations, where $Elec_i$ is the electrical class dummy, $Chem_i$ the chemical class dummy, $Trad_i$ the traditional product technology class dummy, Reg_i the regional market dummy, Dur_i the durable goods dummy, and $Cons_i$ the consumer goods dummy. Standard errors are given in parentheses under the coefficients.

$$(3) \quad \log(E_i + S_i) = - .03 + .95 \log N_i + .94 \log C_i + .30 Elec_i \\
\begin{array}{ccccccc}
& (.11) & & (.24) & & (.24) & \\
+ .44 Chem_i & - .47 Trad_i & - .05 Reg_i & + .05 Dur_i & & & \\
& (.17) & (.11) & (.13) & (.10) & & \\
- .14 Cons_i; & & & & & & R^2 = .830. \\
& (.09) & & & & &
\end{array}$$

$$\begin{aligned}
 (4) \quad \log S_i &= 1.09 + .95 \log N_i + 1.34 \log C_i + .10 Elec_i \\
 &\quad (.22) \quad (.48) \quad (.49) \\
 &\quad + .63 Chem_i - .54 Trad_i + .14 Reg_i - .67 Dur_i \\
 &\quad (.34) \quad (.22) \quad (.26) \quad (.21) \\
 &\quad - .37 Cons_i; \quad R^2 = .600. \\
 &\quad (.18) \\
 (5) \quad \log RD_i &= - .36 + .93 \log N_i + .80 \log C_i + .27 Elec_i \\
 &\quad (.11) \quad (.24) \quad (.24) \\
 &\quad + .51 Chem_i - .41 Trad_i - .01 Reg_i + .10 Dur_i \\
 &\quad (.17) \quad (.11) \quad (.13) \quad (.10) \\
 &\quad - .09 Cons_i; \quad R^2 = .805. \\
 &\quad (.09)
 \end{aligned}$$

When equations (3) through (5) were computed with the concentration variable deleted, the amount of variance explained ($100 R^2$) fell by 5.5, 6.6, and 4.5 percentage points respectively. Thus, the incremental explanatory power of concentration is much less when dummy variables are included than when the concentration variable is used with industry employment N_i alone. Still all of the incremental variance gains due to including C_i in equations (3) through (5) are significant in F -ratio tests at the 1 per cent level. These results therefore tend to support Schumpeter's hypothesis that inventive and innovative activity increases with market concentration.

Turning now to the ratio model, the simple correlations involving only technical employment, total employment, and concentration also supported the hypothesis, although not as consistently. The r^2 's were .21 between $(E_i + S_i)/N_i$ and C_i ; .19 between RD_i/N_i and C_i ; but only .027 between S_i/N_i and C_i . Introduction of the six dummy variables led to much sharper declines in the explanatory power of concentration than in the multiplicative model. The equations were as follows (with the dependent variables scaled in technical personnel per 1,000 total employees):

$$\begin{aligned}
 (6) \quad \frac{E_i + S_i}{N_i} &= 16.4 + .155C_i + 38.1 Elec_i + 35.7 Chem_i - 14.3 Trad_i \\
 &\quad (.109) \quad (9.0) \quad (6.6) \quad (4.1) \\
 &\quad - 4.6 Reg_i + 4.9 Dur_i - 1.3 Cons_i; \quad R^2 = .728. \\
 &\quad (5.0) \quad (4.0) \quad (3.5) \\
 (7) \quad \frac{S_i}{N_i} &= 5.0 + .01C_i + .36 Elec_i + 25.4 Chem_i - 2.2 Trad_i \\
 &\quad (.05) \quad (4.16) \quad (3.0) \quad (1.9) \\
 &\quad - 1.0 Reg_i - 2.3 Dur_i - .45 Cons_i; \quad R^2 = .715. \\
 &\quad (2.3) \quad (1.8) \quad (1.61)
 \end{aligned}$$

$$\begin{aligned}
 (8) \quad \frac{RD_i}{N_i} = & 4.3 + .054C_i + 8.4 \textit{Elec}_i + 11.9 \textit{Chem}_i - 4.0 \textit{Trad}_i \\
 & \quad (.036) \quad (3.0) \quad (2.2) \quad (1.4) \\
 & - 1.1 \textit{Reg}_i + 1.5 \textit{Dur}_i + .3 \textit{Cons}_i; \quad R^2 = .669 \\
 & \quad (1.7) \quad (1.3) \quad (1.2)
 \end{aligned}$$

In equations (6) and (8) the concentration coefficient is barely significant at the 10 per cent level in a one-tail test; in equation (7) it is clearly not significant. Deletion of the concentration variable from equations (6) and (8) causes reductions in the amount of variance explained of 1.1 and 1.6 percentage points respectively—increments significant only at the 20 per cent level in *F*-ratio tests.

To determine whether the regression relationship between intensity of technical employment and concentration was consistent within individual technology classes, separate linear regressions (omitting product characteristic dummy variables) were computed for the classes on which a reasonably large number of observations was available. For the 25 industries classified in the traditional product technology category, the fitted equations were:

$$(9) \quad \frac{E_i + S_i}{N_i} = 1.20 + .176C_i; \quad r^2 = .223. \\
 \quad \quad \quad (2.17) \quad (.063)$$

$$(10) \quad \frac{S_i}{N_i} = -.18 + .079C_i; \quad r^2 = .134. \\
 \quad \quad \quad (1.33) \quad (.042)$$

For the 24 industries classified as having general and mechanical product technologies, the following estimates resulted:

$$(11) \quad \frac{E_i + S_i}{N_i} = 13.36 + .291C_i; \quad r^2 = .091. \\
 \quad \quad \quad (8.68) \quad (.197)$$

$$(12) \quad \frac{S_i}{N_i} = .71 + .070C_i; \quad r^2 = .091. \\
 \quad \quad \quad (2.07) \quad (.047)$$

In all four cases the concentration coefficients are positive. All pass statistical significance tests at the 10 per cent level or higher, with the traditional group showing a somewhat more consistent tendency for the intensity of technical employment to increase with concentration.⁴

⁴ For the $(E_i + S_i)/N_i$ regressions, a test of the hypothesis that the traditional class intercept is greater than or equal to the G&M class intercept was rejected at the 10 per cent level, but a similar hypothesis regarding the slopes can be rejected only at the 35 per cent level. This lends support for regression model (2), which assumes different intercepts but equal slopes, over model (1), which assumes identical (zero) intercepts but unequal slopes. For the S_i/N_i regressions, there is no indication of significant differences in either the slopes or intercepts.

Simple linear regressions for industries in the chemical technology class revealed a *negative* correlation between both $(E_i + S_i)/N_i$ and S_i/N_i and concentration. However, the regression coefficients had *t*-ratios of only .61 and 1.24 respectively, and only five observations were involved.

III. *Interpretation*

We find then divergent results. Using one specification of the model, Schumpeter's hypothesis is sustained with flying colors. Using the alternative and theoretically preferred specification, the support is weaker but not entirely absent. Differences in the role of the dummy variables, and especially the technology class dummies, underlie this divergence. Still one important result is common to both models. When the dummy variables are introduced, the incremental explanatory power of concentration falls sharply.

Further analysis shows that the technology class dummies compete with concentration for explanatory power because they are positively correlated. The technically vigorous electrical and chemical groups had average and minimum concentration indices well above the full sample means. Average concentration in the electrical subsample (with only two observations) was 58, with a minimum value of 48. For the five chemical observations the average was 44, with a minimum of 31. The G&M group had an average *C_i* index of 41, with a range of from 18 to 70. In distinct contrast, the least progressive traditional class had an average of 29, with a minimum of 10 and only one observation (tobacco products) exceeding 50. Two alternative causal chains are compatible with this interdependence. The electrical and chemical classes might be more progressive on the average because they are more concentrated, or they may be more concentrated because in the past they have been more progressive.

Neither possibility can be rejected conclusively. It is clear, however, that the greater apparent progressiveness of the chemical and electrical groups is not due solely to higher concentration, for science has obviously been exceptionally generous to these groups during the past century. More support can be mustered for the contention that technological innovation associated with opportunity has led to concentration.⁵ The high concentration of market power in such fields as synthetic fibers, plastics materials, electric lamps, telephone equipment, and computing equipment was built at least partly upon patent and know-how barriers to entry and (in an earlier era) restrictive patent cross-licensing agreements. The successful exploitation of favorable opportunities may also have permitted especially rapid market share growth for innovating firms, contributing to above-average concentration. On the other hand, the undramatic pace of technological advance in such product areas as sawmill products, furniture, bakery products, shoes, and canned vegetables afforded little opportunity for the erection of strong patent and know-how entry barriers. It seems reasonable to conclude tentatively that the electrical and chemical technology class dummy variables have not captured explanatory power more appropriately attributable causally to concentration, although the possibility of a reverse flow from past opportunity to present concentration cannot be excluded.

Once interindustry opportunity differences are taken into account, the tendency for technical employment to increase with concentration is most persistent for the traditional industries, which are least concentrated on the

⁵ See also Almarin Phillips' analysis [3], which stresses this possibility.

average. This suggests a possible threshold effect: increases in concentration are conducive to technical vigor only in relatively atomistic industries, becoming an unimportant stimulus once a certain broad threshold is crossed [2]. To explore this possibility, an additional variable C_1^2 was introduced into regressions otherwise identical to equations (9) through (12). This test for nonlinearities yielded modest support for the hypothesis. In all four regressions, a relationship concave to the concentration axis was found, although only in the nonlinear analogue of equation (9) was the squared term's (negative) coefficient significant at the 5 per cent level. In all four cases technological employment per 1,000 employees reached a predicted maximum at concentration levels between 50 and 55 per cent—values exceeded only by the tobacco products industry in the traditional class, but by five to eight industries in the G&M class. The four regression equations predicted a zero ratio of technical to total employment at concentration levels between 10 and 14 per cent, suggesting that the threshold lies somewhere above this low range.

These results appear consistent with the neo-Schumpeterian hypothesis that oligopolists display a special affinity toward nonprice competition. Some degree of concentration is required before firms eschew price-cutting and grapple for market position through more complex innovative strategies. But in industries with high concentration—e.g., when the four-firm ratio exceeds 55 per cent—pricing interdependence is fully recognized, and group discipline may even be sufficiently strong to permit a “live and let live” attitude toward technological innovation.

IV. Conclusion

Three tentative conclusions emerge. First, the relationship between industrial inventive and innovative effort and concentration is a complex one, since high concentration and rich technological opportunity tend to coincide. Naïve tests of the Schumpeterian market power hypotheses are not apt to add much to our understanding. Second, even after interindustry differences in technological opportunity are taken into account crudely, there remains evidence of a modest positive correlation between the employment of scientists and engineers and concentration. This correlation is stronger than the results of my 1965 analysis suggested—undoubtedly because industries with traditional product technologies were deliberately excluded from the sample. And third, technological vigor appears to increase with concentration mainly at relatively low levels of concentration. When the four-firm concentration ratio exceeds 50 or 55 per cent, additional market power is probably not conducive to more vigorous technological efforts and may be downright stultifying.

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A Graphical Proof of a Property of a Closed Linear Model of Production

The following are two well-known and related properties of a sufficiently well-behaved closed linear model of production: (1) All efficient infinite sequences of accumulation of capital goods converge to a balanced growth sequence. (2) The efficiency frontiers tend to "flatten out" as time goes on. For special models, the literature contains graphical proofs (or proofs using very simple mathematics) of the first property (see, for example, [3]) but not of the second. The purpose of this note is to present a graphical proof of (2) for a quite well-behaved model.

The two assumptions of the model that I shall use directly are the assumption of constant returns to scale and the assumption that the convex combination of two production processes is a production process. I shall not discuss here the more general set of assumptions underlying a strictly convex closed linear model of production [2].

A quite well-behaved closed linear model of production like that in [3] has the following implications.¹ With stocks of capital goods represented by point a in Figure 1, the state of technology allows the economy to reach, after engaging in productive activity, any point on or below the transformation curve AA' . That is, with point a as the starting point at time 0, AA' is the upper boundary of the set of points that can be achieved at time 1. Assume that there is no consumption, and so the points of AA' are

¹ The assumption on input intensities essential in [3] is not, however, essential in the present paper.

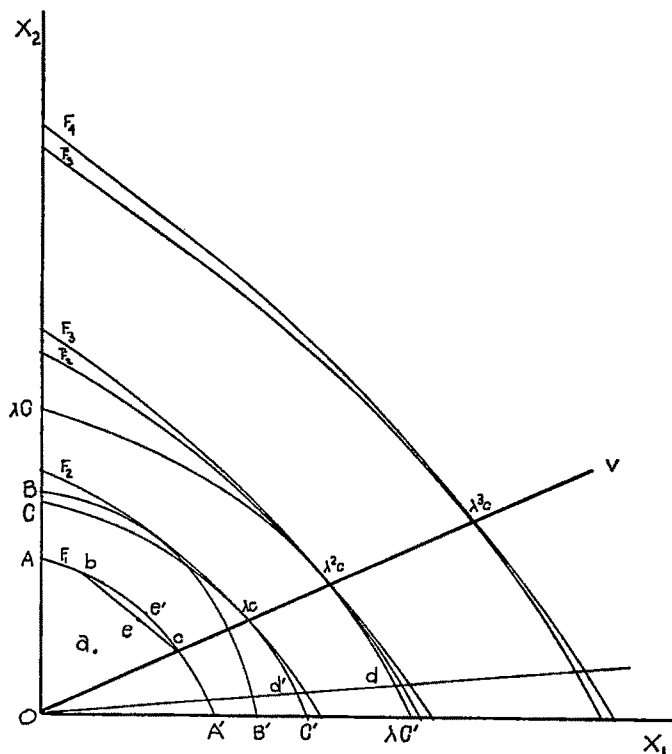


FIGURE 1

available for use as input points at time 1. AA' is the efficiency frontier, F_1 , at time 1.

Using the stocks of commodities represented by points in AA' , the economy can reach at time 2 any point on or below the efficiency frontier F_2 . Any point on or below BB' might, for example, be reached from point b , any point on or below CC' from point c , etc. F_2 is the envelope of curves like BB' and CC' . F_3 , F_4 etc. are similarly constructed.

There is a unique maximal balanced growth path—a unique von Neumann ray—such as v in Figure 1. (I assume that v is not one of the axes.) Let λ be the unique von Neumann growth factor. Growth along the von Neumann ray is known to be always efficient. That is, if the initial endowment is, like point c in Figure 2, a point in v , then λc is in F_1 , $\lambda^2 c$ is in F_2 , and so on. There are constant returns to scale, so that $\lambda c \lambda c'$ is associated with λc just as CC' is associated with c ; and any point such as d in $\lambda c \lambda c'$ in Figure 1 is λ times the point in CC' that lies on the same ray from the origin.

In models with efficiency frontiers shaped like those in Figure 1, each point on F_2 is reached by a unique path from a . For it can be shown that a single point in F_2 cannot be reached from two distinct points in F_1 . For example, suppose that the point λc could be reached from both point b

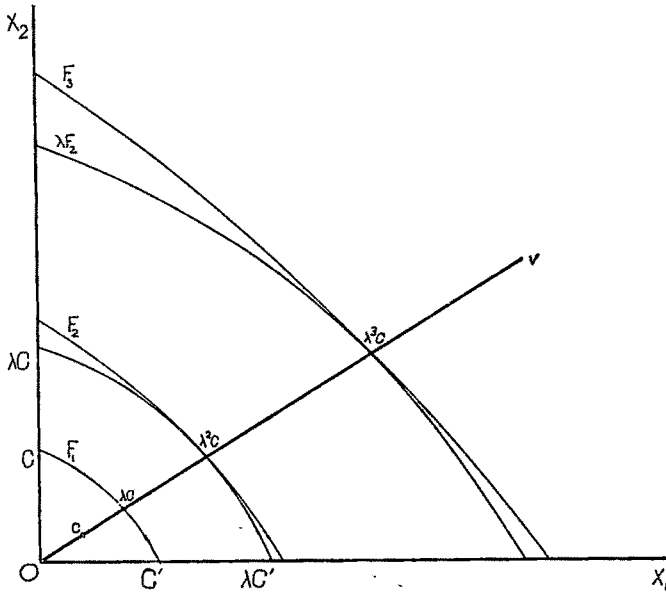


FIGURE 2

and point c . Then, because the convex combination of two production processes is a production process, point λc could also be reached from point e , a point on the line segment connecting b and c . And, because there are constant returns to scale, a point on v that is further from the origin than λc is could be reached from point e' . Because this result would contradict the fact that λc is on the efficiency frontier F_2 , our supposition that λc could be reached from both b and c must have been wrong.

In a model as well behaved as that in [3], it is not only true that a single point in F_2 can be reached from only a single point in F_1 ; but it is also true that two distinct points in F_2 cannot be reached from a single point in F_1 . In other words, curves like BB' and CC' each contribute only one point to the efficiency frontier F_2 . This is an important property as far as the following demonstration is concerned.

In Figure 2, the economy's initial endowment of stocks of capital goods happens to be in the unique von Neumann ray. In this special case, it is very easy to show that the efficiency frontiers tend to flatten out as time goes on. Starting at point c , the economy can reach any point in CC' in one time-period, any point in F_2 in two time-periods, any point in F_3 in three time-periods, etc. But starting at point λc , a point in CC' , the economy can reach, in one period, any point in $\lambda C\lambda C'$ and, in two time-periods, any point in λF_2 . At least one point of $\lambda C\lambda C'$, viz, the point $\lambda^2 c$ is in F_2 ; and at least one point of λF_2 , viz, the point $\lambda^3 c$, is in F_3 . Assume that the model is sufficiently well behaved that $\lambda^2 c$ is the only point of $\lambda C\lambda C'$ that is in F_2 , and $\lambda^3 c$ is the only point of F_2 that is in F_3 . Then it follows that, multiplying each point in F_t by $\lambda^{(1-t)}$, we get a situation like that depicted

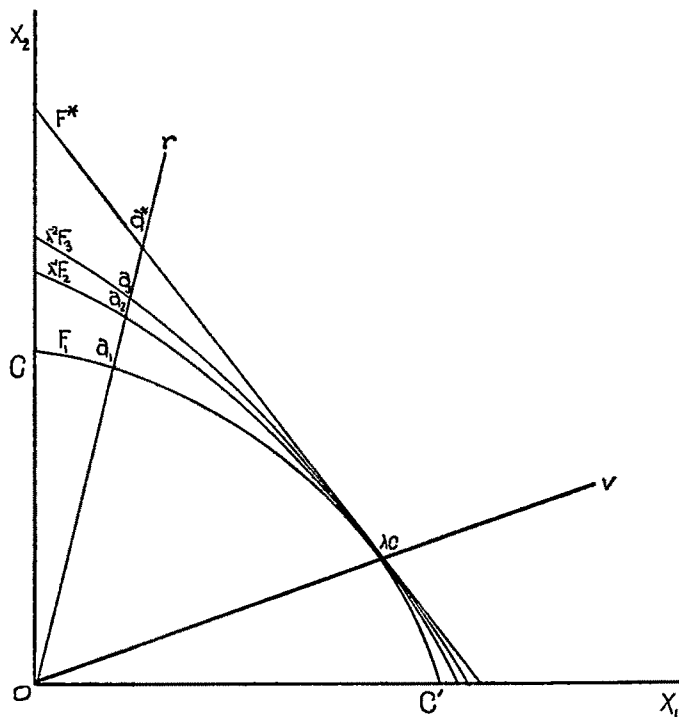


FIGURE 3

in Figure 3. $\lambda^{-1}F_2$ must lie on or above CC' and contain only the point λc of CC' , because F_2 lies on or above $\lambda C\lambda C'$ and contains only the point $\lambda^2 c$ of $\lambda C\lambda C'$. $\lambda^{-2}F_3$ must lie on or above $\lambda^{-1}F_2$ and contain only the point λc of $\lambda^{-1}F_2$, because F_3 lies on or above λF_2 and contains only the point $\lambda^3 c$ of λF_2 , and so on. Thus, it is evident that, as time goes on, the efficiency frontiers tend to flatten out.

It can, in fact, be shown that the curve $\lambda^{(1-t)}F_t$ approaches the straight line F^* as t approaches infinity. Take any ray, such as r in Figure 3, that is not the von Neumann ray. Let a^* be the point at which r intersects F^* , and let a_t be the point at which r intersects $\lambda^{(1-t)}F_t$. The distance between a_t and a^* decreases monotonically as t increases. We know that the sequence of points a_1, a_2, \dots cannot include a point that has coordinates greater than those of a^* (i.e., a_t cannot be further than a^* is from the origin); because we know that $\lambda^{(1-t)}F_t$ cannot be convex to the origin. Therefore, we know that the distance between a_t and a^* converges to zero as t approaches infinity. Because the ray r was chosen arbitrarily, this convergence implies that, as t approaches infinity, the maximum distance between a point in F^* and a point in $\lambda^{(1-t)}F_t$ that lies on the same ray from the origin approaches zero; and so, as t approaches infinity, $\lambda^{(1-t)}F_t$ approaches the straight line F^* .

This, of course, shows only the flattening out of efficiency frontiers in

the special case in which the economy's initial endowment happens to lie in the von Neumann ray. Refer, then, to the situation shown in Figure 1; and suppose that the economy ignores all but one of the opportunities represented by points in AA' and chooses to reach only point c , which is in the von Neumann ray. Then, imagining that c is really the economy's initial endowment, we can construct pseudo efficiency frontiers, CC' , \bar{F}_2 , \bar{F}_3 , etc. \bar{F}_t approaches a straight line as t approaches infinity. It is not in general true that \bar{F}_{t-1} is tangent to F_t at $\lambda^{t-1}c$, unless the initial endowment happens to be in v . But property (1) can be shown to imply that, if t is very large and v intersects F_{t-1} at the point c' , $\lambda c'$ is below but arbitrarily close to F_t and \bar{F}_{t-1} is "almost" tangent to F_t at $\lambda c'$. The limiting \bar{F}_t has been shown to be a straight line. Thus, property (2) holds for this very well-behaved closed linear model of production.

Without loss of generality, it might be assumed that \bar{F}_{t-1} becomes almost tangent to F_t at time 2. Then Figure 1 would illustrate the eventual flattening out of efficiency frontiers.

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A Paradox on Profits and Factor Prices*

When entry of foreign agricultural workers into the United States was curtailed, most individuals, and perhaps economists, indicated that agricultural wage rates would rise, and farm profits fall. However, in 1965 both wages and profits in the farm sectors most affected by the bracero cutback increased [7, pp. 1, 14]. Is the similar movement of the wage rate and profits coincidental, or does a paradox exist: profits may increase if entrepreneurs can convince labor to demand a higher wage? An examination of several price theory texts failed to substantiate the paradox. The purpose of this note is to prove that an increase in the price of a factor of production may

* After this note was written and accepted for publication, Nelson's article [3] was brought to my attention. This note is independent of Nelson's work and more general. Nelson also shows that an increase in the price of a factor of production may increase short-run profits (rents). However, while we state the necessary condition for the occurrence of the paradox irrespective of the number of variable factors, Nelson assumes that there is but a single variable factor, and his necessary condition is correct only when his restrictive assumption holds. Cf. footnote 6.

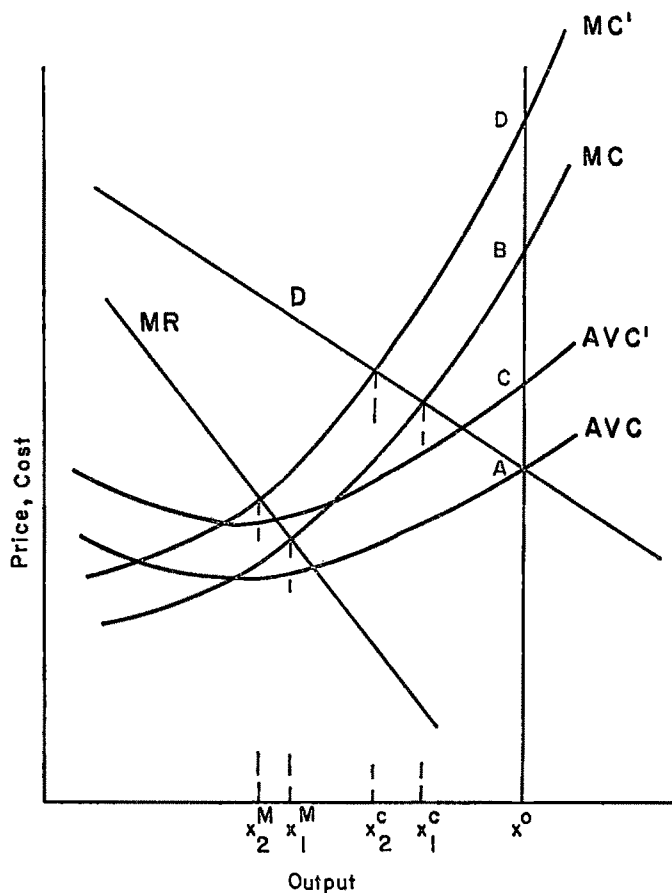


FIGURE 1

increase short-run profits in a competitive industry.¹ A rise in factor prices lowers equilibrium output but increases profits per unit. The net effect on total profits is likely to be positive sufficiently often so as not to reduce the paradox to a mere *curiosum*.

I

In this section we present a diagrammatic proof of the paradox. For simplicity, assume that there is only one variable factor, labor. Given this assumption, an increase in the wage rate leads to an equal percentage increase in marginal costs. In Figure 1, say, a 25 per cent increase in the wage rate shifts short-run industry marginal costs, MC , vertically by 25 per cent to MC' . The percentage increase in average variable costs of

¹ Of course, long-run profits in a competitive industry always equal zero, provided factor prices reflect properly capitalized rents.

any output also equals the percentage increase in wages. Since marginal costs are larger than average variable costs at equilibrium, an equal percentage increase in both means that the marginal cost-average variable cost spread, henceforth called simply the "differential," widens. At equilibrium, price equals marginal costs, and profits can be written as the product of output times the differential, minus fixed costs. If demand were inelastic at x^0 , it is clear that profits must increase, since output is constant, and the differential widens. BA is less than DC . Though output will usually decline, total profits still may increase because profits per unit increase. The necessary conditions are presented in the following mathematical section, where we also drop the single factor assumption.

Higher input prices always reduce monopoly profits. In monopoly, equilibrium output equals the industry profit-maximizing output. Total costs of any given output are larger after the input price rises, and the industry profit-maximizing output falls as the cost curves shift up. If the new, reduced equilibrium output, x_2^M , had been the output prior to the increase in wages, profits at the original wage rate would have been larger. But at original factor prices, profits at x_2^M were smaller than profits at the initial equilibrium output, x_1^M , where marginal revenue equals marginal cost. Consequently, profits must be less at x_2^M than at x_1^M .

In the competitive model, however, the penultimate sentence is false. Precisely because it is false, it does not follow that higher wages will decrease profits in a competitive industry. At the original wage rate, industry profits at x_2^C , the new competitive equilibrium output, would have been larger than profits at x_1^C , since marginal revenue is less than marginal cost between x_2^C and x_1^C . It is impossible to state a priori if profits in a competitive industry will rise or fall as factor prices change.²

II

As a matter of definition, industry profits equal total revenue minus total costs. Algebraically,

$$(1) \quad \Pi = Px - C(x, w_i),$$

where Π equals profits; P , price; x , industry output; $C(x, w_i)$, the total cost function; w_i , the price of the i th factor of production. Factor prices are parameters to the industry. Assume an increase in w_i . Prices of factors other than the i th remain fixed throughout the analysis and, as such, enter the cost function as parameters. Differentiation of (1) to determine the change in profits with respect to w_i gives³

$$(2) \quad \frac{d\Pi}{dw_i} = \frac{x\partial P}{\partial w_i} + \frac{P\partial x}{\partial w_i} - C_x \frac{\partial x}{\partial w_i} - C_i.$$

An increase in the factor price raises profits when (2) is positive.

² Higher factor prices to only one firm will reduce its profits, since a single competitor is like a monopolist in the sense that production is where marginal cost equals marginal revenue.

³ Notationally, $C_i = \partial C(x, w_i) / \partial w_i$; $C_x = \partial C(x, w_i) / \partial x$.

In order to evaluate (2), we first determine the change in equilibrium price and output. Equality of supply and demand price is the market equilibrium condition in a competitive industry.

$$(3) \quad P = D(x) = S(x, w_i),$$

where $D(x)$ is the demand relation, and $S(x, w_i)$ is the supply relation. One could solve for equilibrium output and price explicitly in terms of factor price. To calculate the change in equilibrium price and output with respect to an increase in w_i , $\partial P/\partial w_i$ and $\partial x/\partial w_i$, respectively, we differentiate (3),

$$(4) \quad \frac{\partial P}{\partial w_i} = D' \frac{\partial x}{\partial w_i} = S_i + S_x \frac{\partial x}{\partial w_i}.$$

The values of the derivatives, as well as the values of price and output in (1) and (2), are determined at the equilibrium point that satisfies (3). Transposing terms, one gets

$$(5) \quad \begin{aligned} \frac{\partial x}{\partial w_i} &= \frac{S_i}{D' - S_x}, \\ \frac{\partial P}{\partial w_i} &= \frac{D'S_i}{D' - S_x}. \end{aligned}$$

S_i , the change in supply price with respect to a change in the factor price, output being held constant, is positive. Assuming "normally" sloped demand and supply curves, the changes in equilibrium output and price are negative and positive respectively, as anticipated.

Two well-known conditions of production theory are noted without proof. First, a necessary condition for minimization of costs for any given output is the equality of marginal costs and the factor price-marginal physical product ratios of all n variable factors [5, pp. 65-66]. Assuming profit-maximizing competitors, marginal costs are equal to supply price.⁴ Therefore,

$$(6) \quad S(x, w_i) = C_x = \frac{w_i}{\Phi_i}, \quad (i = 1, \dots, n)$$

where $x = \Phi(v_1, \dots, v_n)$ is the production function, and Φ_i is the marginal physical product of the i th factor. Second, the first-order change in total costs with respect to a higher factor price, output being held constant, is proportional to the quantity of the factor [5, p. 68].

$$(7) \quad C_i = v_i, \quad (i = 1, \dots, n)$$

⁴ Strictly speaking, marginal costs equal supply only when the former are rising and greater than average variable costs. It is assumed throughout that all second-order conditions are fulfilled, and that no external economies exist.

Differentiating (6) with respect to w_i , noting that price equals marginal cost at equilibrium, and substituting (5) and (7) in (2), one gets⁵

$$(8) \quad \frac{d\Pi}{dw_i} = \frac{Px}{w_i} \cdot \frac{D'}{D' - S_x} - v_i.$$

As it stands, it is difficult to estimate the sign of (8) since the right-hand side contains absolute numbers, as opposed to ratios. Therefore, dividing numerator and denominator of the first term by $S_x D'$, and letting the share of v_i (that is, $w_i v_i / Px$) equal β ,

$$(9) \quad \frac{d\Pi}{dw_i} = v_i \left\{ \frac{\eta_s}{\beta(\eta_s - \eta_D)} - 1 \right\},$$

where η_D and η_s are the price elasticities of demand and supply respectively taken with their appropriate algebraic sign. Of course, v_i is positive, so profits increase when the expression in brackets is positive. Rearranging terms shows that it is positive when the absolute value of the ratio of demand elasticity to supply elasticity is less than the ratio of the share of all other productive factors to the share of the higher-priced factor. Mathematically, the necessary condition is⁶

$$(10) \quad \left| \frac{\eta_D}{\eta_s} \right| < \frac{1 - \beta}{\beta}.$$

Either a perfectly inelastic demand or perfectly elastic supply is a sufficient, though not necessary, condition for a higher factor price to result in larger profits. Differentiating equation (9) would show that, *ceteris paribus*, it is more likely that profits will increase (1) the smaller the share of the higher-priced input, (2) the more inelastic is demand, or (3) the more elastic is supply. It is interesting that profits may increase even if demand is elastic, in which case total revenue decreases as input and equilibrium output prices increase.

Though a unit tax like higher factor prices shifts supply upward, a tax generally will lower profits. The profit and market equilibrium equations respectively are

$$(1') \quad \Pi = Px - C(x) - tx,$$

$$(3') \quad P = D(x) = S(x) + t,$$

⁵ From (6), $S_i = 1/\Phi_i = C_x/w_i$, and at equilibrium $C_x = P$.

⁶ Using our notation, Nelson states that an increase in the price of a factor will raise profits when $|\eta_D/\eta_s| < \eta_{AVC}$ [3, p. 391]. We have $(1-\beta)/\beta$ on the right-hand side of the inequality. When there is only one variable factor, the case considered by Nelson, the two inequalities are identical as η_{AVC} equals $(1-\beta)/\beta$. In the single variable factor case, $v_i w_i/x = AVC$ and $\eta_{AVC} = (x w_i dv_i/dx - v_i w_i)/v_i w_i$. At equilibrium, $P = w_i dv_i/dx$, and $\eta_{AVC} = (xP - v_i w_i)/v_i w_i = (1-\beta)/\beta$. However, in the general case of several variable factors, Nelson's inequality is incorrect. In general, whether profits will increase depends upon the share of the higher-priced factor, and not the elasticity of average variable costs.

where t equals the unit tax. Performing calculations similar to (2)–(8), one gets

$$(9') \quad \frac{d\Pi}{dt} = x \left\{ \frac{\eta_D}{\eta_S - \eta_D} \right\}.$$

If demand and supply are negatively and positively sloped respectively, (9') cannot be positive. Profits remain unchanged when a unit tax is levied only if demand is perfectly inelastic, or supply is perfectly elastic.

The difference between higher factor prices and a unit tax lies in that the tax does not change the differential, while higher input prices increase the differential. For example, in Figure 1, higher wages shift MC and AVC vertically by the same percentage and, consequently, the differential widens. A unit tax, however, raises MC and AVC by the amount of the tax. MC and AVC are vertically displaced by the same absolute, not percentage, amount. Since the differential remains constant, and output usually declines, a tax tends to reduce profits.

III

How often will a rise in the price of a factor increase profits? It appears that (10) will be satisfied relatively frequently. For example, if the ratio of demand elasticity to supply elasticity equals one, profits increase provided the share of the higher-priced factor is less than one-half. Profits generally will increase when the share of the factor is small. If the factor's share is ten per cent, profits increase provided the ratio of demand to supply elasticities is less than nine, a condition which casual empiricism suggests will usually hold. Though a change in the price of only one factor has been assumed, it should be clear that higher prices for several factors do not preclude enlarged profits.

While it is not our purpose to explain recent changes in agricultural profits in terms of the wage rate, nor even to suggest that the simplistic competitive model is entirely appropriate, the values for factor shares and elasticities of supply and demand are such that within the competitive model a higher wage rate would enlarge agricultural profits. In view of the numerous statistical estimates of factor shares and price elasticities, one feels rather uneasy about giving a single, point estimate. Nevertheless, labor's share in agriculture is taken to be 65 per cent [2, p. 215]. Braceros were employed primarily in the production of fruits and vegetables. The elasticity of demand for fruit and tree nuts has been estimated to be -0.36 , and the elasticity of demand for vegetables is estimated at -0.10 [1, p. 59]. It follows from (10) that profits in the fruit and tree nuts and vegetables "industries" would increase if the elasticities of supply were no less than 0.70 and 0.20 respectively. Though statistical estimates of supply are somewhat less numerous and more unreliable, estimates of short-run supply elasticities of other farm products are often between 0.50 and 1.0 [4] [6, pp. 94–105]. Therefore, profits in the vegetable industry should increase, and perhaps in the fruit and tree nuts industry as well. To repeat, we provide the example only because agriculture is considered to be the com-

petitive industry par excellence, and not because demand shifts and other exogenous changes are unimportant in explaining farm profits.

A corollary of the paradox is that profits no longer are a good signalling device. Assume that an industry is initially in long-run equilibrium. Since the analysis of higher factor prices was a short-run analysis, the number of firms and quantity of capital in an industry were assumed constant. Larger absolute profits, therefore, mean a higher rate of return, which would induce entrants. However, assuming the relative increase in factor prices is permanent, the long-run supply curve shifts upward, and equilibrium output of the industry falls. If firms' isoquants are homothetic with respect to their origins, a higher price for a factor does not change the optimum firm size. Higher factor prices increase long-run average costs but do not change the output at which average costs of a firm are minimized. Consequently, the reduction in industry output must be accommodated in the long run by the exit of firms from the industry. When the paradox holds, entry is encouraged though the number of firms in an industry is already larger than the industry can support in the long run.

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Friedman-Meiselman and Autonomous Expenditures

Milton Friedman and David Meiselman in a now famous paper [2] have sought to demonstrate the "superiority" of the quantity theory over the autonomous expenditures theory by testing simplified versions of these theories with U.S. data over the period 1897-1958. Their study has attracted widespread criticisms and comments centered in particular on the simplicity of the models used, the periods covered in the tests, and their definition of autonomous expenditures [1][3]. This note does not seek to

question the meaning of the results obtained by Friedman and Meiselman (FM), nor is it an attempt to evaluate the criticisms raised by participants in the debate. Rather, it has the limited objective of demonstrating that FM have not consistently applied their own criteria when selecting the variables of the autonomous expenditures model. It is also shown that a correct application of their criteria for selection provides support for the definitions they used.

FM's version of the autonomous expenditures theory is essentially an expansion of Keynes's fundamental equations. They define an income concept, Y , equal to the sum of all expenditures induced by income, U , and autonomous expenditures of all kinds, A^* . Autonomous expenditures are defined in the usual manner as being independent of current income flows while induced expenditures are a function of Y , giving the following model:

$$Y \equiv U + A^*$$

$$U = f(Y)$$

and also

$$U = g(A^*).$$

This leaves FM with the empirical specification of the variables U and A^* , for in this form the model can accommodate different empirical judgments. They attempt to provide an objective foundation for the selection of autonomous and induced expenditures by performing tests designed to determine the substitutability of individual items with an initial tentative grouping of autonomous and induced items.

The rationale of these tests together with an example of their application in a specific case has been outlined by FM elsewhere [1] [2] and need not be reiterated here. The following is a general formulation of their criteria. Total income, Y , is divided into three parts: (1) a doubtful item, D , which it is desired to classify as an autonomous or induced item; (2) autonomous expenditures, A , which *exclude* the doubtful item; and (3) induced expenditures, N , which also *exclude* D . If, as a result of the tests, D is classified as a component of autonomous expenditures, autonomous expenditures will be $D+A$ for the purposes of the model. Hence the question is whether $D+A$ or A alone is the preferable definition of autonomous expenditures. FM argue that if D and A were perfect substitutes as income-generating expenditures, then N would tend to have a higher correlation with $D+A$ than with either D or A alone. This is the first test. As the sum of autonomous expenditures, induced expenditures, and the item in doubt equals income, the test can be performed in reverse to determine whether or not D is a perfect substitute for N .¹ This is the second test, and so the two tests are:

¹ The additive property of N , D , and A is such to permit the second test to be carried out. This possibility did not arise for the tests performed by FM to determine the definition of money. (For a description of these tests see [2, pp. 242-46].) It is for this reason that the tests for autonomous expenditures and for money differ in execution, even though the idea underlying them is the same.

(i) Autonomous test

(ii) Induced test

$${}^rN(D + A) > \begin{bmatrix} {}^rND \\ \text{and} \\ {}^rNA \end{bmatrix} \qquad {}^rA(D + N) > \begin{bmatrix} {}^rAD \\ \text{and} \\ {}^rAN \end{bmatrix}$$

FM tentatively define autonomous expenditures to comprise net private domestic investment, the government deficit on income and product account, and the net foreign balance. Consumers' expenditure on durable and nondurable goods is tentatively defined as induced expenditures. When making the tests, FM separate out various items in turn for treatment as the doubtful item. The items considered by FM in the tests are consumer durables, imports, exports, the net foreign balance, taxes, and the government deficit. The results of these calculations, although in many cases ambiguous, provide the basis for the choice of autonomous and induced expenditures.

As mentioned previously, FM have been criticized on the grounds that their definition of autonomous expenditures misrepresents the autonomous expenditures model. We cannot be sure that FM's criteria identify autonomous expenditures. At best, the tests they conduct only show that D and A are substitutes as income-generating expenditures, not that these items are independent of current income flows.² FM concede that the concept of autonomous expenditures they selected may have been unfavorable to the autonomous expenditures theory, but contend that it had the merit of being "... derived by a specifiable and reproducible procedure ..." [1, p. 784]. FM's defense is unnecessarily weakened by an inconsistent application of their own criteria. In particular, they (1) do not always perform *both* relevant tests, (2) fail to use a consistent concept of income and autonomous expenditures, and (3) make tests which do not follow from their criteria.

Both tests are required by FM to classify an item as an autonomous or induced expenditure. The doubtful item, D , would be included as a component of autonomous expenditures if test (i) were satisfied, and test (ii) not satisfied. It would be considered induced if these results were reversed. If neither of these combinations holds, then the results must be considered ambiguous. Despite their own requirement that both tests be carried out, FM do not perform test (ii) in four cases. This omission will be corrected in the tests considered below.

The second criticism stems from FM's uncertainty whether Y should be defined as personal disposable income or personal disposable income plus the undistributed income of corporations and the excess of wage accruals over wage payments. These latter items were deducted from the tentative grouping of autonomous expenditures in some of their tests. This change of definition generates confusion, and in at least one instance (discussed below) has a bearing on the results of the tests.

² In order to avoid the least-squares bias which may result from including induced items in A^* , Ando and Modigliani [1, p. 787] have specified that an autonomous variable "... should be uncorrelated with the error term of the test equation"; although FM consider this to be "... figurative rather than operational" [1, p. 784]. Further discussion of this problem is beyond the scope of this paper.

The last criticism can be illustrated by considering FM's tests for the treatment of imports, exports, and the net foreign balance. FM use the following symbols in these tests:

- C = personal consumption expenditures,
 D_0 = net private domestic investment plus the government deficit on income and product account,
 E = exports,
 M = imports,
 $F = E - M$ = net foreign balance.

FM first consider imports, assuming in terms of their tentative definition of autonomous expenditures that exports are autonomous. According to our interpretation of FM's criteria for allocating items between the categories U and A^* , we have the following classification:

$D = M$ (doubtful item)

$N = C$ (induced expenditures, excluding doubtful item)

$A = D_0 + E$ (autonomous expenditures, excluding doubtful item)

Hence the two tests should be:

(i) Autonomous test

(ii) Induced test

$${}^rC(D_0 + F) > \begin{cases} {}^rC \cdot M \\ \text{and} \\ {}^rC(D_0 + E) \end{cases} \quad {}^r(C - M)(D_0 + E) > \begin{cases} {}^rM(D_0 + E) \\ \text{and} \\ {}^rC(D_0 + E) \end{cases}$$

The present writer performed these tests and the results are set out in Table 1. The time periods used conform to those used by FM, and the data were derived from the source given in their Appendix Table II-B2. One of the time periods selected by FM, 1940-1952, includes war years, and may give unreliable results. An extra column has been included in Table 1 to give results for the period 1929-1952, excluding the war years (1942-1946). It can be seen that the exclusion of the war years from the period dramatically improves the correlations; but it gives the same results for the tests. Calculations similar to those in Table 1 were performed including a more recent period (1953-1964) and using revised data published in *Survey of Current Business* (August 1965), but the results do not differ substantially and are not reported here. In addition, a comparison between the results shown in Table 1 and those obtained by FM is given in Table 2.

FM carry out test (i), and the inequality is not satisfied. On the basis of this result they suggest that M should be regarded as induced. This test was performed incorrectly: for induced expenditures excluding the doubtful item, they used $C - M$ rather than C .³ The amended calculations are not

³ The use of $C - M$ rather than C for induced expenditures excluding M could be defended on the grounds that induced expenditures, C , already include a part of M , namely imported consumer goods. Hence $C - M$ may be a better approximation to induced expenditures than C when M is being considered for inclusion as an autonomous item. However, the tests adopted by FM assume in effect that the item being tested is either entirely autonomous or entirely induced, and they do not permit the classification of the doubtful item as partly autonomous and

TABLE 1—EXPERIMENTS WITH ALTERNATIVE TREATMENTS OF THE FOREIGN BALANCE^a

| | 1929-1939 Annual | 1940-1952 Annual | 1929-1952 Annual | 1946-1958 Quarterly | 1929-1952 Excluding 1942-1946 Annual |
|--|---------------------|---------------------|---------------------|------------------------|---|
| 1. Tests for Imports (<i>E</i> assumed autonomous) | | | | | |
| <i>C</i> and <i>D</i> ₀ + <i>F</i> | .8770 | .0067 | .4492 | .4567 | .9229 |
| <i>C</i> and <i>M</i> | .8662 | .9071 | .9454 | .9641 | .9567 |
| <i>C</i> and <i>D</i> ₀ + <i>E</i> | .9193 | .1361 | .5989 | .7389 | .9461 |
| <i>M</i> and <i>D</i> ₀ + <i>E</i> | .8506 | .2972 | .6593 | .8041 | .9631 |
| <i>C-M</i> and <i>D</i> ₀ + <i>E</i> | .9094 | .1246 | .5915 | .7286 | .9413 |
| 2. Tests for Exports (<i>M</i> assumed induced) | | | | | |
| <i>C-M</i> and <i>D</i> ₀ + <i>E</i> | .9094 | .1246 | .5915 | .7286 | .9413 |
| <i>C-M</i> and <i>E</i> | .9027 | .8619 | .9139 | .7061 | .9436 |
| <i>C-M</i> and <i>D</i> ₀ | .8250 | (-) .0024 | .3281 | .4533 | .8414 |
| <i>E</i> and <i>D</i> ₀ | .6831 | (-) .1092 | .1454 | .1507 | .7198 |
| <i>C+F</i> and <i>D</i> ₀ | .8145 | (-) .0061 | .3106 | .4307 | .8341 |
| 3. Tests for Foreign Balance | | | | | |
| <i>C</i> and <i>D</i> ₀ + <i>F</i> | .8770 | .0067 | .4492 | .4567 | .9229 |
| <i>C</i> and <i>F</i> | .2604 | .1693 | .2243 | (-) .1607 | .2987 |
| <i>C</i> and <i>D</i> ₀ | .8306 | (-) .0010 | .3351 | .4642 | .8488 |
| <i>F</i> and <i>D</i> ₀ | .6739 | (-) .6170 | (-) .0578 | (-) .4590 | .0768 |
| <i>C+F</i> and <i>D</i> ₀ | .8145 | (-) .0061 | .3106 | .4307 | .8341 |

^a Each entry is the square of the correlation coefficient between variables indicated by the relevant row and for the period specified in the column.

Source: Data correspond to those given in Appendix Table II-B2 to the FM study 2, pp. 262-63].

substantially different. But FM did not apply test (ii), in order to confirm that *M* is induced. From the figures in Table 1, it can be seen that none of the correlations supports the conclusion that *M* is an induced item.

FM proceed to consider exports, on the assumption that *M* is induced. The autonomous test was undertaken and FM reject the hypothesis that *E* is autonomous, concluding that *E* is "mixed." This test was logically correct, but because of the definition of income adopted (see explanation above) their results differ from those reported here. The results given in Table 1 are more favorable than FM's towards considering *E* an autonomous item: correlations for two periods satisfy test (i), and the induced test is not satisfied.

The relatively low correlations between *E* and *D*₀ in test (ii) are not inconsistent with *E* being an autonomous item. Using the general formulation given earlier, *A* should be more highly correlated with (*D*+*N*) than with either *D* alone or *N* alone if *D* and *N* are induced. However, this inequality

partly induced [2, p. 185 n. 2]. Furthermore, if FM had this in mind when using *C-M* rather than *C*, they should have used *C* rather than *C-M* for induced expenditures including *M* when performing the tests for exports (in which *M* is assumed to be induced).

TABLE 2—COMPARISON OF ORIGINAL AND AMENDED EXPERIMENTS^a

| Test | Inequality Satisfied | Inequality Not Satisfied ^b | |
|------------------------------|----------------------|---------------------------------------|---------------|
| | | Inequality Reversed | Hybrid Result |
| 1. Imports | | | |
| FM's tests (i) Autonomous | — | 3 | 1 |
| (ii) Induced | — | — | — |
| Amended tests (i) Autonomous | — | 3 | 1 |
| (ii) Induced | — | 3 | 1 |
| 2. Exports | | | |
| FM's tests (i) Autonomous | — | 1 | 2 |
| (ii) Induced | — | — | — |
| Amended tests (i) Autonomous | 2 | — | 2 |
| (ii) Induced | — | — | 4 |
| 3. Foreign Balance | | | |
| FM's tests (i) Autonomous | 3 | — | — |
| (ii) Induced | — | — | — |
| Amended tests (i) Autonomous | 2 | — | 2 |
| (ii) Induced | — | 1 | 3 |

^a Correlations for the period 1929–1952, excluding the war years, have not been included in the classification, so as to permit a comparison with FM's results.

^b The correlations for a period satisfy a test when, using the induced test as an example:

$$rA(D + N) > rAD \text{ and } rAN$$

A test is classified as being "not satisfied" when the inequality is reversed or where the correlation on the LHS exceeds one of those on the RHS but is less than the other (the latter result is classified as hybrid). Following FM, the sign of the correlation coefficient has been ignored when making the classification.

Sources: Table 1 and Appendix Table II-A3 to FM [2, p. 252].

may not be reversed even if D is autonomous. The correlation between A and D , the autonomous items, may well be the smallest of the three. This possibility has led to the classification adopted in Table 2.

Finally, FM conduct tests for F , the net foreign balance. In terms of the terminology adopted here, we have $D = F$, $N = C$, and $A = D_0$, so that the two tests should be:

$$\begin{array}{ll}
 \text{(i) Autonomous test} & \text{(ii) Induced test} \\
 rC(D_0 + F) > \begin{bmatrix} rC \cdot D_0 \\ \text{and} \\ rC \cdot F \end{bmatrix} & rD_0(C + F) > \begin{bmatrix} rC \cdot D_0 \\ \text{and} \\ rF \cdot D_0 \end{bmatrix}
 \end{array}$$

FM perform test (i), and decide that it is consistent with the foreign balance

being autonomous. Again they use $C-M$ rather than C for induced expenditures, which appears to be incorrect. The amended calculations given here support their conclusion. FM could have strengthened their case by performing test (ii). The results of this test are shown in Table 1 and they provide no evidence to contradict their conclusion. The low correlations between F and D_0 may be consistent with F being autonomous. FM appear to argue that their classification of F as an autonomous item is consistent with their results for exports and imports: for exports, classified a mixed item, can be considered as the sum of imports (induced) and the foreign balance (autonomous). However, the conclusion that the foreign balance is autonomous may also be consistent with exports being autonomous and imports a mixed item, the interpretations suggested by the calculations shown in Table 1.

As a further illustration of FM's failure to apply their own criteria correctly, their tests for taxes may be considered. In terms of the interpretation adopted here, $N=C$, $D=T''$ and $A=A^*+T''$, so that the tests for taxes (FM's T'' concept) should be:

| | |
|--|--|
| <p>(i) Autonomous test</p> $r_{C \cdot A^*} > \begin{cases} r_{C \cdot T''} \\ \text{and} \\ r_{C(A^* + T'')} \end{cases}$ | <p>(ii) Induced test</p> $r_{(C - T'')(A^* + T'')} > \begin{cases} r_{(A^* + T'')T''} \\ \text{and} \\ r_{C(A^* + T'')} \end{cases}$ |
|--|--|

These tests were carried out with annual data for the periods 1929-1945, 1946-1964, and 1929-1964 but the results are ambiguous, as were those obtained by FM.⁴

On the results of the tests with imports, exports, and the net foreign balance, FM decide to treat the net foreign balance as an autonomous item in the autonomous expenditures model used in the comparison with the quantity theory. Some critics have been surprised by FM's willingness to consider $(E-M)$ as a single item. The question of the appropriate multiplier for income determination is one of historical conjecture [4]. Samuelson [5, p. 149] has argued that "the cases in which the trade balance can be treated as an autonomous element are necessarily those in which—through the action of exchange control, exchange depreciation, or lender-borrower psychology—capital movements prove to be the bottleneck to which trade movements adjust themselves." In this context, it should be noted that

⁴ Correlations for 1929-1945 satisfied the autonomous test but the inequality was reversed in the other periods, and the induced test was not satisfied. FM performed the following tests with annual and quarterly data for the period 1946-1958 to determine the appropriate treatment of taxes, T'' (for the definition of T'' and a description of these tests see [2, pp. 253-56]).

- (i) Autonomous test
 $r_{(C-T'')(A^*+T'')} > r_{(C-T'')T''}$ and $r_{(C-T'')A^*}$
- (ii) Induced test
 $r_{C \cdot A^*} > r_{A^* \cdot T''}$ and $r_{(C-T'')A^*}$

The results of these tests were ambiguous, and FM decided to retain their tentative classification of the government deficit as an autonomous item.

correlations for two periods between the years 1929–1952 favor treating the foreign balance as an autonomous item.

This rationalization of the results obtained for the net foreign balance raises the question as to whether the same treatment of the net foreign balance is justified for the period not covered by these tests. Much of the impact of FM's study stemmed from the "superiority" of the quantity theory over the autonomous expenditures theory during the years 1897–1928.

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International Short-Term Capital Movements: Comments

COMMENT I

The sensitivity of short-term international capital movements to the interest rate differentials between the leading financial centers is becoming increasingly important to policymakers as attempts are made to finance basic deficits in the balance of payments by retaining domestic and attracting foreign, private short-term capital. For this reason the possibility of upward bias in the estimates of sensitivity of short-term funds to the interest rate differential derived by Jerome L. Stein [9] deserves mention. Two aspects of Stein's article are of concern: (1) the distinction between and the test of the applicability of the stock and the flow theories, and the consequent rejection of the stock theory, and (2) the estimates of the sensitivity of capital movements to the rate differential.

1. The susceptibility to the rate differential of the stock of assets and liabilities or of the absolute rate of change of the stocks—the flow model—is important because, “if the interest differential determines a flow, a constant basic deficit is consistent with a constant interest differential” [9, p. 40], whereas if the “differential determines a stock . . . a constant or declining basic deficit requires a growing interest differential” [9, p. 40].

The estimating equation for the stock model is:

$$(15)^1 \quad S(t) = b_0 + b_1 S(t-1) + b_2 R(t) + b_3 x_3(t)$$

and for the flow model is:

$$(22) \quad y(t) = d_0 + d_1 R(t) + d_3 x_3(t)$$

where $S(t)$ is the stock of liabilities to foreigners or of U.S. assets in period t —the two concepts being analyzed separately; R is a speculative variable which does not concern us in this note; x_3 is the uncovered interest differential between London and New York, and $y(t)$ is equal to $S(t)$ minus $S(t-1)$.

Given these postulated relationships, the difference between the stock model and the flow model depends upon the value of b_1 . Either b_1 is significantly different from unity or it is not. If not, the two equations are identical and the stock and the flow theories merge into one. If b_1 is different from unity, the stock equation is a variant of a standard stock adjustment model in which the change in the stock in a period—the flow—is some proportion of the difference between the desired stock and the actual stock at the end of the preceding period: the desired stock being determined by $R(t)$ and $x_3(t)$. In the latter case, if $R(t)$ and $x_3(t)$ stay constant, the change in the stock will monotonically approach zero with the passage of time.² No set of observations yielded a value of b_1 significantly different from unity.

To compare the relative merits of the stock and flow hypotheses, the stock model is recast in terms of flows by taking a first difference of equation (15):

$$(38) \quad \Delta S = K_1 \Delta R - K_3 \Delta I \quad (\text{where } \Delta I = -x_3).$$

The test for the validity of the stock and flow theories is a multiple regression of the transfers of short-term capital in period t on the four independent variables, I , ΔI , R , ΔR . From equation (38) the stock theory is alleged to be the better theory if the first differences prove to be the significant explanatory variables, and from equation (22) the flow theory will be the more acceptable if the levels variables are statistically significant. Equation (38) does not include the first difference of the variable $S(t-1)$ on the grounds that the lagged flow was not a statistically significant explanatory variable of the flow in equations (33) or (37). This lack of significance of the lagged flow emphasizes the difficulties inherent in any attempt to establish the validity of either theory on purely empirical grounds. A linear relationship

¹ The numbers of the equations are those of the original article.

² From (15) and assuming $b_0 > 0$ and $b_1 < 1$,

$$(15A) \quad S(t) - S(t-1) = y(t) = b_0 + (b_1 - 1)S(t-1) + b_2 R(t) + b_3 x_3(t).$$

of $\Delta S(t)$ to $\Delta S(t-1)$ is not a necessary feature of a stock adjustment model when the desired stock is itself changing with each observation, but the lack of applicability of a linear relationship in the lagged flow variable must impugn the use of first difference variables as a satisfactory test for the stock theory—particularly so when the original formulation of both models was identical.³ The consequent rejection of the stock theory on the basis of the statistical insignificance of the two first difference variables in equations (39) and (41) does not constitute adequate proof of the superiority of the flow theory [9, pp. 63–64]. In fact the relationship between the flow or change in the stock and the interest differential is complex and the volume of capital movements may well be a function of both the level and the first difference. A larger differential may not attract a proportionate increase in capital, and the sensitivity to a differential may depend in turn on the levels of the interest rates if the average yield of the portfolio affects its distribution.

The rejection of the stock theory can also be questioned on theoretical grounds. A given interest rate differential offers an infinitely elastic demand curve for individual transfers of international capital. The corresponding supply curve is upward sloping denoting increased transfers to be potentially generated by larger differentials. If the flow theory is held, the supply curve is defined as a flow of capital per period; if the stock theory is held, the supply curve describes the reaction of a given stock of potentially internationally mobile capital which is extant at any one time: a change in the differential will induce a redistribution of the stock and, once the redistribution of the stock is finished, any further international transfers will comprise the international allocation of accretions to the global stock of international short-term capital. The flow theory contends that a given differential will generate capital transfers at a constant rate for an undefined length of time. The lack of any integral part of the theory which explains the termination of the process is an important weakness in the theory since, in its absence, the theory must allow for the existence of potentially infinite stocks of money. Additionally, the flow theory fails to explain why owners of internationally mobile capital should so regulate their total activities as to generate an approximately constant rate of transfers rather than responding quickly to the advantage inherent in the change in the differential and then letting the rate of transfers decrease—the latter behavior pattern being in accord with the stock adjustment model given in equation (15A).⁴

The rejection of the stock theory in favor of the flow theory is not warranted by the evidence.⁵

2. The other item of concern is the possibility that the estimates of the sensitivity of the flows of short-term capital are subject to an upward bias. Assuming no speculative influence, parity in the Treasury bill rates in London and New York is estimated to yield an inflow of foreign private

³ Given $b_1=1$.

⁴ See footnote 2 above.

⁵ This is particularly true, for example, in equation (39) where the levels variable, I , is not significantly different from zero and in fact, has a " t " value of less than that of ΔI .

capital into New York of \$850 million per annum⁶ and an outflow of U.S. capital of \$159 million per annum. The institution of a one per cent differential in favor of London yields an estimated inflow of \$665 million and an outflow of \$436 million per annum, and a reduction in the net inflow of \$462 million per annum. The possible upward bias has two sources. The first source is the multiplication by twelve of the monthly rate of flow in order to compute an annual rate of flow. This procedure is only consistent with the adoption of the flow theory and the constant rate of flow of capital in response to a given differential.

Secondly, the sensitivity of capital flows to an interest differential may be exaggerated if the differential changes its sign during the period. A change in sign would result in the same capital being counted as it moves in both directions as funds, originally attracted away from Europe by an increase in U.S. short-term interest rates, return to Europe as the U.S. rate declines relatively and are then switched back to the United States once more as the old differential is reestablished. In the same way, the estimates of response to an increase in the differential do not consider the problem of maintaining the new differential over a twelve-month period.

The possibility of the estimates of the sensitivity of short-term capital movements to the interest differential being biased upward hinges on the acceptance of a stock adjustment hypothesis according to which a change in the differential will result in a large proportion of the total flow being generated in the first period. The argument in this note is that the refutation of the stock adjustment theory is insufficient for the possibility of an upward bias in the estimates to be eliminated.

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⁶ The interest variable is not significant for the computation of flows of private foreign capital but was used in measuring the induced flows in the summary estimate [9, p. 65].

COMMENT II

In a recent article in this *Review* [9], Stein reviews attempts by Bell [2] and Kenen [7] to use multiple regression techniques to estimate international short-term capital movements resulting from interest rate differentials. He correctly points out that certain of Bell's and Kenen's parameter estimates are biased and inconsistent. Stein then goes on to estimate the sensitivity of capital flows¹ to uncovered interest rate differentials and to a proxy variable which, he claims, reflects the effects of expectations about the future foreign-exchange rate. I suggest that under a more reasonable specification of Stein's system of equations, his method also yields biased and inconsistent estimates. I further question the interpretation of the proxy that Stein uses and suggest that, even if he could obtain a consistent estimate of the parameter of the proxy variable, he is unjustified in interpreting it as an estimate of the effects of speculation on capital movements.

¹ In fact, all equations below deal with stocks of assets rather than flows. As Stein points out, the analysis is basically the same whether one prefers to deal with stocks or with flows.

I. *The Stein Model*

Stein [9, p. 48] posits a supply of dollars $S(t)$ at time t :

$$(E.1) \quad S(t) = S(t-1) + T - c_1p - w.$$

That is, the supply of dollars in period t is equal to the supply of dollars at the end of the previous period ($S(t-1)$) plus dollar sales by foreign governments (T) minus the basic balance surplus of the United States, which is a function of the exchange rate (p) and a disturbance term (w). (E.1) and a demand equation for spot dollars and demand and supply equations for forward dollars yield the following reduced-form equations [9, p. 51]:

$$(E.2) \quad x_2 = A_1p^* - A_2[S(t-1) + T - w] + A_3x_3$$

$$(E.3) \quad p = B_1p^* + B_2[S(t-1) + T - w] - B_3x_3$$

where x_2 is the forward rate, p^* is the expected price of foreign exchange prevailing when the investor wants to repatriate his funds, and x_3 is the uncovered interest rate differential.

Stein then substitutes (E.3) into (E.1) and gets [9, pp. 52-53]:

$$(E.4) \quad S(t) = K_0 + K_3x_3 - K_1p^* + K_2S(t-1) + K_2(T - w).$$

But p^* (the expected price of foreign exchange when the speculator wants to repatriate his funds) is unobservable. In an attempt to solve this practical problem he goes to (E.2), regresses x_2 on x_3 , and derives the residual R . This residual, the difference between the actual and the predicted value of x_2 , will approach the value of $A_1p^* - A_2S(t-1) - A_2(T - w)$ as the sample size for the regression approaches infinity.² Next, Stein develops (E.5) from (E.4) and then estimates (E.7) by ordinary least squares:

$$(E.5) \quad S(t) = K_0 + K_3x_3 + K_2S(t-1) \\ + K_4R - K_4R - K_1p^* + K_2(T - w)$$

or

$$(E.6) \quad S(t) = K_0 + K_3x_3 + K_2S(t-1) \\ + K_4R - K_4A_1p^* + K_4A_2S(t-1) \\ + K_4A_2(T - w) - K_1p^* + K_2(T - w)$$

or

$$(E.7) \quad S(t) = K_0 + K_3x_3 + (K_4A_2 + K_2)S(t-1) \\ + K_4R + r$$

where

$$(E.8) \quad r = -(K_4A_1 + K_1)p^* + (K_4A_2 + K_2)(T - w).$$

² All work below will assume that R is based on an infinitely large sample.

Can one obtain a consistent estimate of K_4 in (E.7) by ordinary least squares? The answer depends on whether r is correlated with $R = A_1 p^* - A_2 S(t-1) - A_2(T-w)$. r will be uncorrelated with R if and only if both $(K_4 A_1 + K_1)$ and $(K_4 A_2 + K_2)$ are equal to zero, i.e., r is equal to zero.³ It turns out that there exists one value for K_4 ,

$$K_4 = - \frac{a_1(g_2 + h) + g_1 h}{g_1},$$

which satisfies $K_4 A_1 + K_1 = K_4 A_2 + K_2 = 0$.⁴ Even though it still has not been demonstrated that the coefficient of R in (E.7) necessarily takes on that specific value, let us assume that it does indeed do so.⁵

A moment's thought about (E.7), however, should make us very uneasy. If r is equal to zero, (E.7) is an *exact* equation. Why should relatively poor econometric fits of (E.7) result if (E.7) is asymptotically an exact equation? There are two possible alternatives: (1) the poor fit of (E.7) results from a poor estimate of A_3 in the regression of x_2 on x_3 ; or, (2) the model is misspecified and, therefore, empirical results do not reflect postulated theory.

II. An Alternative Model

Under a slightly different specification of the model it is easy to demonstrate that ordinary least squares will yield inconsistent estimates of the parameters of (E.7) unless one is willing to violate the restrictions on the parameters of the structural equations which Stein imposes. Let us attach error term u to Stein's equation (A3) [9, p. 49] to yield

$$(A3') \quad D(i) = h(x_3 - x_2) + a_1(p - p^*) + a_3 x_3 + u$$

and also attach an error term v to equation (9) [9, p. 50]:

$$(9') \quad g_1 p + (g_2 + h)x_2 = hx_3 + g_1 p^* + v.$$

³ This assumes that the covariance between p^* and $T-w$ is zero, a valid assumption if p^* and T are treated as predetermined variables.

⁴ $K_4 A_1 + K_1 = K_4 A_2 + K_2 = 0$ implies that

$$K_4 = - \frac{K_1}{A_1} = - \frac{K_2}{A_2}.$$

Since Stein [9, pp. 51-52, footnotes 11 and 12] presents the values of the parameters of the reduced form equations in terms of the parameters of the structural equations, K_4 can be expressed in terms of the structural parameters a_1, a_3, g_1, g_2 , and h :

$$K_4 = - \frac{K_1}{A_1} = - \frac{a_1(g_2 + h) + g_1 h}{g_1}$$

$$K_4 = - \frac{K_2}{A_2} = - \frac{a_1(g_2 + h) + g_1 h}{g_1}.$$

For an interpretation of these structural parameters, the reader should consult [9, pp. 48-51].

⁵ I have been unable to develop a rigorous proof of this although there are intuitive reasons to suspect that it is so. In any case, it is a moot question since the discussion below suggests that under a more reasonable specification of the system, Stein's method certainly yields inconsistent estimates.

Surely, Stein did not mean to assume an exact demand function for spot exchange or an exact function to describe net demand for forward exchange. Apparently, he assumed that attaching w to his equation (A1) [9, p. 48] would reflect all these disturbances without changing the nature of the model. That, however, is not true.

Using (A3') and (9') along with (E.1), we derive a new set of reduced form equations, (E.2') and (E.3'):

$$(E.2') \quad x_2 = A_1 p^* - A_2 [S(t-1) + T - w - u] + A_3 x_3 + A_4 v$$

$$(E.3') \quad p = B_1 p^* + B_2 [S(t-1) + T - w - u] - B_3 x_3 + B_4 v.^6$$

Substituting (E.3') into (E.1) yields:

$$(E.4') \quad S(t) = K_0 + K_3 x_3 - K_1 p^* + K_2 S(t-1) \\ + K_2 (T - w) + K_5 u + K_6 v.^7$$

Now, from (E.2'), regress x_2 on x_3 and calculate the residual, $R' = A_1 p^* - A_2 [S(t-1) + T - w - u] + A_4 v$.

Develop (E.5'):

$$(E.5') \quad S(t) = K_0 + K_3 x_3 + K_2 S(t-1) + K_4' R' \\ - K_4' R' - K_1 p^* + K_2 (T - w) + K_5 u + K_6 v$$

or

$$(E.6') \quad S(t) = K_0 + K_3 x_3 + K_2 S(t-1) + K_4' R' \\ - K_4' A_1 p^* + K_4' A_2 S(t-1) \\ + K_4' A_2 (T - w) - K_4' A_2 u - K_4' A_4 v \\ - K_1 p^* + K_2 (T - w) + K_5 u + K_6 v$$

or

$$(E.7') \quad S(t) = K_0 + K_3 x_3 + (K_2 + K_4' A_2) S(t-1) + K_4' R' + r'$$

where

$$(E.8') \quad r' = - (K_1 + K_4' A_1) p^* + (K_2 + K_4' A_2) (T - w) \\ + (K_5 - K_4' A_2) u + (K_6 - K_4' A_4) v.$$

R' will not be correlated with r' if and only if r' is equal to zero, i.e., if and only if $K_1 + K_4' A_1 = K_2 + K_4' A_2 = K_5 - K_4' A_2 = K_6 - K_4' A_4 = 0$. Therefore, K_4' must satisfy each of the following conditions:

$$^6 A_4 = \frac{a_1 + c_1}{J}; \quad B_4 = \frac{h}{J} \\ ^7 K_5 = 1 - K_2 = c_1 B_2; \quad K_6 = -c_1 B_4$$

$$K'_4 = -\frac{K_1}{A_1} = -\frac{a_1(g_2 + h) + g_1h}{g_1} < 0$$

$$K'_4 = -\frac{K_2}{A_2} = -\frac{a_1(g_2 + h) + g_1h}{g_1} < 0$$

$$K'_4 = \frac{K_5}{A_2} = \frac{c_1B_2}{A_2} = \frac{c_1(g_2 + h)}{g_1} > 0$$

$$K'_4 = \frac{K_6}{A_4} = -\frac{c_1B_4}{A_4} = -\frac{c_1h}{a_1 + c_1} < 0.$$

Checking signs of variables indicates that, in order for r' to be zero, at least one of the structural parameters must have a sign opposite to that postulated by Stein.⁸ Consequently, when Stein's model is modified to include error terms on (A3) and (9), his technique results in inconsistent estimates.

III. On The Interpretation of R

The discussion up to this point has been concerned with the problem of obtaining consistent estimates of the effects of R on capital movements. The logically prior question of the usefulness of such an estimate has been ignored. My conclusion is that, even if consistent estimates of K_4 were available, they would be of little value because R cannot be interpreted as representing the effects of any one variable.

Apparently, Stein is interested in the effects of R on capital movements because R is an approximation to the covered interest rate differential.⁹ Exchange market theorists have traditionally assumed that the size of the covered interest rate differential reflected the degree of speculation in foreign exchange markets. Stein, therefore, is interested in R as a proxy for speculation.¹⁰ His model, however, suggests that $R = A_1p^* - A_2[S(t-1) + T - w]$ reveals nothing about speculation until we know something about A_1 , A_2 , T , $S(t-1)$, and w . Unless we are willing to assume that A_1p^* is "large" and $A_2[S(t-1) + T - w]$ is "small," the assumption that movements in R reflect movements in p^* could be highly misleading. After a search of the foreign exchange market literature, I am left with the uneasy feeling that there is little "hard" a priori evidence on this subject; many people make this assumption mainly because it has been a traditional assumption.

It is not difficult to produce a plausible example in which the covered interest rate differential responds to something besides expectations about the future foreign exchange rate. Suppose that exports rise sharply and that

⁸ He assumes that g_1 , c_1 , h , a_1 , a_3 , and g_2 all are greater than zero [9, pp. 48-50]. I need not belabor the point that in order for all of the above restrictions on K'_4 to be satisfied, other complicated restrictions must be placed on the structural parameters. These will not be discussed.

⁹ If the constant term were zero and the coefficient of I were one in equation (28) in [9, p. 58], R would exactly equal the covered interest rate differential defined by Stein [9, p. 43].

¹⁰ Stein [9, pp. 41 and 64] explicitly interprets R as reflecting speculative pressures, as indeed he must unless he clings to nonoperational statements about R .

the buyers of exports finance their purchases with short-term credit. If those buyers always hedge, they will cover in the forward market. If the increase in exports is large enough, pressure on the forward market will result in a nonzero covered interest rate differential. R has moved and yet no speculative pressures necessarily exist! Anyone who uses R as a proxy for A_1p^* must assume that it is unlikely that a rise in exports will have a significant effect on R . It is certainly not evident to me why such an assumption is empirically reasonable.

In the absence of any persuasive arguments about the relative importance of variables determining the covered interest rate differential, I must conclude that the use of R as an explanation of capital movements is non-operational, i.e., it does not necessarily imply anything about the effects of any particular variable, like p^* .

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COMMENT III

During the past few years several attempts have been made to measure the interest sensitivity of short-term capital movements. In his recent article Stein [9] calls attention to many errors in earlier research on this subject and undertakes to correct them. But serious conceptual and empirical problems remain even in his work. Stein's basic two-equation regression model is of the following form:

$$Y(t) = \alpha_0 + \alpha_1 R(t) + \alpha_2 I(t) + u(t)$$

and

$$X(t) = \beta_0 + \beta_1 R(t) + \beta_2 I(t) + v(t).$$

Where, $Y(t)$ ≡ the change in the stock of U.S. short-term banking claims on foreigners from the end of month $t-1$ to the end of month t .

$X(t)$ ≡ the change in the stock of short-term banking liabilities to private foreigners, payable in dollars from the end of month $t-1$ to the end of month t .

$R(t)$ ≡ a speculation variable constructed by Stein.¹

$I(t)$ ≡ the U.K. 3-month Treasury bill rate minus the U.S. yield rate on new 3-month Treasury bills as of month t .

$u(t)$ and $v(t)$ are the stochastic disturbance terms in their respective equations. Implicitly, these regression equations assume that both R and I are exogenous variables. That is, from knowledge of α_1 and α_2 , one can predict the impact on Y of specified independent changes in R and/or I .

In this comment I shall ask in turn (1) whether R solely represents speculation and, given the way Stein has constructed R , whether it is meaningful to use I and R together as independent variables in a multiple regression

¹ For the precise calculation technique see Stein [9, pp. 57-59].

equation, (2) whether it is truly proper to regard R and I as exogenous variables in the context of an explanation of short-term capital flows, and (3) whether there are not logical variables, other than I and R , which will explain a larger portion of the variance in Y .

Speculation and Interest Sensitivity

The values for the speculation variable, R , constructed by Stein, are the residuals from a time series regression of the U.K.-U.S. interest differential, I , on the forward premium (—) or discount (+) of the British pound, X_2' .² Thus,

$$X_2'(t) = S_0 + S_1 I(t) + R(t).$$

R is simply an unusual forward premium or discount on the pound sterling, in the sense of not being accounted for by the interest rate differential. To contend that R represents only speculative pressure is incorrect: R may fluctuate in the absence of speculative pressure. Suppose that there is no fear of devaluation (of either the pound or the dollar), but that in a given month an exogenous increase occurs in the U.S. trade balance with Britain. If exporters are prone to hedge foreign accounts receivable, the demand for forward dollars by U.S. exporters will increase relative to the volume of forward dollars supplied by British exporters. The premium on the dollar will rise and will be registered as an increase in R . As R increases this should elicit a decline in the outflow of U.S. short-term capital because of the increased expense for U.S. capital-exporters in hedging their short-term pound assets. Thus we would expect to find Y and R negatively correlated, but not solely owing to speculative pressure. Similarly, exogenous changes in capital flows could give rise to a positive correlation between Y and R even in the absence of speculative pressure. Joint variation in Y and R has many sources, and is not uniquely related to speculative activity.

It is a requirement of least-squares regression that R and I be statistically independent. Stein himself points this out: "By the property of a least-squares regression, X_2 [which is the variable I] and the residual R are uncorrelated" [9, p. 55]. But, that being the case, must one not doubt the usefulness of a multiple regression in which R and I appear as the sole independent variables? Stein's interest sensitivity coefficient is not different from the one derived from the simple linear regression of capital movements on the interest rate differential below. Using Stein's basic model, least-squares regression on monthly data from November 1958 through October 1962 yields the following equation:

$$(1) \quad Y = 12.90 + 24.06I - 43.17R.^3$$

(11.50) (34.88)
 $r^2 = .06$

$D - W = 2.23$

² The notation used here is Stein's [9].

³ Because of some minor data differences, I have been unable to produce the precise results presented by Stein [9, pp. 62, 64]. However, the slight discrepancies do not affect the substance of the argument. The time period analyzed in this paper starts with November 1958 and ends with October 1962. The data are on a monthly basis. The interest rate data are from the *Federal*

Using I as the sole explanatory variable yields:

$$(2) \quad Y = 12.30 + 24.19I.$$

$$(11.57) \quad \bar{r}^2 = .05$$

$$D - W = 2.12$$

Stein's method, thus, has not corrected the capital flow interest differential relationship for speculative capital movements.

In Stein's criticism of the earlier works of Bell [2] and Kenen [7] he implies that he has indeed separated these effects and, thus, has come up with a pure interest differential coefficient. In his conclusion Stein states:

Bell found that the interest rate differential was not a significant variable. . . . One possible reason for our different results is the level of aggregation. . . . A more important reason is that he failed to take account of the speculative expectations which I took into account with my R variable [9, pp. 65, 66].

This "more important reason" by Stein's own analysis and description is invalid.

Are R and I Exogenous?

Stein recognizes [9, p. 43] that the forward premium on the pound is affected by capital flows and, hence, is endogenous. This in turn renders the covered interest rate differential an endogenous variable. He concludes that the covered interest rate differential cannot be used as an explanatory variable without bias. Yet Stein proceeds to use his index of speculative activity, R , as an additional explanatory variable. What is R ? The nominal label of speculation notwithstanding, R is nothing more than an unusual forward discount or premium on the pound. It is, indeed, an endogenous variable in the system. The same reasons used by Stein [9, pp. 43, 44] to show that the covered interest differential is not exogenous, apply *a fortiori* to Stein's R .

Stein's own writing is ambivalent as to whether I , the uncovered interest rate differential, can be treated as exogenous. He suggests a particular instance in which it most certainly was not exogenous, vis à vis capital flows. Specifically Stein states [9, p. 43], "As a result of the capital flow, the U.K. authorities raised interest rates. . . ." Since the data are generated by two-way interaction of interest differentials and capital flows, the estimates of the impact of changes in the interest rate differential provided by Stein's model are subject to a downward bias.

Interest Sensitivity in a More General Framework

In earlier studies of U.S. short-term capital movements, several authors have felt it appropriate to introduce variables representing trade [2][3]

Reserve Bulletin, the speculation data from Stein [9, p. 59] and the capital flow data from the *Treasury Bulletin*, Capital Movements. Section III, Table 2. The numbers in parentheses below the regression coefficients are the standard errors of their respective regression coefficients. \bar{r}^2 is the coefficient of determination corrected for degrees of freedom and $D-W$ is the Durbin Watson "d" statistic.

[5][7] and the domestic availability of short-term capital funds.⁴ Stein did not examine any such variables, presumably because he was principally concerned with the interest sensitivity of short-term capital flows. Yet, there is a major problem here in determining the net regression coefficient of any variable unless the specification of the model includes all the appropriate variables in the multiple regression. This is especially so in time series analysis, for time series tend to move in similar patterns, thus creating a high probability of multicollinearity.

It is, therefore, important to inquire what happens when Stein's basic regression equation of U.S. short-term capital movements is augmented by the inclusion of two other exogenous variables. These are: (1) the U.S. trade balance (T), and (2) monthly changes in the U.S. money supply (M).⁵ A large portion of U.S. short-term capital flows consists of trade financing. According to Bell [2, pp. 427, 428], "It would appear, then, that virtually all of the net outflow of U.S. short-term capital outflow before 1960, and 50-60 percent of the much larger outflow of 1960 and 1961 (and indeed the first half of 1962 as well) consisted of export finance." The following empirical tests take as the trade variable, the U.S. trade balance; on a priori grounds, the trade balance seems more appropriate than exports, given the net nature of the capital movements being analyzed here. The availability of short-term funds is measured as monthly changes in the U.S. money supply. One may think of banks as having a marginal propensity to invest in foreign short-term assets. The extremely high variance of this series precludes the possibility of banks lending these funds for longer periods of time. The results of Stein's basic model were given in equation (1). In all, only 6 per cent, after adjusting for the degrees of freedom, of the variance in U.S. short-term capital movements is explained—principally by I , which was significant at the 5 per cent level. R was not significant at even the 20 per cent level.

When T and M are introduced we obtain:

$$(3) \quad Y = -15.31 + 9.04I - 39.61R + 15.27M + .150T,$$

$$(14.36) \quad (33.62) \quad (6.23) \quad (.109) \quad \bar{r}^2 = .18$$

$$D - W = 2.19$$

and after dropping the variables I and R .

$$(4) \quad Y = -22.04 + 14.57M + .211T.$$

$$(6.17) \quad (.080) \quad \bar{r}^2 = .18$$

$$D - W = 2.10$$

Note that in regression 3, the coefficients of I and R are insignificantly different from zero. The monthly changes in the U.S. money supply, M , ap-

⁴ Kenen [7] considered this variable specifically as the amount of "U.S. net free reserves."

⁵ The data for these variables are from the *International Financial Statistics* of the International Monetary Fund.

pears as the explanatory variable to which Y is most strongly related: it is significant at the 2 per cent level. The trade balance coefficient is larger in relation to its standard error than are the coefficients of I and R , although it is not significant at confidence levels above 85 per cent. Equation 3, after adjustment is made for the degrees of freedom, explains 18 per cent of the variance of U.S. short-term capital movements, three times as much as the basic Stein model, and the interest differential variable no longer seems to be important.

Indeed, the separate test in equation 4, including only M and T , yields results that are as good as those in equation 3. Both variables are significant at the 2 per cent level and 18 per cent of the variance is explained, after making an allowance for the degrees of freedom. Therefore, on statistical grounds it would appear much more plausible to accept regression equation 4 instead of the basic model advanced by Stein.

Conclusions

In this comment I have attempted to make the following points which, in part or completely, invalidate Stein's results. (1) The variable R is not solely a speculation variable, but may fluctuate for reasons other than speculation. The use of R in Stein's basic model has no effect on the coefficient of the interest differential variable, I . (2) The variables R and I are endogenous variables and, therefore, their respective coefficients, as measured using Stein's basic model, are biased. (3) Other more truly exogenous variables explain more of the variance in U.S. short-term capital flows than do R and I , and when used in conjunction with R and I reduce the coefficients of R and I to a point where they are statistically indistinguishable from zero.

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COMMENT IV

In a recent article in this *Review*, Jerome L. Stein [9] attempted, among other things, to choose between the stock theory of international capital movements employed by Philip W. Bell [2] and the flow theory employed by Peter B. Kenen [7]. While Stein's recognition of the "simultaneity problem" (the simultaneous determination of the covered interest rate differential and capital flows) is a welcomed contribution, his treatment of the stock versus flow issue is objectionable. Clarification of this issue and correction of some errors that stemmed from Stein's misunderstanding of it are the objectives of this comment.

I. The Speed of Adjustment

The stock versus flow controversy arose because Bell [2] regressed the stock of U.S. short-term private claims outstanding on the level of interest

rate differentials, while Kenen [7] regressed the change in the stock of claims on the level of the differentials. Bell did not discuss his procedure and Kenen was somewhat cryptic.¹

This issue revolves around the *speed* at which economic units adjust their holdings of foreign assets in response to changes in interest rate differentials. Bell, by regressing stocks on levels of rate differentials, has implicitly assumed complete adjustment within a quarter to changes in rate differentials; if the relevant U.S. bill rate differential fell this quarter, and remained at the lower level in the future, it is assumed that U.S. investment in foreign assets would rise this quarter, but would not be further affected in following quarters. In contrast, Kenen's procedure of relating capital flows to the level of interest rate differentials assumes an extremely slow (technically zero) speed of adjustment. A permanent fall in the U.S. bill rate differential is expected to generate increases in U.S. investment abroad during this quarter and during all future quarters. Put another way, Kenen assumes (ignoring the constant term and other explanatory variables) that as long as there is a positive interest differential there will be a capital flow. Bell assumes that if the differential does not change—even if it is very large—there will not be a capital flow. While neither assumption is very likely correct, the former can be defended as an empirical approximation of a rapid speed of adjustment and latter, of a very slow speed of adjustment.²

This discussion of the Bell and Kenen assumptions can be formalized in terms of a Koyck-Nerlove distributed lag model as follows. In general, holders of an asset can be thought of as changing their stock of holdings during a period in response to the difference between their desired stock, S_t^* , and the stock existing at the beginning of the period, S_{t-1} .

$$(1) \quad S_t - S_{t-1} = k(S_t^* - S_{t-1}) \quad \text{where } k \text{ is the speed of adjustment.}$$

For simplicity, assume that the desired stock is linearly related to an interest rate differential, r , so that

$$(2) \quad S_t^* = \alpha_0 + \alpha_1 r_t$$

Then by substitution,

$$(3) \quad S_t - S_{t-1} = k\alpha_0 + k\alpha_1 r_t - kS_{t-1}$$

¹ In the only relevant passage in his report Kenen said: "By correlating these two types of data [capital flows and the level of interest rate differentials], we implicitly assert that a given interest rate difference will cause a given inflow or outflow of cash. One can conceive of an alternative hypothesis, linking the interest rates to total claims and liabilities, and changes in one to changes in the other. But this alternative would make sense only if we worked with absolute interest rates rather than with interest rate differentials" [7, p. 161]. (Interesting enough, Bell followed the exact reverse of the procedure that Kenen proposed. Bell regressed short-term claims outstanding on levels of interest rate differentials, but net purchases of foreign long-term bonds on absolute interest rates.)

² The appropriateness of the assumption depends in part on the length of the period of observation being used in the empirical estimation. For example, while the stock assumption might be very appropriate when working with annual data, it is probably very inappropriate when working with monthly data.

The flow theory which relates $S_t - S_{t-1}$ to r_t only, assumes that k is equal to zero, making S_{t-1} irrelevant.³ The stock theory, which relates S_t to r_t only, assumes that k is equal to one, again rendering S_{t-1} irrelevant (adding S_{t-1} to both sides of equation (3) gives a coefficient of $1 - k$ for S_{t-1} on the right hand side). An alternative to making either extreme assumption regarding k is to estimate equation (3), or its transformed counterpart that has S_t as the dependent variable.⁴ Stein [9, pp. 60-62] has inadvertently done the latter; the estimated monthly speeds of adjustment in response to changes in the U.S.-U.K. differential and his speculative variable are .07 for foreign investment in U.S. assets and .00 for U.S. investment in foreign assets. These results suggest that the flow theory might be a reasonable empirical approximation.⁵ This is, of course, quite consistent with Kenen's positive findings regarding the significance of interest rates and Bell's negative results.

II. Stein's Analysis

Stein, when attempting to resolve the stock-flow issue, mistakenly equated his general stock-adjustment equation [equation (3) above] to Bell's very restrictive stock (immediate-adjustment) theory. Stein failed to realize that estimation of a general stock-adjustment equation can yield empirical results that are consistent with *either* the stock or flow theories (or with neither). In fact, as was noted above, estimation of the stock-adjustment equation is a means of discriminating between the theories. This misunderstanding caused Stein much misery as he then felt it necessary to choose between his stock-adjustment and flow equations which are extremely similar. For example, compare the following equations of Stein [9, pp. 61, 62], where $SY(t)$ is the current stock of U.S. short-term banking claims of foreigners, $Y(t)$ is the corresponding flow, $SY(t-1)$ is the lagged stock, $R(t)$ is Stein's residual variable, and $I(t)$ is the current U.K.-U.S. 3-month Treasury bill rate differential:

³ Because the flow theory also assumes that an increase in foreign short-run interest rates relative to domestic rates generates a *perpetual* increased outflow of capital (assuming the differential remains unclosed), the long-run impact of an increase in the differential on the desired stock of assets, α_1 , is infinity. Thus r_t is relevant in equation (3), even when k is close to zero.

⁴ While the lagged stock formulation is an excellent theoretical device, its empirical usefulness is somewhat questionable due to the severe constraints imposed on the lag structure. The behavioral economic unit is assumed to adjust its holdings of an asset during a period by a constant fraction of the discrepancy between its long-run desired holdings and its existing stock the previous period, and the constant fraction adjustment is assumed to be the same regardless of which of a series of variables determining long-term desired holdings changes. For an empirical test of the appropriateness of these assumptions with regard to the demand for money, see Hendershott [4].

As an alternative to employing the stock-adjustment model one might regress capital flows on current and lagged changes in interest rate differentials, etc., allowing the data themselves to determine the lag structure. (This technique was employed in my test of the assumptions underlying the stock-adjustment model [4].)

⁵ My own limited experimentation also supports the flow theory as an empirical approximation. These experiments are to be reported in [6, Ch. 6].

$$(35) \quad SY(t) = \underset{(.0208)}{1.0005}SY(t-1) - \underset{(37.9192)}{43.8306}R(t) + \underset{(13.8128)}{23.765}I(t) + 11.5762$$

$$r = 0.9941$$

$$(37) \quad Y(t) = - \underset{(34.7920)}{51.9627}R(t) + \underset{(11.4715)}{23.1371}I(t) + 13.2214$$

$$r = 0.3518$$

The speed of adjustment *implied* by equation (35) is zero ($1-1.0005$), the same as that *assumed* by the formulation of equation (37). Not too surprisingly the regression coefficients are very similar (the coefficients of the interest rate differential are almost identical). There is no need (or basis) to discriminate between equations (35) and (37) because they say virtually the same thing—the capital flow is related to the level of the interest rate differential.

Stein's misunderstanding of Bell's stock theory also leads him to an erroneous explanation of why he found the interest rate differential to be a significant determinant of the U.S. stock of foreign assets but Bell did not. The two explanations that Stein provides [9, pp. 65, 66]—his analysis of aggregative data and his employment of the speculative variable—should lead him to a lower rather than higher estimate of the interest sensitivity than that obtained by Bell. As Stein notes [9, p. 47] there are substitution effects that *offset* part of the direct effect of interest rate movements and these might be missed in a disaggregative analysis such as Bell's.⁶ Similarly, Stein's speculative variable is designed to separate the speculative and interest rate effects, both of which would be attributed to interest rate sensitivity in an analysis such as Bell's. The reason for the difference in the Bell and Stein findings is that Stein uses the stock-adjustment formulation which, when the coefficient on the lagged-stock approximates one, is equivalent to Kenen's flow theory, rather than to Bell's stock theory.

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⁶ Stein's conjecture [9, p. 57] that the aggregative approach is the only way of accurately measuring the total interest rate effect is incorrect. Disaggregation overstates the total interest rate sensitivity (by ignoring substitutive effects) only if coefficients for some sub-categories of capital flows are not estimated and thus not included in the summation of interest rate coefficients. Total summation nets out substitution effects.

COMMENT V

Jerome Stein's recent article in this journal [9] has made a significant contribution to the study of private international short-term capital flows. Its importance rests at least as much upon its methodological innovations and implications for future study as upon the conclusions which it presents. Stein's criticisms of the previous studies in this area are well founded, and

his development of a proxy measure of speculative influence is an interesting but unsuccessful attempt to differentiate quantitatively between speculative and interest-induced short-term capital movements. Because of the importance of this article and its probable influence on future studies, it seems necessary to raise a point concerning Stein's procedure of disaggregating capital flows into two groups, United States and foreign, using separate regressions to estimate the sensitivity of each.

Stein's rationale for this separation is that "There is no a priori reason why the coefficients of the U.S. capital equation should be the same as those of the foreign private capital equation, since different parties are involved" [9, p. 57]. While this is true, the nature of the short-term capital flows during the period ... do not solely reflect differences in the responsiveness of these parties. Stein's estimating equations (15 and 22) state that U.S. and private foreign capital flows are functions of an interest-differential variable (I), a variable measuring speculative influence (R), and a disturbance term.¹ The characteristics of the present international monetary system are such, however, that the independence between U. S. and foreign capital movements assumed by Stein's regressions does not hold.

The flow of private foreign short-term capital between the United States and foreign countries is not independent of the flow of U.S. short-term capital. That is, outflows of capital from the United States will in themselves generate a partial offset in the form of induced inflows into the United States. The widespread practice among American banks of requiring borrowers to hold a certain portion of their loans in the lending banks as compensatory balances, as well as the tendency for many foreign borrowers to keep a portion of their funds in the United States as working balances, means that an increase in the foreign assets of U.S. banks will usually generate a somewhat automatic though smaller increase in the banks' liabilities to foreigners.² This is particularly important in the case of the large capital flows between the United States and Canada.

The claims of Canadian banks upon banks in this country ... representing more than one-fourth of total foreign commercial claims ... are closely related to the volume of funds placed by U.S. residents ... in Canadian banks. ... A very large proportion of the liquid U.S. funds placed in Canada is reinvested in the United States. [10, p. 118.]

Thus foreign private capital inflows into the United States are, *ceteris paribus*, a positive function of the outflow of capital from the United States.³ The outflow of U.S. capital is, however, a function of the interest

¹ Equation (15), the stock equation also includes the previous period stock.

² This consideration may have had little effect on the conclusions of Stein's aggregative analysis, for despite "strong direct correlation for Japan and Canada ... all countries taken together ... yield [slight] inverse relationships" [7, p. 159]. It could, however, be highly important in the study of capital flows between the United States and individual countries.

³ See also Oscar Altman [1 pp. 536-37]; and Kenen [7, p. 159]. In fairness to Stein, it must be mentioned that a large portion of the U.S. funds placed in the Canadian market come from nonbank corporations and would, therefore, not be included in his data.

rate differential, which in turn is a major determinant of the size of the foreign capital inflow. Therefore, an independent variable in each of Stein's foreign capital flow equations is correlated with the disturbance term, and the least squares estimates of the coefficients of responsiveness of foreign and U.S. capital flows . . . do not accurately measure the relative responsiveness of U.S. and foreign investors. In particular, this "bias" leads to an understatement of the interest sensitivity of foreign investors and an overstatement of the sensitivity of U.S. investors. The inflow of foreign funds generated by an interest-induced outflow of U.S. capital will move in a direction counter to that which is dictated by interest rate differentials, diminishing the net movements of foreign funds. Furthermore, this return inflow would tend to replenish the uncommitted balances of domestic arbitrageurs and allow them to place even more funds abroad. The actual sensitivity of U.S. capital would then be overstated by the amount of this secondary outflow. If movements of capital from Canada to the United States were induced, the desire of Canadian banks to draw down their New York balances would again generate a flow of capital counter to the direction of interest-rate and/or speculative incentives, once more overstating the sensitivity of U.S. investors and understating the sensitivity of foreign investors. This structural relationship may be one of the reasons why Stein found that private foreign capital appeared considerably less sensitive to interest rate differentials than did U.S. capital.⁴

It should be stressed that this comment on Stein's methodology is an economic, not a statistical one. This structural relationship need not statistically bias the least squares estimates of the capital variable coefficients because the secondary movements noted above are directly ascribed to the independent variables. The point is that the coefficients of the capital variables do not accurately measure U.S. and foreign investors' responses. In addition, the coefficients of the U.S. capital equation will not be independent of the decisions of foreign investors and, vice versa, the coefficients of the foreign capital equation will not be independent of the decisions of U.S. investors.

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⁴ Stein estimates [9, p. 65] that if the U. K. interest rates were raised by one percentage point (covered) above the U.S. rate, U.S. capital outflows would increase by \$277 million per annum, while foreign private capital inflows would decline by only \$185 million per annum.

Short-Term Capital Movements: A Reply

Apparently, there is no criticism of my theory of short-run capital movements. What has been criticized is my statistical estimation procedure. Since I think that the statistical analysis has low explanatory power, I wel-

come criticisms of it. In this Reply, I shall evaluate the criticisms of Gray, Heckerman, Hendershott, and Laffer and indicate some possible lines of research.

I. *The Use of the R Variable*

Three models were considered in my paper, summarized by equations (14), (21) and (26); hence, variable R has three meanings. Corresponding to the first model, from eq. (11),

$$R_1 = A_1 p^* - A_2 S(t-1) - A_2 (T-w).$$

Corresponding to the second model, from eq. (20),

$$R_2 = A_1 p^* - A_2 (T-w).$$

Corresponding to the third model, from eq. (25),

$$R_3 = \frac{-g_2}{g_1 + h} Z.$$

It is obvious from the first two equations and it was stated explicitly on pages 52-53 that, in the first two models, R is affected by price expectations (p^*), the parameter of the basic balance function (w), and official purchases or sales (T). Only in the third model (i.e., R_3) can R be regarded as depending entirely upon price expectations. Unfortunately, I was guilty of misleading nomenclature when I referred to R as a "speculative" variable; and I hope this misunderstanding is now cleared up.

Heckerman (in particular) questions my estimate of R on the grounds that it is correlated with "the disturbance term." If that were the case, the estimates of the regression coefficients would be biased. I shall show that, in my model, the parameter estimates are unbiased. They would be biased had I chosen the specifications of his alternative model.

Solve R_1 for p^* and substitute this value into equation (14) to obtain:

$$(14.1) \quad S(t) = K_3 x_3(t) + (K_2 - K_1 A_2 / A_1) S(t-1) - (K_1 / A_1) R_1 \\ + (K_2 - K_1 A_2 / A_1) (T-w).$$

It would appear that "the disturbance term," in the estimating equation, is $(T-w)$ which would be correlated with R_1 , since it is contained in R_1 . But this is not the case, since a substitution for the values of K_1 , K_2 , A_1 , and A_2 (from footnotes 11 and 12) shows that:

$$K_2 - K_1 A_2 / A_1 = 0.$$

The statistical counterpart of (14.1) would be:

$$(14.2) \quad S(t) = b_0 + b_2 R(t) + b_3 x_3(t) + u_1,$$

where u_1 represents a set of disturbances beyond what is specified in the model. These disturbances are not $(T-w)$, and hence are not obviously correlated with $R(t)$.

Equation (14.2) is the equilibrium solution¹ of difference equation (14), given p^* and $(T-w)$. My method of estimation is not amenable to the criticism that R is correlated with the disturbance term u_1 .

There are several problems. First, there is no reason to believe that R is a very good estimate of $K_1p^* - K_2W$, for the reasons presented in footnote 13. Second, the value of $S(t)$ is not necessarily at its equilibrium, given p^* and $(T-w)$. For these reasons, I used

$$(15) \quad S(t) = b_0 + b_1S(t-1) + b_2R(t) + b_3x_3(t) + u$$

as my estimating equation. The (implicit) value of u reflects the disturbances that were not captured by the other variables.

Heckerman shows that if disturbances are inserted in each of the structural equations, (A3) and (9), then my method of estimating R_1 or R_2 would lead to biased parameter estimates. That is correct; but I did not use such a model. My method of estimation was related to the model I specified.

It is possible to try to estimate p^* and w , in equations (14) and (21), directly rather than indirectly through the use of the residuals R_1 and R_2 . I tried this approach with very little success. Let $p^* = v_1p(t-1) + v_2p(t-2)$, where $p(t)$ is the dollar price of the pound in period t . Let the exogenous parameter $w(t) = m_0 - m_1M(t-1)$, where $M(t-1)$ is the U.S. money supply lagged one period. Substitute these into equations (14) (or 21) and obtain:

$$(14.3) \quad S(t) = b_1S(t-1) + b_2M(t-1) + b_3x_3(t) + b_4p(t-1) \\ + b_5p(t-2) + u.$$

With respect to the corresponding flows $y(t)$, the regression equation was:

$$(21.1) \quad y = a_1x_3 + a_2M(t-1) + a_3p(t-1) + a_4p(t-2) + u.$$

These equations were estimated for both U.S. short-term capital and private foreign short-term capital.²

Variables $S(t-1)$ and $M(t-1)$ were the only significant variables explaining the stock of short-term liabilities to foreigners. No independent variable was significant in explaining the flow of short-term liabilities to foreigners.

The stock of U.S. short-term claims on foreigners was significantly related to $S(t-1)$ and $x_3(t)$. The regression coefficient of x_3 was \$35 million dollars per percentage point. The flow of U.S. short-term claims on foreigners was significantly related only to $x_3(t)$. A one percentage point rise in the U.K. minus the U.S. Treasury bill differential would raise the flow of U.S. short-term capital by \$27.3 million per month, (the value of t was 2.13). The interest sensitivity is close to what I obtained in the original article.

Laffer's use of the U.S. trade balance as an independent variable in estimating capital flows is undesirable. It is a dependent variable: a function of

¹ This assumes that $K_2 \neq 1$. See Part II below. The references to equations refer to the original article. Equations with "decimals" such as (14.1) have been added here.

² The data for a given date change from month to month in the *Treasury Bulletin*. Hence, I used the last reference to a given date, among a set of issues, as my value for a capital figure. If Laffer did not use this approach, he would obtain his slightly different results.

the price of foreign exchange; and it is linked to the short-term capital flow via an accounting identity. It also fails to show the role of opportunity costs in determining the rate at which foreign or domestic assets are accumulated or decumulated. There is no difficulty in "explaining" the flow of short-term capital movements in a statistical sense, by regressing the flow $y(t)$ on a set of ad hoc variables. High values of R^2 can always be obtained, with sufficient experimentation. What is so difficult is to estimate the values of coefficients which a reasonable model claims are significant. I have not succeeded in this task, and the challenge to estimate the coefficients still exists.

Laffer suggested that the interest rate differential x_3 and "disturbances" are not independent; and cited my own example. Hence, *any* estimate of the regression coefficients, in an equation containing x_3 , would be biased. This is indeed a serious problem, since we are interested in the effect of changes in a *policy* variable upon the capital flow. What would be the appropriate policy variable to use, which is also independent of the other exogenous variables? Despite the valid criticism, I still prefer to use the Treasury bill differential, since it is a major policy variable. It could be made independent of the current period disturbance, if we assumed that the capital flow during period t depended upon $x_3(t-1)$. But is such an assumption justified? Can a better policy variable be suggested? Much more work remains to be done, if there is to be a connection between the theory and the empirical data.

II. Stocks or Flows?

Gray is primarily concerned with my statement that: more fruitful empirical results are obtained by considering the interest rate differential as a determinant of the allocation of a *flow* of international capital among countries, rather than as a determinant of the allocation of a *stock* of capital countries. I prefer the stock theory, on logical grounds, as does Gray. But the question is which theory provides a better explanation of the data? Ideally, how would we decide which theory provides a better explanation of the data? Gray did not come to grips with this question. My approach is as follows.

The stock theory is summarized by:

$$(14) \quad \begin{aligned} S(t) &= K_2 S(t-1) + [K_3 x_3 - K_1 p^* + K_2 (T - w)] \\ &\equiv K_2 S(t-1) + A. \end{aligned}$$

If K_2 is a positive fraction, this difference equation has the solution for a given A ,

$$(14.4) \quad S(t) = \frac{A}{1 - K_2} (1 - K_2^t) + S(0)K_2^t.$$

On the other hand, if K_2 is not significantly different from unity we have a flow theory. Consequently, one test of the explanatory power of the stock theory is whether our estimate of K_2 (i.e., b_1) is significantly different from unity. Gray noted that no set of observations yielded a value of K_2 different

from unity. This does not mean that the two theories merge into one; but rather that the stock theory fails this test.

There is another way to try to ascertain which theory is inconsistent with the data. Suppose that the flow of capital consists of two parts: one arises from a stock theory (eq. 14) and the other from a flow theory (eq. 21). Hence, the composite flow:

$$S(t) - S(t-1) \\ = f(x_3, p^*, T-w) + s(\Delta x_3, \Delta p^*, S(t-1) - S(t-2), \Delta(T-w)),$$

where f is a weighted function of the flow theory and s is a weighted function of the stock theory. If we find that some of the variables (x_3 , etc.) in f are significant, but that none of the variables (Δx_3 , etc.) in s is significant, are we not led to question the stock theory? Since the variables in s were not significant, I concluded that the flow theory does better on empirical grounds.

My estimation techniques were not very successful in explaining capital flows; and eventually better techniques will be found to test my model (Part II). But regardless of which set of techniques is used, is Gray willing to accept my tests of stock and flow theories? If so, then where does he differ with my paper? If not, what alternative tests are offered?

Although P. Hendershott and I agree that the flow theory seems to fare better than the stock theory on empirical grounds, there are some methodological differences between us. First, he believes that the introduction of a distributed lag adjustment process is required to differentiate between the stock and flow theories. As Yair Mundlak [8] has shown, however, it is illicit to introduce in an arbitrary manner a distributed lag adjustment process. The logical structure of a model must imply its own dynamic adjustment. Equation (14) is the dynamic equation implied by my model; and the coefficients included in K_2 determine its stability properties. The procedure suggested by Hendershott may lead to nonsensical or misleading results in many cases, as Mundlak has shown. For this reason I cannot agree that I should have explicitly used the distributed lag approach that he suggests.

Second, his distributed lag equation (1) hardly suffices to define a flow theory. He states that: "The flow theory . . . assumes that k is equal to zero . . . the stock theory . . . assumes that k is equal to one. . . ." If k is equal to zero, then $S_t - S_{t-1} \equiv 0$, and hence there can be no net flow of capital. Does he really mean to define a flow theory in such a way as to preclude a flow of capital? Since the data show large capital flows, would he reject the flow theory immediately? He has fallen into this difficulty because he has arbitrarily introduced a distributed lag as a *deus ex machina*.

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Diminishing Returns and Linear Homogeneity: Further Comment

While I have no desire to extend for extension's sake the already lengthy discussion in this *Review* regarding the consonance of increasing short-run physical marginal product with linearly homogeneous production, Robert Piron's recent "disproof" [3] of Stonier and Hague's "proof" [5, p. 226] of incompatibility is clearly in need of substantial correction.

Piron (rightly) considers the issue decided in favor of compatibility, noting that both Nutter's [1] and Rowe's [4] linearly homogeneous production functions exhibit a range of increasing marginal product for the variable factor. Wondering, then, where Stonier and Hague "went wrong," Piron (wrongly) concludes that "the persuasiveness of [their] proof seem-

ingly lies in the fortuitous choice of the fixed value for input Y ," that the factor input combination is "placed correctly" on the equal product map so as to fit the proof, and that by "badly" placing the input combination it is possible to negate their geometric proof. Specifically, Piron argues that "... had more isoquants been drawn, ... the demonstration of monotonically declining marginal product for X could prove difficult indeed" [3, pp. 185-86].

What follows is a demonstration of the proposition that it was not "the inherent shortcomings of plane geometrical proof" [3, p. 186] that led Stonier and Hague to a false generalization, but rather the fact that—in utilizing plane geometry—they restricted themselves to a consideration of downward-sloping convex isoquants. They did so on the ground that isoquant ranges that are upward sloping cannot contain equilibrium positions [5, p. 213]. Although downward-sloping convex isoquants, together with the linear homogeneity assumption, do imply diminishing returns (i.e., a convex production surface) over that particular range of inputs, it is nonetheless true that differently shaped or sloped isoquants would have yielded them different results. This is so because the shape and direction of slope of isoquants reflect the shape of the *linearly homogeneous* production surface, and thus the behavior of marginal product.

Following Stonier and Hague's analysis, assume a linearly homogeneous production function. Consider *any* combination P of fixed-factor Y and variable-factor X , as in Figure 1. P is necessarily a point on some isoquant with a value, say, of Q units. We assume isoquant Q to be convex to the x -axis. In attempting to determine the behavior of the marginal product of X , Stonier and Hague address themselves to the following question: Will it take a larger, smaller, or the same increment of X to reach isoquant $Q+\bar{k}$ units from isoquant Q , as it took to move from isoquant $Q-\bar{k}$ units to isoquant Q ?

A ray OR may be drawn from the origin through point P . OR will intersect isoquants $Q-\bar{k}$, $Q+\bar{k}$, and $Q+2\bar{k}$ at points S , V , and W respectively. The assumption of linear homogeneity ensures that $SP=PV=VW$. Consider a tangent to isoquant Q at point P . If the isoquant is *downward sloping*, it follows that the tangent to Q must extend from P to the northwest and southeast, as does tangent T_2 in Figure 1. Linear homogeneity implies that the marginal products of X and Y are homogeneous of degree zero, so that tangents T_1 through S , T_3 through V , and T_4 through W will be parallel to tangent T_2 , and will thus intersect a horizontal of height \bar{Y} drawn through P —say at A , C , and D respectively. Therefore, $AP=PC=CD$. Since all points (excluding S but including E) on isoquant $Q-\bar{k}$ must lie to the right of tangent T_1 , and all points (excluding V but including M) on isoquant $Q+\bar{k}$ must lie to the right of tangent T_3 , EP —the increment of X (Y constant) required to move from isoquant $Q-\bar{k}$ to isoquant Q —is necessarily less than AP , while PM —the increment of X required to move from isoquant Q to isoquant $Q+\bar{k}$ —must be *greater* than PC and therefore AP .

Is this demonstration of diminishing returns merely a result of "correctly placing" point P ? In particular, if we consider a further move from point M on isoquant $Q+\bar{k}$ to point U on isoquant $Q+2\bar{k}$, would Piron's assertion

Z but including P) on isoquant Q lie to the right of tangent T'_2 ; similarly, all points (excluding F but including U) on isoquant $Q+2\bar{k}$ lie to the right of tangent T'_4 . Therefore, $PM < JM = ML < MU$. And $EP < PM < MU$ reflects the monotonic decline of marginal product over input ranges of a linearly homogeneous production function characterized by *downward*-sloping convex isoquants.

But suppose that the relevant isoquants are *upward* sloping. Granted that these segments will not contain equilibrium points, it is nevertheless true

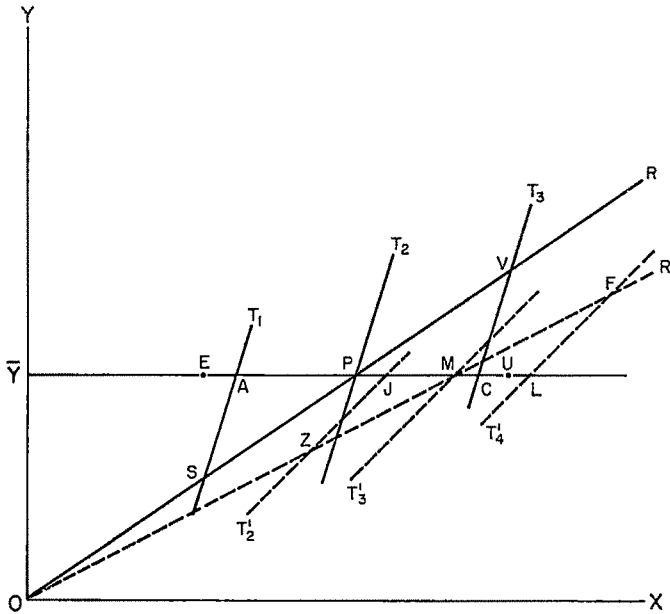


FIGURE 2

that linearly homogeneous production in itself does not exclude the possibility that such ranges may exist. What do upward-sloping convex isoquants—within the context of linear homogeneity—tell us about marginal productivity?

Precisely the same kind of analysis as above can be used to demonstrate *increasing* marginal product in the *upward*-sloping convex isoquant range of linearly homogeneous production. In Figure 2, the tangents and lettered points refer to isoquants with values comparable to those in Figure 1. However, the assumed shape of the isoquants imply northeast-southwest sloping tangents, and all relevant isoquant points are to the left of their respective tangent lines. $EP > AP = PC > PM$. Further, $PM > JM = ML > MU$. Thus, $EP > PM > MU$ demonstrates monotonically increasing physical marginal product.

In contradiction to Piron, the foregoing demonstrates that the tools of plane geometry are sufficient to determine the direction of movement of marginal product in the case of linearly homogeneous production. The

analysis could be extended to a consideration of isoquants that are concave to the x -axis; however, the results may be anticipated by taking note of three conditions that hold for the linear and homogeneous production function [2, p. 752]. Given such a function $q = \phi(x, y)$:

$$(1) \quad \phi_x = q/x - (y/x)\phi_y \quad (\text{Euler's theorem})$$

$$(2) \quad dy/dx = -\phi_x/\phi_y$$

and therefore,

$$(3) \quad d^2y/dx^2 = -(q/y)^2 \phi_{xx}/\phi_y^3$$

While it is true that the shape of the production surface is generally not revealed by isoquant analysis (e.g., convexity of isoquants does not in itself establish convexity of the production surface), the above conditions nevertheless imply that convexity and slope of isoquants do reflect the convexity of the linearly homogeneous production surface, and thus the behavior of marginal and average productivity. Consider Table 1. The above conditions

TABLE 1

| | $d^2y/dx^2 > 0$ (isoquants convex to x -axis) | $d^2y/dx^2 < 0$ (isoquants concave to x -axis) |
|---|--|---|
| $\phi_x > 0, \phi_y > 0$ (downward-sloping isoquants) | $\phi_{xx} < 0$ (diminishing marginal product) $\phi_x < q/x$ (diminishing average product) | $\phi_{xx} > 0$ (increasing marginal product) $\phi_x < q/x$ (diminishing average product) |
| $\phi_x > 0, \phi_y < 0$ (upward-sloping isoquants) | $\phi_{xx} > 0$ (increasing marginal product) $\phi_x > q/x$ (increasing average product) | $\phi_{xx} < 0$ (diminishing marginal product) $\phi_x > q/x$ (increasing average product) |

(2) and (3) together imply diminishing marginal product for variable factor X if and only if the relevant isoquant ranges are (a) downward sloping and convex to the x -axis, or (b) upward sloping and concave to the x -axis. Similarly, increasing marginal product for X emerges if and only if isoquants are (a) upward sloping and convex to the x -axis, or (b) downward sloping and concave to the x -axis. (Thus, Piron's statement [3, p. 186] that Stonier and Hague's equal product map—reproduced as his Figure 2—"could well represent a picture of the [increasing marginal product ranges of the] Nutter or Rowe functions" is incorrect, since the relevant isoquants are both downward sloping and convex to the x -axis.) Finally, conditions (1) and (2) together imply that only isoquants that are downward (upward) sloping reflect diminishing (increasing) average product.

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Reply

Saraydar correctly demonstrates that the Stonier-Hague proof can be extended to any number of isoquants. However, his assertion that "... the foregoing demonstrates that the tools of plane geometry are sufficient to determine the direction of movement of marginal product in the case of linearly homogeneous production" is much too confident.

Downward-sloping, "correctly" curved isoquants, coupled with linear homogeneity, do, and I stand corrected on this point, provide sufficient conditions for a geometrical proof of monotonically declining marginal products. But, and here Saraydar is again correct, these are merely conditions defining a convex production surface. It is the convexity of the surface which provides the sufficient condition for falling marginal products, however, not the linear homogeneity property. This latter point was amply demonstrated in this *Review* earlier. Thus, where Stonier and Hague "went wrong" in assuming that linear homogeneity was sufficient for falling marginal product, was in choosing for their example the particularly fortunate case of a convex production function.

Had they instead used a linearly homogeneous, nonconvex production function, i.e., one which, in particular, generated downward-sloping "humped" isoquants as in the Nutter-Rowe examples, the geometrical proof of monotonically declining marginal products or of any behavior of marginal products, would have been exceedingly trying.

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The Demand for General Assistance Payments: Comment

In a recent article published in this journal [2], C. T. Brehm and T. R. Saving (henceforth BS) argued that the demand for General Assistance Payments¹ can be formulated as a special case of the demand for leisure, and assuming potential relief recipients are as rational as the rest of the popula-

¹ A class of public relief or welfare benefits that are not federally subsidized. Henceforth referred to as GA.

tion, that higher levels of benefits lead to a greater demand for them. Although they carefully disclaimed any policy conclusions [2, p. 1015], BS in effect revived the view that high benefits encourage sloth, a view that antedates Marshall and Pigou, whom they cite [2, pp. 1001-3], and may also be found in the Report of the Poor Law Commissioners and in the economic tales of Harriet Martineau.² BS reformulated the argument with the use of indifference curve analysis, derived a demand curve for GA, and estimated its parameters in an econometric study. The results of their empirical analysis appear to indicate that the amount of GA taken in the several states is positively related to the level of benefit payments. On a close examination of the empirical work, we discovered that BS had made a significant error either in compiling or interpreting their data. With the correction of this error the observed positive association between the level of GA and the number of recipients, a relationship crucial to their analysis, completely evaporates. Without this empirical support the demand for leisure model of the relief environment is open to serious question.³

Empirical Findings

Let us begin with a brief review of the BS empirical model and results. The demand function derived by BS relates the per cent of a state's population receiving assistance payments (N) to the following independent variables: average monthly payment per recipient (P), average monthly manufacturing wage in the state (W), the rate of insured unemployment in the state (U), and nonagricultural employment in the state (NA), [2, p. 1012]. Equations employed in the empirical analysis were of the ratio form:⁴

$$(3.21) \quad N = a_1 + b_{11} \left(\frac{P}{W} \right) + b_{21} U$$

$$(3.22) \quad N = a_2 + b_{12} \left(\frac{P}{W} \right) + b_{22} U + b_{32} NA$$

and of the linear form:

$$(3.23) \quad N = a_3 + b_{23} U + b_{43} P + b_{53} W$$

$$(3.24) \quad N = a_4 + b_{24} U + b_{34} NA + b_{44} P + b_{54} W$$

P is the critical variable in the analysis. A positive significant value was predicted by BS for the coefficient of this variable in equations (3.23) and (3.24) and for the coefficient of P/W in equations (3.21) and (3.22). This result was apparently obtained in the BS calculations. It is our contention that the predicted behavior was not actually observed, and that the BS re-

² See Blaug's work on the discrepancy between data and conclusions in the study made by the Poor Law Commissioners under Nassau W. Senior's guidance [1, pp. 231-33] and Martineau's "Cousin Marshall" [3, pp. 103-6, 110-122, 129, 130-32] and "The Parish" [4, pp. 209-10].

³ We are currently developing alternative models of relief choice.

⁴ Subscripts i and j for state and sample year identification are omitted here for purposes of simplification.

sult is very probably the consequence of a misreading of the tables attached to the basic data or an error in model construction.

Perhaps the best way to explain the BS error and to clarify the empirical problems in this matter is to chronicle the history of our research. In the text of their paper, BS considered "the number of general assistance recipients as the relevant dependent variable. . . . Data have been collected on numbers of G.A.P. recipients on an average per-month basis. . . ." [2, p. 1007] We were, at first, unable to duplicate the results given in the original article since data on *recipients* of GA for the years 1951 through 1959 did not, apparently, exist in any published form, whereas data on *cases* were easily available in the *Social Security Bulletin* [8] and the *Statistical Abstracts* [7]. The citation given in the article was clearly an error, and we wrote to BS to ask for clarification. In reply, Brehm referred us to the *Social Security Bulletin* and *Statistical Abstracts*. On a hunch, we calculated regressions on the BS model using *case* data for their *recipients* variable. Our results approximated theirs and it became clear that BS used *case* data in their work. The distinction between *case* and *recipient* is important here. A recipient is always one person. A case consists of one or more recipients of a particular category of public assistance, and may comprise an entire household, part of a household, or one person. Definitions of GA case units, however, differ among states, and the GA case cannot be identified with a decision unit such as a household or family. In any event, payment per case cannot be considered a valid measure of benefit levels. Surely, \$50 per month for a case unit of three individuals represents a lower level of benefits than \$50 per month for a case unit of one person. Hence, when we discovered a considerable amount of state-to-state variability in the number of persons per case, what had at first appeared to be a minor error in labeling assumed substantive importance. When we adjusted the case data to a recipient basis and recalculated the regressions, we discovered that the positive relationship between the benefit level and the proportion of the population receiving such benefits disappeared.

In the paragraphs below we shall present the data and show the point at which the error was made. The significance of the error will first be shown crudely with the aid of scatter diagrams and then more precisely with the use of regressions recalculated with the correct data. This will be done in detail for a single representative year, 1959, and, to conserve space, in somewhat less detail for the rest of the period covered by the BS study. Finally, regressions on the BS model will be calculated for 1961–63 since actual recipient as well as case data are available for those years.

Data on individual recipients are available only for 1961 and subsequent years. We used 1961 figures to calculate a recipients-per-case measure for each state to correct the case data for the BS sample period.⁵ Table 1 con-

⁵ We chose 1961 as the basis for adjustment as it is the year with adequate data that is closest to the sample period. Recipients-per-case ratios do not change appreciably in later years, and our analysis assumes that they remain close to 1961 levels in earlier years as well. Federal programs instituted in 1961 reduced the number of GA recipients but apparently did not alter the relationships discussed in this study. Attention must be also directed to the poor quality of state data used both by us and by BS. Federal statistical standards do not apply, and data are not always comparable from state to state.

TABLE 1—CASE DATA CORRECTED TO A COMMON RECIPIENTS BASIS

| States ^a | | Average Payments per Case ^b | Cases as Per Cent of Popula- tion ^c | Recipients per Case ^d | Average Payment per Recipient | Recipients as Per Cent of Population |
|---------------------|----------------|--|---|-------------------------------------|-------------------------------------|--|
| | | (1) | (2) | (3) | (4) = (1)/(3) | (5) = (2) × (3) |
| ME | Maine | 43.13 | .0030 | 3.59 | 12.08 | .0107 |
| NH | New Hampshire | 51.46 | .0016 | 3.50 | 16.18 | .0049 |
| MA | Massachusetts | 77.65 | .0017 | 2.17 | 35.78 | .0036 |
| RI | Rhode Island | 72.17 | .0036 | 2.24 | 33.26 | .0077 |
| CT | Connecticut | 71.48 | .0018 | 2.57 | 26.77 | .0048 |
| NY | New York | 95.91 | .0022 | 2.29 | 42.44 | .0050 |
| NJ | New Jersey | 102.69 | .0016 | 3.40 | 30.93 | .0053 |
| PA | Pennsylvania | 69.89 | .0038 | 1.21 | 57.76 | .0046 |
| OH | Ohio | 70.66 | .0036 | 3.70 | 19.15 | .0135 |
| IN | Indiana | 35.09 | .0043 | 3.38 | 10.38 | .0147 |
| IL | Illinois | 94.05 | .0046 | 2.30 | 37.77 | .0115 |
| MI | Michigan | 104.14 | .0044 | 3.62 | 28.07 | .0162 |
| WI | Wisconsin | 82.43 | .0021 | 4.38 | 25.60 | .0067 |
| MN | Minnesota | 74.00 | .0027 | 3.01 | 23.34 | .0085 |
| IA | Iowa | 39.02 | .0015 | 2.50 | 16.06 | .0037 |
| MO | Missouri | 59.63 | .0020 | 1.37 | 42.29 | .0028 |
| ND | North Dakota | 56.12 | .0011 | 4.30 | 12.67 | .0048 |
| SD | South Dakota | 34.34 | .0006 | 3.26 | 11.08 | .0019 |
| NB | Nebraska | 48.22 | .0008 | 3.05 | 16.02 | .0024 |
| KS | Kansas | 65.84 | .0011 | 2.92 | 21.04 | .0033 |
| DE | Delaware | 62.72 | .0039 | 1.66 | 40.73 | .0060 |
| MD | Maryland | 64.35 | .0010 | 1.14 | 56.45 | .0011 |
| VA | Virginia | 41.07 | .0005 | 2.04 | 17.93 | .0012 |
| WV | West Virginia | 31.74 | .0013 | 1.56 | 20.35 | .0021 |
| NC | North Carolina | 22.80 | .0005 | 3.02 | 7.81 | .0014 |
| SC | South Carolina | 31.03 | .0006 | 1.38 | 17.14 | .0010 |
| GA | Georgia | 24.94 | .0007 | 2.55 | 10.14 | .0017 |
| KY | Kentucky | 30.12 | .0007 | 2.68 | 10.04 | .0022 |
| TN | Tennessee | 15.57 | .0006 | 3.20 | 4.90 | .0020 |
| AL | Alabama | 12.49 | .0000 | 1.32 | 9.46 | .0000 |
| MS | Mississippi | 15.05 | .0005 | 1.36 | 11.32 | .0007 |
| AR | Arkansas | 15.41 | .0002 | 3.86 | 3.97 | .0008 |
| LA | Louisiana | 49.03 | .0027 | 1.14 | 44.57 | .0030 |
| MT | Montana | 51.69 | .0048 | 3.22 | 16.62 | .0149 |
| WY | Wyoming | 60.60 | .0016 | 4.25 | 14.09 | .0069 |
| CO | Colorado | 47.59 | .0010 | 3.87 | 11.99 | .0040 |
| NM | New Mexico | 42.00 | .0006 | 1.73 | 24.71 | .0010 |
| AZ | Arizona | 45.92 | .0025 | 2.01 | 26.09 | .0044 |
| UT | Utah | 85.00 | .0029 | 2.17 | 65.38 | .0038 |
| NV | Nevada | 28.98 | .0011 | 2.01 | 13.93 | .0024 |
| WA | Washington | 79.14 | .0044 | 2.12 | 47.11 | .0074 |
| CA | California | 59.57 | .0022 | 2.50 | 24.31 | .0055 |

^a States arranged by Census Region. Complete data not available for missing states.^b Source: "Public Assistance—Payments to Recipients" [7, 1960].^c Source: "Public Assistance—Recipients" column headed General Assistance (cases); "Estimated Population by States" [7, 1960].^d Source: "Public Assistance Recipients" [9, 1962].

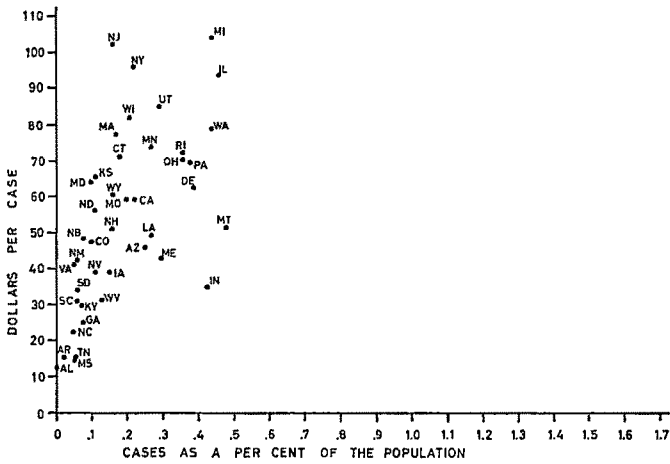


FIGURE 1

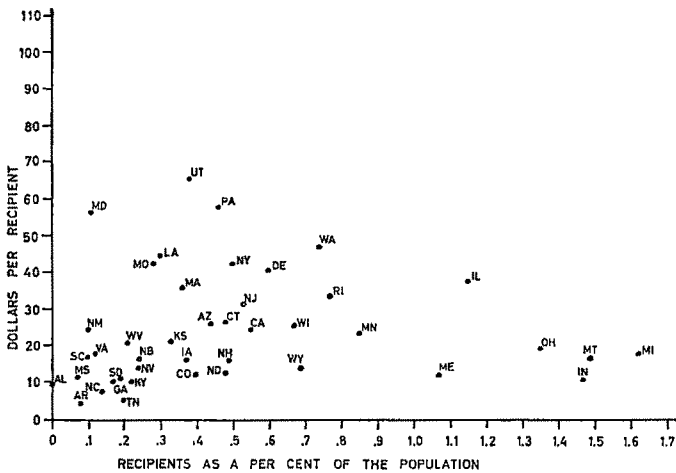


FIGURE 2

tains the data for 1959 presumably used by BS: average payment per case (Column 1) and number of cases as a per cent of state population (Column 2). The recipients-per-case ratio is given in Column 3, and Columns 4 and 5 contain the case data adjusted to a common recipients basis. Thus, where Colorado might seem to have a high benefit level, \$47.59 per case, as compared to \$15.05 per case for Mississippi, the differences disappear when the data are placed on a common basis: \$11.99 per recipient in Colorado and \$11.33 per recipient in Mississippi. The payment-per-recipient measure approximates the measure of benefit levels that the relief client will take into

TABLE 2—CORRECTION OF BREHM-SAVING REGRESSION EQUATIONS, 1959 SAMPLE

| Equation No., Data | Coefficient (Standard Error) | | | | | R ² (F) (6) |
|--------------------|-------------------------------|-------------------------------|-------------------------------|--------------------------------|-------------------------------|---------------------------|
| | P/W (1) | U (2) | NA (3) | P (4) | W (5) | |
| (3.21) BS | .1211 ^a (.0237) | .2090 ^b (.0882) | | | | |
| Case | .1054 ^a (.0315) | .1739 (.1264) | | | | .246 (6.70) |
| Recip. | -.0286 (.1739) | .3396 (.4451) | | | | .015 (.29) |
| (3.22) BS | .0952 ^a (.0274) | .2180 ^b (.0898) | .0515 ^b (.0339) | | | |
| Case | .0602 (.0422) | .1773 (.1241) | .0855 (.0542) | | | .290 (5.46) |
| Recip. | -.2933 (.1813) | .4194 (.4064) | .4432 ^a (.1479) | | | .203 (3.23) |
| (3.23) BS | | .2637 ^a (.0883) | | .0386 ^a (.0062) | .0005 (.0010) | |
| Case | | .2245 ^b (.1120) | | .0192 ^b (.0078) | .0089 ^b (.0036) | .433 (10.16) |
| Recip. | | .5695 (.3800) | | -.0381 (.0395) | .0429 ^a (.0105) | .318 (5.89) |
| (3.24) BS | | .2584 ^b (.0915) | .0190 (.0344) | .0370 ^a (.0073) | .0008 (.0010) | |
| Case | | .2229 ^b (.1121) | .0461 (.0482) | .0137 (.0097) | .0088 ^b (.0035) | .446 (7.83) |
| Recip. | | .5819 (.3668) | .2720 ^b (.1395) | -.0745 ^b (.0423) | .0374 ^b (.0185) | .381 (5.70) |

^a Significant at 1 per cent level.

^b Significant at 5 per cent level.

Sources: *N* and *P* given in Table 1; *W*, computed from "Production Workers in Manufacturing Industries" [7, 1960]; *NA*, computed from "Employees in Non-Agricultural Establishments" [7, 1960]; *U*, "Benefits and Claims Data" [10].

account.⁶ The effects of the correction are quite apparent when we display the data in scatter diagrams. In Figure 1 *cases* as a per cent of population are displayed on the ordinate (the letters identify the states—see Table 1). The positive relationship of the BS study is easily detectable. When the data are corrected to show *recipients* as a per cent of population and average payment per *recipient* (Figure 2) the scatter shows no such relationship.

⁶ In our exposition we have followed BS who, from the wording in their text, appeared to consider the recipient (individual) to be the relevant economic unit. BS may, perhaps, have used case data with the intent of developing a decision-unit or household model. However, the corrections we have made apply even if one wishes to develop the analysis in these terms. Case data must be weighted by case size to obtain comparability among states and, as we have shown, payment per recipient is an appropriate measure of benefit levels, whereas payment per case is not. Of course, a more nearly perfect measure of benefit levels can be devised by taking into consideration the actual formulae applied by relief administrators to determine budgetary needs, but this is beyond the scope of our Comment.

Figure 2 portrays the true relationship. The positive association seen in Figure 1 is spurious and a purely arithmetic consequence of nonuniformity in case definitions among the states.

We turn next to the correction of the BS regression results. The correction of regression equations for the representative year 1959 is shown in Table 2. The rows of the table are broken into four sections, corresponding to equations (3.21, 3.22, 3.23, and 3.24) above. Each section contains three subdivisions: the first has the original BS results, the second has our approximation of the BS equations using *case* data, and the third subdivision contains the corrected regressions using *recipient* data. We employed single equation, least squares in estimating, rather than the Aitkin-Zellner technique used by BS. Exact duplication of their empirical procedures was not warranted; our correction, since it involves back-extrapolation from current data, is undoubtedly more accurate for the later years of the BS sample than for the earlier years.

We see first a close correspondence between the BS results and our regressions using "incorrect" data. Coefficient signs, magnitudes, and levels of statistical significance correspond closely; the minor differences are attributable to the different estimation techniques and to our use of single monthly observations rather than annualized data.⁷ The "correct" regression, however, reveals a much different type of behavior than that hypothesized by BS. The highly significant positive relationship between the per cent of population taking assistance and the benefit level disappears (see Columns 1 and 4). In fact, a negative sign crops up and the other coefficients take on strange values. R^2 and F fall drastically from their levels in the "incorrect" regression. (Note, in particular, the value of R^2 in equation 3.22.)

Corrected results for the earlier years of the sample, 1951-58, are presented in Table 3. To conserve space, only R^2 , F , and coefficient values and levels of significance for P and P/W are given. Values for these statistics are given in the columns of the table for each year of the sample, while the rows are arranged in blocks of three cells. As before, the first cell of each block contains the original BS result, the second cell contains the corrected regression. Again, recipient data were constructed by back-extrapolation of the 1961 recipients-per-case ratio. Although it would be foolish to put much reliance on the accuracy of our correction for the earliest years of the sample, it is again clear that no positive significant relationship exists between the number of recipients and the benefit level. A negative sign was observed in all runs. About half the time the values were significant.

Finally, we calculated regressions on the BS model using the more complete data available for the sample period 1961-63. Table 4 follows a format similar to that of Table 3 and contains results in regression equations calculated first with case data and second on the correct basis with current recipient data. The crucial parameters, the coefficients of P or P/W , are statistically insignificant and usually negative when recipients data are used, and again, R^2 and F decline precipitously.

⁷ We were not able to obtain recipient data to correspond precisely with the annualized data used by BS.

TABLE 3—CORRECTION OF BREHM-SAVING REGRESSION EQUATIONS, 1951-58 SAMPLES

| Equation No. and Data | 1951 | | | 1952 | | | 1953 | | | 1954 | | |
|-----------------------|--|--------------|----------------|--|--------------|---------------|---|--------------|---------------|--|--------------|---------------|
| | Coefficient of P/W or P^a | R^2 | F | Coefficient of P/W or P^a | R^2 | F | Coefficient of P/W or P^a | R^2 | F | Coefficient of P/W or P^a | R^2 | F |
| (3.21) BS Case Recip. | .1236 ^b .1279 ^b — .0174 | .526 .188 | 20.52 4.29 | .0925 ^b .1211 ^b — .1076 | .327 .016 | 9.24 .31 | .0935 ^b .1139 ^b — .0680 | .349 .016 | 10.19 .30 | .1141 ^b .1480 ^b — .0290 | .445 .001 | 15.61 .02 |
| (3.22) BS Case Recip. | .0648 .0454 ^b — .2632 ^a | .675 .561 | 24.93 15.33 | .0317 .0423 — .3681 ^b | .452 .403 | 10.16 8.32 | .0808 ^b .0525 — .3288 ^a | .452 .400 | 10.17 8.22 | .1006 ^b .1042 ^a — .3543 ^a | .477 .325 | 11.56 6.10 |
| (3.23) BS Case Recip. | .0423 ^b .0365 ^b — .0168 | .573 .410 | 16.09 8.53 | .0314 ^b .0254 ^a — .0633 | .463 .326 | 10.63 5.96 | .0359 ^b .0306 ^b — .0295 | .453 .237 | 10.22 3.83 | .0398 ^b .0430 ^b — .0183 | .462 .145 | 10.87 2.14 |
| (3.24) BS Case Recip. | .0247 ^a .0148 — .0913 ^a | .676 .574 | 18.25 11.77 | .0134 .0073 — .1271 ^b | .532 .485 | 10.22 8.46 | .0311 ^b .0166 — .1045 ^a | .505 .444 | 9.19 7.20 | .0385 ^b .0309 ^a — .1111 ^a | .448 .351 | 8.82 5.00 |
| | | | | | | | | | | | | |
| (3.21) BS Case Recip. | .1236 ^b .1250 ^b — .0601 | .309 .004 | 8.93 .07 | .0887 ^b .1184 ^b — .0407 | .317 .005 | 8.83 .09 | .0850 ^b .0789 ^a — .0648 | .147 .030 | 3.29 .58 | .1100 ^b .1375 ^b — .0644 | .346 .087 | 10.05 1.80 |
| (3.22) BS Case Recip. | .0967 ^b .0729 ^a — .3263 ^a | .372 .292 | 7.71 5.23 | .0729 ^b .0810 ^a — .3404 ^a | .343 .302 | 6.44 5.33 | .0732 ^b .0291 — .4441 ^a | .188 .309 | 2.85 5.37 | .0825 ^b .0869 — .6121 ^a | .371 .320 | 7.27 5.81 |
| (3.23) BS Case Recip. | .0435 ^b .0332 ^b — .0332 | .324 .188 | 6.24 2.94 | .0299 ^b .0270 ^b — .0319 | .366 .259 | 7.12 4.30 | .0274 ^b .0129 — .0547 | .244 .355 | 3.99 6.60 | .0329 ^b .0257 ^a — .0743 | .499 .398 | 12.29 8.15 |
| (3.24) BS Case Recip. | .0376 ^b .0182 — .0985 ^a | .385 .357 | 5.96 5.13 | .0280 ^b .0149 — .0985 ^a | .397 .429 | 5.92 6.77 | .0276 ^b .0033 — .1191 ^a | .266 .460 | 3.27 7.45 | .0315 ^b .0140 — .1765 ^b | .520 .515 | 9.73 9.55 |

^a P/W appears in equations (3.21) and (3.22). P appears in equations (3.23) and (3.24).^b Significant at 5 per cent level.

Sources: Same as Table 2, for appropriate years.

^b Significant at 1 per cent level.

TABLE 4—REGRESSIONS USING PUBLISHED RECIPIENT DATA, SAMPLE YEARS 1961-1963 [9]

| Equation No. and Data | 1961 | | | 1962 | | | 1963 | | |
|--------------------------|---|--------------|---------------|---|--------------|--------------|---|--------------|--------------|
| | Coefficient of P/W or P^a (1) | R^2 (2) | F (3) | Coefficient of P/W or P^a (4) | R^2 (5) | F (6) | Coefficient of P/W or P^a (7) | R^2 (8) | F (9) |
| (3.21) Case Recip. | .1295 ^c .0474 | .342 .061 | 10.13 1.21 | .1024 ^c — .0249 | .274 .023 | 7.12 .43 | .0546 — .6132 | .217 .084 | 5.26 1.64 |
| (3.22) Case Recip. | .1040 ^c — .1640 | .363 .180 | 7.21 2.63 | .0706 — .2688 | .310 .163 | 5.55 2.34 | .0116 — .9411 | .271 .095 | 4.58 1.23 |
| (3.23) Case Recip. | .0233 ^c — .034 ^b | .471 .318 | 11.25 5.60 | .0158 — .042 ^b | .395 .281 | 8.06 4.69 | .0030 — .014 ^b | .376 .087 | 7.44 1.11 |
| (3.24) Case Recip. | .0204 ^d — .058 ^b | .478 .355 | 8.45 4.81 | .0122 — .070 ^b | .408 .326 | 6.20 4.23 | — .0038 — .2276 | .408 .104 | 6.20 .98 |

^a P/W appears in equations (3.21) and (3.22). P appears in equations (3.23) and (3.24).^b No further significant digits.^c Significant at 1 percent level.^d Significant at 5 percent level.

Sources: Recipients data [9, 1962-1964]; other data same as in Table 2 for appropriate years.

Summary and Conclusions

Since empirical evidence does not support the BS hypothesis, their "demand for leisure" approach and its theoretical underpinnings must be questioned. It seems to us that the key issue is one of choice. BS characterize the general assistance programs as offering a voluntary income alternative to work: "... qualifications [for GA eligibility] may be met by a deliberate action of the consumers and thus fall within the scope of our [BS] analysis" [2, p. 1007]. Under typical conditions, however, the choice is not effectively offered. The aid recipient is not permitted simply to opt for leisure. He is generally required (not "allowed") to work if able to work and work opportunities exist either in the market place or in work projects. In practice, the case worker controls a budget that he is likely to judge inadequate relative to legitimate demands for assistance at allowable benefit levels and he may be motivated to cut off from assistance those who could work but who would rather not. In effect, the BS analysis seeks to describe voluntary behavior in a situation in which restricted behavior is the general rule.⁸ This is not to say, of course, that disincentive effects may not exist in specific cases where standards of control are loose and where certain individual benefits may be high enough to encourage chiseling (averaged data may conceal instances of high benefits). On the whole, however, the BS disincentive effect can be dismissed as a significant consideration in policy. The GA recipient may, indeed, "be like the remainder of consumers . . . and react to economic incentives" [2, p. 1018], but only if a meaningful choice is offered to him. The relief controversy in Newburgh, N. Y.⁹ may be a good illustration that a model based on individual choice is not appropriate where such choice is absent.

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* The authors, respectively associate and assistant professor of economics at New York University, wish to thank their colleagues in the Faculty Research Seminar for their valuable suggestions, and the Institute of Labor Relations for its financial support. The calculations were performed on the CDC 6600 at the Courant Institute.

⁸ Alternatives to GA that are actually open are the following: (1) to work for less than the GA social minimum income, a choice made by people whose discount k [2, p. 1004] is high; (2) as a family unit, to work an unusually large number of hours per week, e.g., by putting school age children to work; (3) to participate in private rather than social redistribution of wealth, such as charity or theft; (4) to starve. It may be useful to note, at this point, that the average national level of GA benefits is in the neighborhood of \$22 per month per recipient.

⁹ On July 17, 1961, after a thorough and somewhat unsympathetic analysis of 1300 recipients of all forms of public assistance, the city found one man who might—possibly—be physically able to work [5, pp. 38–39]. Recent experience in Pennsylvania suggests that a social investment model may be a more appropriate tool of analysis. In that state, increases in benefit levels to finance dignified and unencumbered entry into the labor market were successful in reducing the absolute number of individuals on the relief rolls [6, p. 1211]. This raises an old question: do low benefit levels actually increase the dependence of the relief population on assistance?

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The Demand for General Assistance Payments: Reply

It is gratifying to see that our article on the general assistance program has generated some interest in this problem by other economists. The question of the extent that Public Assistance programs are self-defeating is still an open one and, we believe, an area in which the application of economic analysis has been sorely lacking. Unfortunately, while Stein and Albin have discovered a problem in the data we used, they have not succeeded in removing this flaw. Thus, their results are as open to question as our original results. Moreover, when the problem they are concerned with is properly taken account of, the conclusions we reached in our original paper are substantiated.

Essentially, the Stein and Albin argument may be stated in the following way. The data we originally used were on a case basis with a case being defined as a group of people, usually a family, who pool their resources and live as a single unit. We then interpreted the case unit as the decision-making unit, in general a correct conclusion, and applied the theoretical argument we developed to this unit. However, we did not anticipate that subsequent data on total number of persons in families being assisted (called recipients by HEW) would vary by state in such a way that the average assisted family size would show significant differences from state to state. (As it turns out, there is good reason for this. We should have seen this point immediately.) Of course, if the level of the general assistance payment is independent of family size, the differences in average family size would be irrelevant. However, this is not the case and, hence, Stein and Albin's suggestion for expressing everything on a HEW recipient basis is justified. Stein and Albin then proceed to use data on total persons in assisted families (HEW recipient) for the period 1961-1963 and their ("correct"?) results indicate that no relationship exists between "recipients" and levels of payments per recipient.

Now we reach the crux of the problem. The so-called "correction" that Stein and Albin have made is inappropriate for two reasons. First, their correction fails to account for the problem they have discovered because it

fails to consider the reason for the problem. Second, a change in the Social Security Act in May 1961, of which Stein and Albin should have been aware but apparently were not, raises a serious question concerning the relevance of the data they use. We now discuss each of these points.

Problems with the Stein-Albin "Correction." Stein and Albin assume (as we did also) that all persons receiving general assistance fit into the theoretical model we described in our paper. In other words, they assume that each recipient could make the choice between work and general assistance. However, this is not the case. More importantly, the degree to which this assumption is in error differs from state to state in a systematic manner and accounts, in large part, for the differences between the states in average assisted-family size. In particular, differences in the scope of the general assistance program between the various states and, hence, in the ease with which someone who has the work-assistance option can get on the assistance rolls, will affect the proportion of all recipients to which the theoretical apparatus applies. For example, all states use general assistance to assist the aged and disabled but not all states allow the able-bodied to be on the rolls. Thus, the more restrictive the rules the fewer the able-bodied who will be aided. Since the aged and partially disabled are generally aided on a single-person basis, the states with restrictive rules will tend to have a smaller number of recipients per case. What needs to be done is to remove payments to single-person cases and the single-person cases from the study since these do not reflect the theoretical structure. Of course, if these problems were unrelated to magnitude of assistance payments, this adjustment would not affect the results. But, this, as we shall see, is not the case.

The Change in the Law. In May 1961, Congress amended the Social Security Act to provide federal aid to those states wishing to extend their program of Aid to Dependent Children to families with an unemployed member present (this program is referred to as ADC-U). This gave the states the opportunity of substituting a partially federally supported program for an entirely state supported one; that is, taking families on general assistance because of unemployment and transferring them to the ADC-U program. That the states have been doing this is well documented by their own welfare agency reports. The importance of this change in the law is that it has systematically removed from the general assistance rolls the very people for whom the theoretical model applied; those with the work-assistance choice. But, the years for which Stein and Albin have data are subsequent to the change in the law and, hence, the model should be expected to perform badly with their data.

"Correcting" the "Correction." As we have indicated above, the data we used in our original paper were inappropriate and needed to be adjusted for the single-person cases. We were able to obtain data on the number of one-person cases and the payments to one-person cases for the month of December, 1959 from the Department of Health, Education and Welfare. We then subtracted the one-person cases from the total persons in assisted families and subtracted the payments to one-person cases from the total payments. Unfortunately, these data were available for only 34 of the 48 states we

used in our original work. In addition, this correction did not remove all the variation in the average assisted family size so we followed Stein and Albin's lead and used total recipients in the *ith* state as a percentage of population in the *ith* state (corrected for one-person cases, of course) as our dependent variable and payments per recipient as our primary independent variable. We also included the rate of unemployment and two dummy variables that indicate the ease of getting on the welfare rolls. We ran the following regressions.

$$(1) \quad R_i^* = a_1 + b_{11}P_i^* + b_{12}U_i + b_{13}D_{1i} + b_{14}D_{2i}$$

$$(2) \quad \ln R_i^* = \ln a_2 + b_{21} \ln P_i^* + b_{22} \ln U_i + b_{23}D_{1i} + b_{24}D_{2i}$$

where:

R^* = Recipients (corrected) in the *ith* state divided by population in the *ith* state.

P^* = Assistance payments per recipient in the *ith* state.

U = Unemployment rate in the *ith* state.

D_1 = A dummy variable taking the following value:

0—if *ith* state has no residence requirement.

1—if *ith* state has a residence requirement.

D_2 = A dummy variable taking the following value:

0—if any kind of need qualifies.

1—if continuing or emergency needs qualify.

The results were as follows:

$$(1) \quad R_i^* = -0.942 + 0.177P_i^* + 0.536U_i - 1.60D_{1i} + 2.99D_{2i};$$

$$(3.22) \quad (0.111) \quad (0.579) \quad (1.35) \quad (1.74)$$

$$R^2 = 0.154$$

$$(2) \quad \ln R_i^* = -0.552 + 0.773 \ln P_i^* + 0.200 \ln U_i - 0.209D_{1i} + 0.318D_{2i};$$

$$(0.511) \quad (0.317) \quad (0.456) \quad (0.146) \quad (0.182)$$

$$R^2 = 0.228$$

The standard errors are presented in parentheses below each coefficient.

As is apparent from the results the assistance payment has the right sign in both cases. It is significant at the 12 per cent level in equation (1) and at the 2 per cent level in equation (2). By way of comparison with our original results, we show below the elasticities of welfare recipients with respect to

payments for these two equations (1) and (2) (above) and our previous estimates ([3.21]–[3.24]).

TABLE 1—ELASTICITIES OF WELFARE RECIPIENTS WITH RESPECT TO WELFARE PAYMENTS (1959)

| Equation | Elasticity |
|----------|------------|
| [1] | 0.57 |
| [2] | 0.77 |
| 3.21 | 0.67 |
| 3.22 | 0.53 |
| 3.23 | 0.82 |
| 3.24 | 0.79 |

As is readily discernible from the table, the newly estimated elasticities differ very little from the previous estimates. In addition, the coefficient of the payments variable is much more significant than the coefficient of unemployment in both regressions. This result is also similar to the results obtained in our previous paper. Thus, it appears that in spite of the problem uncovered by Stein and Albin our original results stand. One thing is clear, however, and that is that this area could stand a closer examination than economists have given it.

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Errata

In the article by Richard R. Nelson, "Full Employment Policy and Economic Growth" (*American Economic Review*, December 1966, pp. 1178–92), many (but not all) subscribers' copies contained a number of typographical errors which resulted from the breaking-off of parts of certain characters as the printing of the issue proceeded. Thus the dots over several variables, intended to mark them as time derivatives, were omitted from a number of expressions. The omissions occur in (2), (7), (7a), (10), (11), (12), (12a), (12b), (12c), (13), and (14); in other expressions in footnotes 2 and 8 and on pages 1189 and 1190. In almost all cases, these are easily identified, for the omission of the dots makes the numerator and denominator of certain terms in the expressions identical.

BOOK REVIEWS

General Economics; Methodology

Analytical Economics. By NICHOLAS GEORGESCU-ROEGEN. Cambridge, Mass.: Harvard University Press, 1966. Pp. xvi, 434. \$12.95.

This collection of twelve papers first published between 1935 and 1960 together with a major new essay is a useful addition to current economic literature. It is certainly fitting that the previously scattered works of an important contributor to economic theory, especially in its mathematical formulations, should be assembled for ready access of today's students—both those currently in graduate schools and those whose degrees were awarded some time ago.

The republished essays are presented in Parts II to IV of the volume and are entitled, respectively, "Choice: Utility and Expectation," "Special Topics of Production," and "Economic Development." These are careful, perceptive, and eloquent expositions of important developments in economic thinking combined with frequent original insights and formulations. The author's ability to bring forth revealing thoughts from past literature in many fields while always taking a fresh critical look at the central issue under consideration is a rare gift. Thus Professor Samuelson's accolade, contained in his foreword to the book, "a scholar's scholar, and economist's economist" is well deserved.

While these virtues are still visible in the new essay, this work appears generally less rewarding to the present reviewer. Perhaps this is partly explained by disagreement with the main theme, but whether or not there are sound reasons for disagreeing will have to be judged by each reader. Interested readers should, of course, consult the essay itself since the account given here necessarily reflects a number of the reviewer's opinions and such a brief summary, even if ideally constructed, could convey only a small fraction of the discussion.

After some introductory speculation about the very early origins of science, the author indicates his conception of science and introduces the theme that economics is not a theoretical science. It is explained that theoretical science deals with arithmomorphic concepts (having basic properties similar to real numbers) which cannot encompass the varied phenomena essential to economics. Much of economics requires dialectical concepts which overlap with their opposites and therefore cannot be successfully handled by logic. This development is sketchily illustrated in the quotations below. Extended direct quotations are numbered to facilitate later reference to some of them.

1. It is clear then that the *causa materialis* (again, in the Aristotelian sense) of science is stored communal knowledge, that is, the body of all descriptive propositions available to any member of a community and believed to be true according to the criteria of validity prevailing at the time of reference. To take this equation as a definition of science would be patently inept. On the other hand, we must agree that the equation is valid

for all times and places, from the earliest cultures to those of today. Furthermore, the point disposes of the view that science is the opposite of description. On the contrary, science cannot exist without description. [pp. 3, 4]

2. By logical sorting, all propositions, P_1, P_2, \dots, P_n , already established in any particular field of knowledge can be separated into two classes (α) and (β), such that (1) every β -proposition follows logically from some α -propositions, and (2) no α -proposition follows from some other α -propositions. This logical sorting represents the inner mechanism by which a scientific theory is constructed and maintained. Theoretical science, therefore, is a catalog which lists known propositions in a logical—as distinct from taxonomic or lexicographic—order. In other words, we have a first equation

“Theoretical science” = “Logically ordered description.”

Actually, the logical economy does not always stop here. Often some speculative propositions are “thought up” and added to (α) with a view of shifting as many α -propositions to (β). Thus, (α) is replaced by (ω), the latter having the same properties and the same relation with the new (β) as (α) has had. The only difference is that (ω) contains some unobservable propositions, i.e., some first principles. But this does not affect the validity of the equation written above. [pp. 6, 7]

3. To sum up: *Anatomically*, theoretical science is logically ordered knowledge. A mere catalog of facts, as we say nowadays, is no more science than the materials in a lumber yard are a house. *Physiologically*, it is a continuous secretion of experimental suggestions which are tested and organically integrated into the science’s anatomy. In other words, theoretical science continuously creates new facts from old facts, but its growth is organic, not accretionary. Its anabolism is an extremely complex process which at times may even alter the anatomic structure. We call this process “explanation” even when we cry out “science does not explain anything.” *Teleologically*, theoretical science is an organism in search of new knowledge. [p. 15]

4. In place of “all sciences must imitate mechanics,” the battle cry of the scholarly army is now “no science without theory.” But the change is rather skin deep, for by “theory” is commonly understood a logical file of knowledge as exemplified by geometry and mechanics. [pp. 17-18]

5. . . . we cannot cease to admire men like Jevons and Walras, or numerous others who even in physics hurried to adopt a new viewpoint without first testing their ground. But our admiration for such unique feats does not justify persistence in a direction that trying has proved barren. Nor do we serve the interest of science by glossing over the impossibility of reducing the economic process to mechanical equations. [pp. 18-19]

6. Opposition to Walras’ and Jevons’ claim that “economics, if it is to be a science at all, must be a mathematical science,” has not failed to manifest itself. But, in my opinion, during the ensuing controversies swords have not been crossed over the crucial issue. For I believe that what social sciences, nay, all sciences need is not so much a new Galileo or a new

Newton as a new Aristotle who would prescribe new rules for handling those notions that Logic cannot deal with.

This is not an extravagant vision. For no matter how much we may preen ourselves nowadays upon our latest scientific achievements, the evolution of human thought has not come to a stop. To think that we have even approached the end is either utter arrogance or mortifying pessimism. We cannot therefore write off the possibility of striking one day upon the proper mutant idea that would lead to an anatomy of science capable of thriving equally well in natural as in social sciences. [p. 19]

7. . . . *discrete* distinction constitutes the very essence of Logic: . . . [p. 21] Since any particular real number constitutes the most elementary example of a discretely distinct concept, I propose to call any such concept *arithmomorphic*. Indeed, despite the use of the term "continuum" for the set of all real numbers, within the continuum every real number retains a *distinct individuality* in all respects identical to that of an integer within the sequence of natural numbers. The number π , for instance, is discretely distinct from any other number, be it 3.141592653589793 or 10^{100} . So is the concept of "circle" from "10¹⁰⁰-gon" or from "square," and "electron" from "proton." In Logic "is" and "is not," "belongs" and "does not belong," "some" and "all," too, are *discretely* distinct. [p. 22]

8. At a particular historical moment a nation may be both a "democracy" and a "nondemocracy," just as there is an age when a man is both "young" and "old." Biologists have lately realized that even "life" has no arithmomorphic boundary: there are some crystal-viruses that constitute a penumbra between living and dead matter. Any particular want, as I have argued along well-trodden but abandoned trails, imperceptibility slides into other wants.

It goes without saying that to the category of concepts just illustrated we cannot apply the fundamental law of Logic, the Principle of Contradiction: "B cannot be both A and non-A." On the contrary, we must accept that *in certain instances* at least, "B is both A and non-A" is the case. Since the latter principle is one cornerstone of Hegel's Dialectics, I propose to refer to the concepts that may violate the Principle of Contradiction as *dialectical*. [p. 23]

Another suggested limitation of arithmomorphic theories is that qualities cannot be usefully incorporated. Even if other difficulties were overcome, the sheer number of qualities would be too great.

9. Clearly, there is no reason why the cardinal power of all the qualities we can think of even in a simple set-up should not exceed that of the arithmetical continuum. On the contrary, as I have argued in relation to individual expectations and preferences, there are good reasons for the view that real numbers are not always sufficient for cataloguing a set of qualities. In other words, the manifold of our thoughts differs from the arithmetical continuum not only by its indivisible continuity—as I have explained in some of the preceding sections—but also by its dimensionality. As we say in mathematics, the continuum of the real number system forms only a simple infinity.

The suggestions, natural at this juncture, of using more than one real number, i.e., a vector, for labeling qualities would still not reduce quality to number. For, as set theory teaches us, no matter how many coordinates we add, no set of vectors can transcend simple infinity. [p. 39]

These views are developed in the first two chapters of the new essay. In chapters 3 and 4, "Some Object Lessons from Physics" and "Evolution versus Mechanics," various developments in physical sciences are considered along with scientists' statements on related philosophic issues and some of the author's reactions.

The discussion is interesting and sometimes provocative. I believe many readers will join me in sometimes questioning the author's interpretations, but it does not seem appropriate to try to pursue these issues now. Since I am not at all familiar with some of the contexts and have not deeply studied others my reactions of agreement and disagreement are at this state too casual to mention. In addition, these interpretations do not seem vital to the author's or readers' views toward economics. Once one foreswears, as the author seems to do in quotation 4 above, prior notions about how closely the viewpoints and theoretical apparatus of one science can usefully be copied in another, there is no compulsion to determine one's approach to his own science according to the outcome of discussions elsewhere. Study of other sciences is, of course, still important. Interesting hints, contrasts and parallels may be found. Occasional fruitful inspirations may be had, and it is to be hoped that unifying principles (as between physics and chemistry in recent decades) will sometimes emerge. However, possible relevance to another field must be critically examined.

The final chapter, "General Conclusions for the Economist," continues various threads of discussions introduced earlier. Not all of the conclusions are startling. For example, a discussion of possible significance for economics of the study of entropy concludes—

10. It is thus seen that we cannot arrive at a completely intelligible description of the economic process as long as we limit ourselves to purely physical concepts. Without the concepts of *purposive activity* and *enjoyment of life* we cannot be in the economic world. And neither of these concepts corresponds to an attribute of elementary matter or is expressible in terms of physical variables. [p. 98]

After a section on the boundaries of economics, the nature of theoretical science and the disqualifications of economics are again considered.

11. *Why Is Economics Not a Theoretical Science?* Everyone uses "theory" in multifarious senses. To wit, in one place Schumpeter uses it to mean "a box of analytical tools." But in discriminating usage, the term generally denotes a logical edifice. Or, as I have explicitly put it (Chapter One, section 4, above), theory means a logical filing of *all* extant knowledge in some particular domain such that every known proposition be either contained in the logical foundation or deducible from it. That such a filing has the unique merit of affording *comprehensibility* is a leitmotif inherited from Aristotle. However, hardly any attention has been paid to the fact that there can be no comprehensibility without the *compressibility* of extant knowledge in only a relatively few ω -propositions. If our knowledge

of a certain domain is not compressible, i.e., if its logical filing results in a very great number of ω -propositions, Aristotelian comprehensibility does not obtain. I have illustrated this point in connection with chemistry where, because of the frequency of novelty by combination, any logical foundation must contain far more numerous propositions than the β -class. For the same reason a logical foundation of chemistry would have to be continuously "under construction." A chemical theory, clearly, would serve no purpose whatsoever. The same applies with even greater force for any science concerned with evolution, for the scene of evolution is dominated by novelty. [p. 108]

The principal argument elaborated in the remainder of the section is that economic theory as we know it applies to a particular institutional setting and therefore does not have the required comprehensibility, it is not a filing of all extant knowledge. The remainder of the paper stresses the view that a critical difference between the social and natural sciences lies in the purposes served by arithmomorphic models in the two areas. Models serve didactic purposes in both areas, but in physical sciences a model is also a calculating device or accurate blueprint whereas in social science a model is best regarded as an analytical simile.

12. To illustrate now the difference between blueprint and simile, let me observe that one does not need to know electronics in order to assemble a radio apparatus he has purchased in kit form. All he needs to do is follow automatically the accompanying blueprint, which constitutes an *operational* representation by symbols of the corresponding mechanism. The fact that no economic model proper can serve as a guide to *automatic action* for the uninitiated, or even for a consummate economist, necessitates no special demonstration. Everyone is familiar with the dissatisfaction the average board member voices after each conference where some economic consultant has presented his "silly theory." Many graduate students too feel greatly frustrated to discover that, in spite of all they have heard, economics cannot supply them with a manual of banking, planning, taxation, and so forth. An economic model, being only a simile, can be a guide only for the initiated who has acquired an analytical insight through some laborious training. Economic excellence cannot dispense with "delicacy and sensitivity of touch"—call it art, if you wish. And it is only too bad if at times the economist lets himself be surpassed in this respect by the layman. The widespread view that the economist's role is to analyze alternative policies, whereas their adoption is the art of statesmanship, is no excuse. An artless analysis cannot subserve an art. [p. 117]

Although I find much that is instructive in Professor Georgescu-Roegen's discussions of particular problems and developments, I find myself in frequent disagreement with his conclusions and broad interpretations. This can in part be traced to my not accepting his rigid Aristotelian interpretation of theoretical science (quotations 11, 2, 3). Is a science not theoretical if it (temporarily we hope) has to employ several logical structures to interpret relevant observed phenomena? Is it not theoretical if its logical foundation is continuously "under construction"? Is there now or likely to be any science which really meets the author's criteria? Note that he refers several times to the current

need for a corpuscular and a wave theory in physics and, on page 30, lists important changes in physical theories.

Granted that economic theory as developed in Western industrialized countries has aspects which do not apply in other institutional settings, is this problem basically different from that of the applicability of some physical theories to other galaxies? Is this kind of lack of comprehensiveness really a disqualification in either domain?

While the role of theory in permitting a scientist to deduce new implications (tentative hypotheses whose empirical refutation would lead to a need for some theoretical reformulations) is not neglected in the new essay (e.g., the latter part of quotation 2), I believe it is underemphasized relative to the role of theory as a compact store of established knowledge. Such results as Slutsky's equation and Hicksian relations among cross elasticities follow readily from Walrasian models but apparently did not appear until such models had been analyzed for some time.

The author's use of the term, arithmomorphic, is unclear and troublesome. Statements like ". . . within the continuum every real number retains a *distinct individuality* in all respects identical to that of an integer within the sequence of natural numbers" (p. 22) and "Whatever properties the arithmetical continuum may have, its structure is still that of beads on a string, *without the string*" (p. 33), are at best confusing and if interpreted literally are false. Quotation 7 reads as though a concept is arithmomorphic if it is distinct from other concepts (among the items which are unclear is whether or not discretely distinct means anything different from distinct). If this is the intention, arithmomorphic just means well defined and there is no more reason to associate the concept with the real numbers than with any other set since nonidentical elements of any set are distinct.

In other passages (e.g., quotation 9) the term is used as though it implies other properties of the real continuum as well. If this is the intention, then one should not assume that theories need to be based entirely on arithmomorphic concepts. Mathematics provides many possible structures with varying properties in terms of which theories might be formulated. Real numbers have been predominately used (though not exclusively, e.g., integers, complex numbers, vector spaces) because, fortunately, many empirical phenomena reflect the properties of real numbers to a satisfactory approximation and because, unfortunately, many scientists are only acquainted with real numbers and a few closely related structures.

The problem of choosing appropriate structures can be illustrated by pursuing the example of quotation 9 a little further. Georgescu-Roegen notes, as seems reasonable, that in some contexts the number of qualities that an economist needs to represent may exceed the number of real numbers available, i.e., the cardinality of the continuum which he calls simple infinity. He considers the possibility of using vectors to represent the qualities and concludes, "no matter how many vectors we add, no set of vectors can transcend simple infinity." The conclusion is true only if we are restricted to vector spaces of countable dimensionality.

Consider the set of all real-valued functions defined on the unit interval.

The cardinality of this set is greater than that of the continuum and the set is a vector space. Moreover it can readily be made a metric space (also a Banach space) so a large array of mathematical results are at hand to help analyze any phenomena that can usefully be represented as vectors in such a space.

This of course, does not mean that the space is necessarily a good one to serve as a space of qualities. To judge its appropriateness one would first have to bring sufficient economic considerations to bear to say what properties are needed or desirable in a space of qualities. If no previously studied mathematical structure provided a suitable framework, the possibility of developing one could be explored. Probability theory is a good example of a branch of mathematics whose development has been partly guided and greatly stimulated by the desire to have a suitable mathematical foundation for interpreting a specific class of empirical phenomena. Of course, it may be some time before economists understand qualities well enough to determine the kind of mathematical structure needed.

Trying to develop new theories involving, when it seems promising, hitherto unutilized mathematical structures is an endeavor quite different from trying to "prescribe new rules for handling those notions that Logic cannot deal with" (quotations 6 and 8). I cannot see that the author has developed a strong case for the latter. The notions that logic cannot deal with are incompletely defined notions. We cannot do without these and ordinary parlance involves a large fraction of such terms. But the cost of including poorly defined terms in theories is confusion.

The cost of casting theories in well-defined terms is that theory represents a highly idealized reality. Choosing the aspects of reality to be reflected with some accuracy and those to be grossly simplified is a vital choice which demands all the "delicacy and sensitivity of touch" (quotation 12) that can be mustered. The latter qualities are also crucial for applying theories to a complex and only partly understood world.

In the various respects discussed above it seems to me that the positions of theories in natural sciences and in social sciences are much more alike than different. Nor can I agree that these sciences differ greatly in ability to furnish blueprints (see quotation 12). Economics engineers have furnished blueprints for such decisions as where to borrow money and how to choose an economical ration for animals. I suspect a not altogether useless blueprint for national economic stabilization policy could also be constructed. It would not work satisfactorily in all circumstances but neither does a blueprint for a radio installation. For a particularly important or novel installation scientists are usually consulted as scientists are consulted for important aspects of economic stabilization. Disagreements among economic scientists are more widely publicized but disagreements among physical scientists on both abstract and practical levels certainly exist. It would be fun to have information for making a meaningful comparison.

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The Structure of Economic Science: Essays on Methodology. Edited by SHERMAN ROY KRUPP. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. vi, 282. \$7.95.

Inevitably, sixteen original essays on methodology will reflect sixteen different positions—and at least as many terminologies. I may perhaps be forgiven for interpreting them along a single broad continuum and for imposing a common (in both senses of the term) vocabulary, if it is understood in advance that violence will be done to all. The center position on the continuum proceeds by analogy to physical science: constructs are distinguished from brute reality; they are related to it by rules of correspondence; they are related among themselves in a deductive system providing inferences which are certain; ever greater generality is sought through ever higher degrees of abstraction; consistency and ability to predict are the criteria of confirmation and explanatory power. These principles are graphically reviewed by the physicist-philosopher Henry Margenau. He also defines an inductive approach, which relates empirical variables and arrives at probabilistic conclusions of limited applicability and explanatory power.

The former, "scientific" approach identifies the center of the continuum and the latter, the empiricist end. The distinction reflects much of recent controversy in economics such as the discussion emanating from the first chapter of Milton Friedman's *Essays in Positive Economics*. This, as well as several other controversies, are usefully unravelled by Martin Bronfenbrenner in an introductory review essay. He is permissive to the point of reducing significant differences of viewpoint to a matter of subjective preference. This will satisfy few, and rightly so. Fritz Machlup's paper is a specific, unrelenting attack on empiricism. Eugene Rotwein, by no means an extreme empiricist, recognizing that highly abstract systems may provide insights and first approximations and that even "descriptive" propositions represent a degree of abstraction, in the end requires that empirical verification apply to each concept directly, which is the heart of the matter. These authors and their allies have developed their basic positions elsewhere, and few readers will be further swayed by the elaborations here. I am persuaded that, as in previous confrontations, the advantage is with the centrists. Be that as it may, the latter are not home free. The strength of this collection is that it probes the other part of the continuum. This can be loosely defined as extending out from the center as empirical confirmation is held to be even less direct, possible, or passive. To that extent, theory is thought to be characterized by a greater degree of contingency, and ultimately relativism may emerge.

It is customary for the centrist position to emphasize, as do Machlup and Bronfenbrenner here, the distinction between logical and empirical validity of a theory. A "truth" value is imputed to models so long as they are consistent with their axioms. Applicability is another but a separate criterion, and, as in physical science, it is guided by the device of *ceteris paribus*. However, in physical science, when the *ceteris paribus* conditions are ultimately cancelled out, the various parts are brought together into general principles which are formidable in their scope as well as in their consistency. In this precise sense the whole possesses greater validity than its parts. Emile Grunberg is sensitive

to the plight in economics, although he remains committed to noncontingent relationships. He shows, in effect, that it is not sufficient to assert the magic words, *ceteris paribus*. Laws must "make explicit mention of all the conditions under which a particular effect is expected to occur." Because of the indefinite number of relevant variables, hypotheses in economics are subject to an "unspecified" *ceteris paribus* condition. The consequence is an "open system" in which hypotheses are not disconfirmable since failure of prediction may be due either to false hypotheses or to the failure of the values of some of the "givens" to remain in fact constant. K. E. Boulding, in his discussion of verifiability, makes the related point that random elements render the parameters of an economic system relatively unstable and thereby frustrate prediction. He believes that this may impose ultimate limits on knowledge of society but that there is a long way to go, and his own interdisciplinary labors represent one kind of attempt to firm up the parameters.

The rationale for much, although not all, of the crossing into other disciplines is sharper specification of the *ceteris paribus* conditions by expanding the scope of the theory. An alternative or additional strategy is to seek closure of the system by introducing a principle of selection which will restrict variables and relationships to those judged relevant, but which will salvage some kind of general status for the theory. (Closure as has been indicated, has a meaning more substantive than merely formal determinacy.) S. R. Krupp's typology of controversies in economics shows that such discrimination—based on time runs, problem and policy orientations, differentiated socio-economic systems, values—may be the ultimate source of conflict. Lawrence Nabers suggests that the history of economic thought, as a branch of the sociology of knowledge, needs to take such selective orientations into account in its interpretations.

Attempts to close the system by value judgments have a long history. Typically, they have taken the form of applying such judgments to individual preference schemes. Although Jerome Rothenberg marshalls the strongest possible case for a positive theory, free of value judgments, he allows for a separate normative economics. He shows that the poverty of the Paretian version has provoked a variety of formulations which are more radical from the standpoint of centrist methodology. His own work on the possibility of empirically establishing value consensus in a given society illustrates one such approach towards closure of a system so that it can provide more general results.

A philosophical underpinning for the view that statements about the mental states of others (conceived as preferences and not pleasures) are empirically confirmable is provided by R. B. Brandt. He is unclear as to how much of the analysis applies to interpersonal comparisons, but a large part does, and is a useful antidote to exaggerated purism. Although precise measurement of utility involves familiar unresolved difficulties, the ability of even ordinary language to convey empathy in some degree suggests that the problem is not one of principle. Acquiescence in Brandt's further contention that statements about individuals' preferences constitute value judgments which are confirmable by the methods of empirical science depends on one's taste for analytic philosophy. A different philosophical argument is employed by C. W.

Churchman to the effect that measurement and interpersonal comparison of utility must be possible because the "criteria of objective evidence of wants," i.e., of measurement, are created through a social process. Preferences are not merely internal givens; they are "ascribed." The presentation is terse but amounts to a devastating polemic against solipcism concerning intensities of preference.

Useful surveys dealing with method from the standpoint of mathematical economics and econometrics by W. J. Baumol and by Gerhard Tintner, special problems of aggregation for micro-macro consistency by Kelvin Lancaster, and linkages between economics and other disciplines by J. M. Buchanan and by Benjamin Ward round out the volume. The collection as a whole represents a unique dialogue on methodology, in terms of coverage of topics, variety of viewpoints, and qualifications of participants. It summarizes as well as contributes to the glacial advance of clarification in this area. Both the general economist and the addict will be served.

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Modern Economics: An Introduction. By LORIE TARSHIS. Boston: Houghton Mifflin, 1967. Pp. xvi, 814. \$8.95.

Exactly 20 years after publication of his pathbreaking introductory text, *The Elements of Economics* (Boston, 1947) emphasizing national income and employment analysis, Professor Tarshis now provides a new textbook for the beginning course. Or if not entirely new, then at least a modern revision of his earlier work.

Modern Economics is a competent and conventional book by a respected economist, and it will surely find an appreciative audience of students and professors. It is not, however, another pathbreaker; nor does it claim to be. The book is traditional in approach, treatment, and style. "Alas, a moment's reflection [discloses that our society] has not managed to banish economic want and do away with scarcity," p. 59; "The business firm in our economy embraces both the little boy who shines shoes" [and AT&T, p. 45]. It is a historical, not highly mathematical, not more difficult nor less readable than average, has the standard 800 pages, and contains idiosyncracies of style and notation that will prove inconvenient to experienced teachers who dislike molding their course entirely to a textbook (e.g., pp. 13ff, classification of productive factors; p. 431, GNP multiplier represented by $M = 1/1-R$; p. 313, inelastic demand curve for money). In Tarshis' own view, it is one more contribution to the "proliferation of brands" in the textbook market, offering "some things that are new and others that are at least put in new ways." One can anticipate that the availability of this book may make the selection of a "Principles" text harder for some teachers of the course, though it won't make the task easier for many.

Modern Economics is divided into seven sections, plus a very brief Preface and Epilogue (in which students are comforted at journey's end with the author's guess that "as long as we continue trying to keep the economy competitive and to improve budgeting procedures within firms and government,

we can be reasonably content with the patterns of the [economy's] output." This organization reflects emphasis on four economic questions: (1) *How much output* will the economy produce? (national income analysis—33 per cent of the book); (2) *What kinds of output?* (microeconomic theory, with heavy emphasis on the business firm—27 per cent); (3) *To whom will the output go?* (distribution, mostly functional—8 per cent); and (4) *How much output will the economy produce over time?* (growth—7 per cent). The other three sections provide an Overview of the U.S. Economy (7 per cent) and treat the separate topics of Money and Banking (6 per cent) and International Economic Relations (11 per cent).

Section One opens with a discursive Introduction that seems longer than its nine pages, owing to an unattractive typography, only the first of several unfortunate mechanical features of the book—including pastel subheadings, poor indexing, superfluous digits and ciphers in statistical tables (pp. 34, 690), some inept graphics (pp. 22, 401), and inconvenient numbering of chapters (by individual section rather than 1-42 for the entire book). The first three chapters provide a sketch of the U.S. economy and the business firm, with half a dozen pages on government.

Section Two contains 12 chapters on standard microeconomics with industry applications. The treatment is relatively thorough (no indifference curves), highly skilled, and in places novel (Ch. II-3, "Supply Under Strong Competition" and Ch. II-7, "Mark-up Pricing and Marginal Pricing").

Section Three concisely treats the financial sector, with some emphasis on near-money and non-bank financial institutions. Section Four consists of 13 chapters on "The Size of the Nation's Output" with special chapters on Inflation, Debt, and Automation. Section Five includes income distribution, with attention devoted almost exclusively to functional distribution (since this "is what primarily interests the economist") and labor market structure. Tarshis dismisses the need for any special treatment of poverty, pointing out (p. xi) that "after all, what is economics about if it is not about poverty?" Section Six deals with "the incredible chaos of opinion" on growth, both for the "have-not nations" (poverty does receive some attention here) and the advanced economies, drawing heavily on Denison's findings for the United States. The final section provides a thoughtful treatment of "The United States Economy in the World Context," with sharp focus on balance-of-payments accounting procedures and on policies for restoring equilibrium.

It would appear that the opportunity cost (one of the many concepts missing from the index and neglected in the text) of the author's expanded treatment of the international sector is a regrettable neglect of income distribution and public finance. In speculating on what it is that students will need to know 5 or 10 or 15 years hence in order to keep up with current economic events, we might well agree with Tarshis that international economics clearly demands greater attention. On the other hand poverty, resource development, and the public sector are not likely to shrink in importance in the coming decade, and widespread public understanding in these areas is still sorely lacking.

One final note: Although Tarshis demonstrates awareness of and sensitivity to the subject of values and policy, he nevertheless avoids any direct confron-

tation with the problem of valuing. This may prove disappointing to serious students, who are (in the author's words) "full of curiosity and energy, skeptical, able, and concerned." Increasingly today's students are demanding that economics, along with other social sciences, provide insight and analytical skills that are immediately relevant and operationally useful in seeking out solutions to real socio-economic problems, like poverty and urban decay. They are impatient with the familiar position reiterated (pp. 595f.) by Tarshis: "Luckily for the economist, the boundaries of his subject are such that these [value] questions are left to others . . . [His] concern must be to serve neither as an apologist for nor as a critic of 'the system'; these are different roles to be played perhaps by a lawyer, or a politician, or a journalist." Idealistically, today's students look to economics and social science if not for answers then for methods of finding answers, based on disciplined reasoning and hard evidence, in place of unsatisfactory methods and economic-policy judgments of the lawyers, politicians, and journalists.

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Growth and the Economy: Principles of Economics. By DAVID MCCORD WRIGHT. New York: Charles Scribner's Sons, 1954. Pp. xii, 398. Paper, \$5.25.

Professor Wright's Principles text is less than half the length of the heavy volumes which dominate the market. Brevity has been achieved not only at the expense of much historical and institutional information but also at the sacrifice of much of the principles of economic analysis generally demanded of students in introductory courses nowadays. For this reason the book can hardly serve for the standard two-term course. It does not provide the hard theoretical skeleton which could be fleshed out with supplementary readings.

The book is extremely uneven both with regard to quality of analysis and of style. Its theme, from the first chapter to the last, is that "growth comes through change and causes change. As output expands, the pattern of wants constantly changes, and also the pattern of production." In expounding his views on the prerequisites for maintaining a social system's capacity to generate growth and adapt to change the author is often interesting as well as provocative and sometimes eloquent. But the presentation of standard static price theory (Chs. 6-11) and of income-expenditure theory (Chs. 19, 20) is perfunctory. Here the theme of ever-ongoing change takes the form of repetitive reminders that the propositions derived are "theoretical" and that the *ceteris paribus* assumptions seldom hold. In addition, the author is satisfied with giving only a "general idea" of many important concepts and functional relationships. As a consequence, the student can hardly help but be left with the feeling that in economic analysis "anything may happen."

Production methods must change with growth "for there is (a) technical expression, 'the law of diminishing returns,' which says that in an unchanged technical environment growth becomes progressively more difficult" (p. 10). The proposition that "the pattern of want will change as output changes," the author seeks to base on differences in the price elasticities of demand for

different goods, while openly admitting that the argument "violates the *ceteris paribus* assumption of the individual demand curve" (p. 69). Getting into immediate difficulties, he then leaves the student to his own devices: "a very little imagination will show that some consumption experts, or market analysts, will use three-dimensional calculations of income, price, and sales change, but we do not enter on such complications here." In this case, the student's imagination has little to work with since the book does not introduce him to indifference curve analysis. The first three parts (143 pages) contain many other exceedingly loose formulations, e.g., on the Law of Variable Proportions (pp. 79-80), on the relationships between Marginal and Average Product (p. 81) and between Supply Curves and Cost Curves (pp. 87-88), and on the definition of the Marginal Efficiency of Capital (p. 105). There are few diagrams and algebra is shunned altogether. In its stead, the term "proportional" does heavy duty, being used to denote equality of ratios (p. 62), elasticity (p. 66), and partial derivative (p. 80).

The five chapters on National Income Measurements and Money, Banking and Credit Control generally do a creditable job within the short space allotted, although the first of them confuses matters by twice referring to the basic accounting convention as the "equality of prices and incomes." The pride of place given to the idea of ever-ongoing change next dictates that business cycle theories be discussed before the student has been introduced to the theory of income determination. In Chapter 17, Professor Wright has his heart in the work—it deals exclusively with "over-investment" and is probably the best in the book. Remaining cycle-theories are given one chapter and here the author has to discuss the interaction of multiplier and accelerator while the reader has two chapters to go before encountering the explanation of the multiplier. Even one who shares some of the author's skepticism of the simple income-expenditure model must find his 27-page sketch entirely too cursory. From the proposition that "we have full employment when the 'inducement to invest' and the 'propensity to consume,' taken together, add up to full employment" (p. 222), the student is taken post-haste to the implicit advice to forget the whole thing: in the real world, the multiplier "is almost wholly unpredictable"; the marginal propensity to consume "might rise or it might fall" (p. 241).

The 100-page Part IV has three chapters on International Economics, two on Government and Business, two on Unions, and one on Comparative Systems. The first five of these, in particular, are generally well written, but annoying signs of questionable craftsmanship continue to occur. (Why does the Bibliography advise the student interested in International Finance and Trade Policy to consult *International Economic Papers*, 1951 to date?) In the concluding chapter on Stabilization Policy, a variety of policy measures to combat inflation or deflation is catalogued, but there are arguments both for and against all of them, and the discussion is totally inconclusive. Three times within as many pages (pp. 354-56), the author promises to come back to a fuller appraisal later, but, in fact, the issues in question never reappear.

Teachers of introductory courses have to struggle with the tendency of many students to quit when they "feel" they understand the readings "in a

general sort of way." In this struggle, Professor Wright has all too often taken the side of the students.

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**Price and Allocation Theory; Income and Employment Theory;
Related Empirical Studies: History of Economic Thought**

Industrial Production Models—A Theoretical Study. By SVEN DANØ. New York: Springer-Verlag, 1966. Pp. viii, 220. \$9.75.

The development of mathematical programming in the postwar period exercised a strong influence on the theory of production. For the first time, systematic treatment of cases involving discontinuous factor substitution became possible, and the way was opened for reinterpretation of the traditional neo-classical model. Extensive discussion in the literature has made the new themes familiar, but Professor Danø's excellent book is the first text to provide a really comprehensive and integrated exposition of the separate strands of thought on production models. By examining the formal properties of the leading types of production functions used in microeconomic theory, and contrasting the differences among them, Danø is able to give valuable perspective to the field. What emerges is a clear statement of how the older marginal analysis of the firm compares with that of linear programming, and how the modern Kuhn-Tucker generalization of marginalism relates to both these basic cases.

Because mathematical interest is on constrained systems, coverage of the book is limited to short-run analysis. The standard setting envisioned for production is a given manufacturing plant in which variable factors cooperate with fixed capital equipment. All the models considered are timeless, or mono-periodic, and no attempt is made to deal with the problems of investment, replacement, production scheduling, inventory management, or similar difficulties associated with the operation of an ongoing productive unit. Still further simplification is obtained by assuming the existence of a competitive universe; each firm is taken to be a short-run profit maximizer shaping its actions in the light of specified market prices for inputs and outputs. Of course, this specialized framework is adopted merely as a matter of convenience, and has good justification. The book's primary purpose is to explain alternate formulations of the production function, not provide a full-fledged theory of the firm. Thus, the derivation of optimality conditions for firms facing different market structures and following complex behavioral patterns can legitimately be left to the reader.

Within the limits imposed by assumptions, the discussion shows sensitivity in relating the various abstract models to the actual circumstances of production. There is no tendency to deal lightly with fundamental concepts and definitions, and then hurry on to the purely formal development of the models. Rather, a nice balance exists between economic interpretation and mathematical exposition. At times, one might wish for somewhat more information on

the state of empirical research so that a feeling for the probable "shapes" of the functions could be obtained. For example, it would be interesting to know what elasticities of substitution can be expected for isoquants derived from an individual technical process, or what internal structure a multi-variable production function tends to possess, and so on. However these are questions to which unequivocal answers are not easily forthcoming, and Danø is only conforming to a great tradition by not giving too much attention to such factual matters in a theoretical treatise.

To begin the systematic study of how different technological restrictions influence the possibilities for economic choice, Danø introduces the elementary case of fixed proportions. This simple construction affords a convenient way to approach the concept of shadow pricing and leads logically to the linear programming model of the firm. Discussion of the latter is quite adequate, but sticks to essentials and is conducted largely in terms of numerical examples rather than matrix algebra. Supplementing the work of the chapter is a useful appendix on the simplex criterion. In general, the material offered is lucid and readable without sacrificing substance to ease of presentation.

Chapters IV-VII contain a polished statement of marginalist theory—in its traditional and modern forms. Explanation of the respective single-product models proceeds with deliberation and few, if any, topics of importance escape notice. Emphasis here is, of course, on cases which assume inputs are smoothly substitutable for one another in production; however, the functions studied include both those where all inputs are mutual substitutes, and those where some inputs are substitutes and others are not (i.e., product and factor "shadows"). Standard optimization problems are examined in relation to the different types of production models presented. Of particular interest is the contrast drawn between the conventional neoclassical model, based on indivisibility of the fixed factor, and the newer approach which recognizes non-negativity requirements for the inputs and the possibility of variable utilization of plant capacity. Quite apart from economic arguments, the formal discussion makes clear that once the procedures are established for dealing with problems of maximization (or minimization) subject to inequality constraints, it is a natural step to question the indivisibility assumption and the U shaped marginal cost curve. In this area, as elsewhere, the commentary of the chapter is strengthened by detailed appendices covering the essential mathematics of constrained systems and homogeneous production functions.

The remaining chapters extend the basic theory to take account of such things as: variation in product quality, the relationship of individual process functions to the plant production function and, finally, multi-product models. Many of the concepts here are especially useful because they relate to areas of production research which have received too little attention in the past. Exposition conforms to the general standards of the book; the presentation is rigorous and tightly organized. Mathematical treatment, which involves elementary calculus, is invariably deft so that even familiar demonstrations take on new interest.

It is difficult to be critical of Professor Danø's perceptive survey of short-run production theory. The work is obviously of high quality and meets the

need for an authoritative text outlining the modern position. Of course, one can disagree with the book on matters of emphasis, or question the omission of particular topics; for example, it seems regrettable that there is no mention of learning and the adaptive production function. However, these problems are essentially minor. The one important criticism that can be made concerns the book's failure to explain, at least in general terms, the relationship between a static model and a dynamic economic system. Preoccupation with rigidly circumscribed short-run analysis means that the theory of production is wholly separated from the theory of investment—yet adequate understanding of instantaneous production decisions almost always depends on knowledge of the long-term optimization plan.

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Théorie générale du capital, de la croissance, et des fluctuations. By A. COTTA. Paris: Dunod, 1967. Pp. 430.

In this review I would like to call to the attention of American readers a novel and most interesting work just published in Paris by one of the ablest of the new generation of French economists. Cotta attempts to formulate a general theory of economic fluctuation and economic growth. In so doing he departs, and I think goes beyond, the Keynesian paradigm by focusing in a particular way on the role of "capital." What Cotta means by "capital" is, however, a world removed from the Böhm-Bawerkian notion or from the National Bureau datum. Cotta, rather, is concerned to explore the relevancies of virtually every form of real or financial stock and accumulation, including the funds and securities representing financial "wealth," including all the goods in store and inventory and in personal possession intended for private consumption, including tools and materials durable and nondurable, to be used in the processes of production, and finally (and especially) including the accumulation of "knowledge." These accumulations are understood, and are carefully and elegantly analyzed both as the primary objectives of economic endeavor and as the major constraints upon economic activity.

Cotta's book deserves to be quickly translated and made available in English. One hopes that it soon will be. Meantime, it should not be neglected by those who have the competence to study it in the original.

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Microeconomic Theory. By C. E. FERGUSON. Homewood, Ill.: Richard Irwin, 1966. Pp. xiv, 439. \$8.00.

In addition to the traditional treatment of consumption, production, the firm, and factor markets, Professor Ferguson includes chapters on general equilibrium theory and welfare economics. To me, this is a desirable extension of intermediate price theory. Other innovations are a brief discussion of game theory and technical change, an early presentation of Edgeworth boxes, and some comments upon recent empirical studies in microeconomic theory. Expla-

nations are generally lucid, and many interesting exercises appear at the end of chapters and in an appendix [by] Fritz Machlup. The references are extensive although not always up to date, and on the whole the presentation is of a scholarly nature.

With regard to particular topics, I like the treatment of the following: (1) The Sweezy theory of oligopoly is exposed as vacuous (pp. 277-78). (2) There is a good discussion of the dependence of factor demands on the own factor price and the own product price. I hope that future editions will incorporate the analysis of the dependence of factor demand on the other factor prices [7, Ch. 6] [8] [10] [15]. Also, Ferguson uses the property that an increase in the use of one factor increases the productivity of another. This is not obviously derivable from other properties (e.g., diminishing returns) and often must be treated as an axiom [2]. (3) There is a substantial section on cartels and price leadership (pp. 286-94). (4) There is no sign of capitulation to non-neoclassical theories of the firm, such as "satisficing." (5) The survey of utility theory is a good one (pp. 11-24), although an exposition of Occam's razor might have made clearer the difference between cardinalists like Jeremy Bentham and "quasi-cardinalists" like every economist who maximizes a utility function.

Unfortunately, the book suffers from several flaws. There are both sins of omission and sins of commission. Many of these are shared by most other intermediate (and elementary) textbooks, but this makes them no less annoying. Some appear below.

1. As in most texts, with the notable exception of Stigler [18], we are told that whenever the long-run average cost curve is U shaped, the long run equilibrium of the industry is at the minimum point of that U (pp. 213-14). But we are reminded that this is not so if the long-run average costs are increasing over the whole range of output (p. 217). Evidently, as the bottom of the U tends to the zero output, long-run industry output tends to zero, but once the limit is reached, the industry is released from the bounds of long-run equilibrium. Are we to believe that an industry with the slightest indivisibility so as to have a U shaped cost curve with a minimum at say $1/1,000,000$ of a milligram is relegated to that output in the long run?

The source of the difficulty is easy to discover. The assumption is made that new firms will enter until they drive the profits down to zero. Evidently, there is a virtually infinite source of entrepreneurial talent. This analysis is objectionable on economic grounds. First, if knowledge is in fact unlimited, as in the case of an unlimited supply of entrepreneurs, what then is the source of diminishing returns to the industry? Why should average cost curves be U shaped to begin with? (Here, one is involved in Clapham's empty boxes [13] [17] [5].) Second, one would expect per capita output to increase over time. The entrepreneurial role is played by people and, therefore, the ratio of output to entrepreneurial talent grows steadily. Entrepreneurship becomes a scarce "factor" and requires a rent, namely, a positive profit from firms. The only reason this factor is not treated like others is that knowledge is attached to indivisible units, namely people.

2. We are told that the going market price is the one where supply equals

demand (Ch. 9). As a matter of fact, a market is not always in equilibrium and the tendency of price to equal the equilibrium price is dependent upon a dynamic adjustment system (Samuelson [16, p. 68, pp. 480-86]). If an equilibrium price is not stable for the dynamic system, then that price does not represent the going price.

3. Although a long-run theory of cost appears, no mention is made of the very significant costs of construction and organization of new plant and equipment, which limits the rate of expansion of the firm. Ferguson's approach is the traditional one of Viner (pp. 175-84), which assumes that set-up costs will not change over time so that the firm expands at the maximum rate possible. To the contrary, a major part of the investment decision of firms is concerned with the pace of expansion (Eisner and Strotz [4]). The solution to such problems determines the set-up costs incurred and therefore the level of a long-run marginal cost curve. Possible analytical techniques to use are dynamic programming and the calculus of variations.

4. Ferguson's presentation of welfare economics is limited to the Bergson approach. He does not enumerate the fundamental classical theorems (Arrow [1] and Debreu [3]): (a) A competitive equilibrium is Pareto optimal, and (b) Any Pareto optimum can be attained by a competitive equilibrium with an appropriate redistribution of wealth. Incidentally, these are easily illustrated by use of the Edgeworth box (Newman [12]).

5. Similarly, his treatment of general equilibrium theory suffers from a lack of reference to modern developments.

No longer is the number of equations said to be sufficient to insure the existence of equilibrium, but the distinct impression is left that it is necessary (pp. 361-62). This is not the case: linear equation systems with n unknowns and m equations $n > m$, normally have solutions, provided the equations are linearly independent. Even if the number of equations exceeds the number of unknowns, a system of linear equations may have solutions. What can be shown is that if the number of variables equals the number of equations, if the matrix of partial derivatives has linearly independent rows, and if one equilibrium exists, then there are no others nearby; the equilibrium, if it exists, is "locally" unique (up to a multiplicative constant). Again, the existence of competitive equilibrium can be constructed for the Edgeworth box. Ferguson's figure 4.2.2 (p. 63) is all that is needed for the purpose.¹

6. In his treatment of production, he states that the firm will never choose to produce in region I, where marginal costs are falling (pp. 121-22). He does not warn the reader that he intends only to say that the competitive firm will never choose such a production. This becomes clear only later when a counter example is illustrated for a monopolist (figure 104.7(b), p. 239). A more substantial objection is that the reader is told that a U shaped cost curve is com-

¹First, observe that, from an initial position, a line segment to the upper right-hand origin is below the line tangent to the indifference curve through the origin. Second, the line segment and the tangent lines vary continuously along the contract curve to the lower left-hand origin, where the line segment lies above the tangent line. Somewhere between, the line segment coincides with the tangent line. This gives a competitive consumption; the price system is derived by taking the coordinates of a vector tangent to the tangent line.

patible with pure competition without being warned that in this case, competitive equilibrium may not exist, even at a zero level of output [9, pp. 49-50] and [14, pp. 170-72].

7. In his presentation of elasticity of market demand (pp. 77-89), Ferguson omits the fact that market demand elasticity is the weighted average of individual consumer demand elasticities [18, pp. 42-43].

8. Ferguson ignores the Nash [11] and Zeuthen (Harsanyi [6]) approaches to bargaining games, whereupon he states unequivocally that the consensus on game theory is that it is "more relevant to the study of specific business problems than to general theory" (pp. 278-79).

9. Also, Ferguson declares that the indeterminacy of bilateral monopoly is due to the fact that economics does not contain all the aspects of bargaining skills and personal characteristics necessary to determine the outcome (p. 248). Equally well, it could be argued that the problem is simply not well defined, and if various kinds of bilateral monopoly were specified, then a solution could be found. In my opinion, the Nash solution applies to one of these varieties, namely where bargaining leads to an enforceable contract.

10. Ferguson writes that "in almost all cases the income effect is positive" (p. 49). Such a statement requires empirical reference and/or logical justification.

11. There appears an uncritical presentation of monopolistic competition (Ch. 11). An alternative viewpoint is given by Stigler [19].

12. After an extensive discussion of the distinction between social and private costs (pp. 163-65), Ferguson might have pointed out that under some circumstances they are equal (e.g., competitive equilibrium with the "proper" income distribution under convex preferences and production possibilities).

I would recommend the text for classroom use if and only if the teacher supplements the text to make these reservations.

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Macroeconomic Analysis. By E. SHAPIRO. New York: Harcourt, Brace and World, 1966. Pp. xx, 618. \$8.95.

Macroeconomic Analysis is an intermediate undergraduate text that utilizes the orthodox Keynesian "elasticity pessimistic" models complete with liquidity trap as the vehicles for discussion of aggregate income determination.

The menu is rather standard fare. With a few transpositions of chapter headings, the table of contents could easily be mistaken for that of Ackley or Dernburg and McDougall. The relative allocation of space to subject matter is about the same as that in Dernburg and McDougall with one exception. Shapiro devotes a relatively large number of pages—124—to National Income Accounting. He works up from a two-sector through a four-sector economy before discussing the U.S. accounts. Whether this is good or bad depends upon one's point of view. I do not happen to find the rules of income accounting particularly fascinating, and would prefer merely to have them stated so they can be memorized if necessary and ignored if not. Shapiro's purpose in all this is, of course, to enable the student to relate the later elaboration of investment, government, and consumption functions more directly to their real world counterparts in the accounting system.

With some exceptions, Shapiro does a rather good job of developing the standard Keynesian model with its IS-LM functions. The virtues and defects of this type of model have been amply presented elsewhere so I shall deal with only Shapiro's treatment of the system.

His discussion of the consumption function includes a comprehensive presentation of empirical findings related to consumption but is theoretically rather weak in that he relies almost exclusively on the Duesenberry thesis of "relative" versus "absolute" income levels in explaining the slopes and shifts

of various consumption functions. He completely ignores, except for one brief footnote, the intrinsically more interesting and theoretically more important permanent income hypotheses of Friedman and Modigliani and Brumberg.

In reading the section on the demand for money, I could not help but wonder why it seems to be necessary to continue perpetuating for undergraduates the idea that the demand for money (defined as means of payment) can have any rational basis other than for transactions. There are numerous riskless ways of holding assets that yield a positive rate of return and are completely liquid. The only reason why a person should hold cash instead of one of these assets is that there is some probability he will be making a payment in the near future and the cost of converting from cash to asset to cash is greater than to return over the holding period.

The speculative demand, in particular, is a rare animal to conjure up. An individual can always hold time deposits, savings and loan shares, or short-term bills and still speculate against rising interest rates. The only important entity in the economic system that might conceivably have a "speculative demand" is the banking system. If a bank's only alternatives were excess reserves and long-term bonds (because of shortage of supply of short-term notes or loans) it could conceivably choose the former. But excess reserves are not part of the money supply and therefore demand for them cannot legitimately be included as part of the demand for money. Shapiro's discussion of "idle" deposits versus "active" deposits in commercial banks is in much the same vein. Again, if the only logical reason for holding demand deposits at all is to take care of current or expected transactions, then they all must be considered "active."

Shapiro is rather critical of monetary policy for the usual reasons, i.e., that there are all kinds of "slippages" through adjustments in velocity and through the operations of nonbank financial intermediaries. The argument that results implies that the effectiveness of monetary policy as a function of the strength of application is apparently a step function. Either it is too gentle to do any good because of "slippages" or if it is tough enough to overcome these private adjustment mechanisms it is disruptive and destabilizing. There is apparently nothing in-between that might approximate a correct policy. The existence of this kind of function is yet to be proved and lacks somewhat in logic.

There are legitimate arguments against relying too heavily on monetary policy because of lags and the difficulty of forecasting and so on but, given the present legislative/executive nexus that makes spending and taxing decisions, these same arguments are applicable to fiscal policy. It would, therefore, seem rational to give at least equal weights to the two.

Overall, the book is well written. It contains many numerical examples that are for the most part useful. The book's greatest strength is its comprehensiveness. The major general weakness is the lack of an agnostic point of view that would permit genuine critical analysis of the models and systems presented. In spite of this (or perhaps because of it) it is likely to prove to be a popular book. I would rank it behind Dernberg and McDougall and Ackley, but probably ahead of other texts in the area as a teaching aid.

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Elementary Aggregate Economics. By HOWARD J. SHERMAN. New York: Appleton-Century-Crofts, 1966. Pp. vii, 249. \$2.95.

Public commentary on economics problems usually emphasizes certain slogans familiar to many and understood by few. In recent years, the "new economics" is the title given in popular publications to any discussion of national economic policy. Reflecting this vogue, several books on macroeconomics have appeared recently with the purpose of explaining in a nontechnical manner what the analysis means.

Professor Sherman's book is designed to supplement the elementary economics text or to be used as one of a series of readings in a corresponding course. With the purpose of helping the elementary student to understand "the important economic issues facing all citizens," he has organized the book around three issues: continuing unemployment and business cycles, inflation due to monopoly power, and growth in developed and underdeveloped countries. The approach and contents closely resemble an earlier book by Sherman, *Introduction to the Economics of Growth, Unemployment and Inflation*, except for the addition of certain elementary subjects such as how banks create money.

The strength of this well-written book is the discussion of business cycles in Chapters 2 to 6. Sherman presents an excellent review of the historical development of business cycle theory both before and after Keynes. In addition to a satisfactory presentation of Keynesian analysis, he discusses how it differed from the classical position and Say's Law. Utilizing modern national income terminology, Sherman reviews the work of several eminent economists including Mitchell, Schumpeter, Hicks, J. M. Clark, Hayek, Haberler, Hawtrey, Robertson, and Duesenberry. His emphasis in this part is with the theoretical causes of business cycles rather than the various policy proposals made to solve them, although the latter might have been of interest to many students. He does discuss, later in the book, the postwar debate among economists over appropriate policies to achieve economic stability.

The problem of inflation is the main subject of Chapter 7 with emphasis on the effects of oligopolistic concentration in industry. The approach is unorthodox regarding the usual elementary treatment of causes of inflation, and would be useful in explaining the view that wage-price guidelines are necessary to prevent inflation during a time of expansionary policies. Sherman argues that larger firms have a more stable profit rate, and that the effect of increased concentration of output has been to decrease the cyclical instability of profit margins in certain industries. He does not discuss, however, union influence in oligopolistic industries although he briefly discusses the general effects of unions in other chapters.

The discussion of growth in developed and underdeveloped countries is adequately analyzed in two chapters, although one might desire a more detailed study of the experience of the USSR which Sherman presents as a case study.

The final two chapters contain a discussion of monetary and fiscal policies in elementary detail and, as is true in many elementary textbooks, offers a too brief evaluation of them in regards to historical experience. For example, in discussing discretionary fiscal policy he refers to the "relatively minor prob-

lems of economic forecasting and administrative flexibility," a view difficult to accept when one examines nothing more than the debate on whether taxes should have been increased in 1966.

In summary, I would recommend this book as a supplement to an undergraduate course studying business cycle theory. I doubt that it would be more useful than Sherman's earlier book in supplementing the typical principles text used in the introductory course which was his original purpose. Its discussion of our banking system, monetary policy, and fiscal and budgetary policy is sketchy compared to that in the better principles' texts, omitting such subjects as our experience with monetary policy, problems of the public debt, and possible conflicts between monetary and fiscal policies. If these subjects, plus a more detailed analysis of the qualitative problem of unemployment, had been discussed with the same clarity and depth as that of business cycles, I believe the book would have been a more useful supplement to existing texts. However, if the book is selected for its strong features, I consider it superior to most elementary macroeconomics books now on the market.

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Economic History; Economic Development; National Economies

Accelerating Development: The Necessity and the Means. By LAUCHLIN CURRIE. New York: McGraw-Hill, 1966. Pp. xiv, 255. \$7.50.

After a distinguished academic and government career in the United States during the 1930s and in wartime, Lauchlin Currie went to Colombia in 1949 to head the first general country survey mission assembled by the World Bank. Fighting the brain drain in his own fashion, he stayed on in Colombia in a succession of roles: as adviser, businessman, rancher, and finally again as adviser and teacher. *Accelerating Development* is the result of this exceptionally long and varied exposure of a "Northern"-trained economist to the problems of one particular "Southern" country.

The book has many faults. It is disorganized, poorly written, repetitious and four of five of its 15 chapters—mainly those where Currie recapitulates and disputes the notions of his colleagues—could have been omitted without loss. Moreover, those who look to the book for the insights the author must surely have gathered on political and cultural factors bearing on economic progress will be disappointed: while Currie says that it is "absolutely essential" to take such factors into account in framing development policies his observations on the Colombian scene or national character are remarkably uninteresting as well as dubious ("The Colombians are prone to compromise, to avoid conflicts on principles and to improvise in economic matters" p. 7).

Nevertheless, it would be a pity if these defects were to keep students of economic development from having a serious look at Currie's message. For this is, above all, a book with a message which I shall now attempt to summarize.

The aim of economic development must be to assure a minimum standard of living to the poorer half of the population. This poorer half lives today

overwhelmingly in the countryside as subsistence farmers. Investment in agriculture would only make the situation of these people worse because of prevailing low income and price elasticities of demand for food, and agrarian reform would mainly serve to tie them more securely to their misery. The only way out is to transfer large numbers of people from the countryside to the cities and to provide them with productive employment there. To do this is less different from depression economics than one might think: (a) because there exists a good deal of unutilized capacity in mass consumption industries, especially if full-capacity operation is defined as round-the-clock operation in three shifts; (b) because technical advances in tropical agriculture have increasingly become available and will be more widely introduced as soon as the "man with the hoe" stops competing with the commercial farmer using tractors and fertilizer. In spite of these favorable factors, the accelerated transfer (and employment) of people from the country into the cities implicit in Currie's "breakthrough plan" requires acts of economic policy akin to wartime controls. If undue inflationary and balance of payments pressures are to be avoided, the structure of production of the country must be radically reoriented from catering to the rich and the middle classes to providing housing, public services, and wage goods for the new city dwellers; and since the tax instrument is most imperfect in countries like Colombia, resources presently misapplied must be released through a battery of mandatory controls over imports, building licenses, bank lending, and so on.

Currie gives only scattered "illustrative" indications about the size of his breakthrough plan. At one point he talks about a country with half of its labor force in agriculture and surmises that during each year of the plan the whole natural increase of about 3 per cent *plus* an additional 3 per cent of the work force in agriculture might be transferred to nonagricultural pursuits. Considering present trends, this would mean almost a doubling of the current high rate of urbanization of Latin America. Further, although the term "breakthrough" and the frequently invoked wartime analogy would make one think that the contemplated period would last a few years at most, we are told at the end of the book that the breakthrough could be considered accomplished only when the agricultural labor force constitutes no more than 10 per cent of the total, when the rate of population growth has fallen to one per cent or less, and when "basic equality of income, consumption and opportunity prevails" (p. 244). In other words, the emergency controls advocated by Currie must remain in place and function at peak efficiency for at least a generation!

In spite of such incongruities, Currie's prescriptions reflect a commendable desire to tackle the present situation in the developing countries with the means at hand instead of waiting for some *deus ex machina*—be it large-scale foreign aid, improved terms of trade, an interregional common market, or revolution. With his thorough experience in depression economics, he has correctly perceived certain characteristics of semi-industrialized developing countries like Colombia, such as disguised unemployment combined with unutilized capacity in consumer goods and construction industries, which make Keynesian remedies applicable *provided* either limited inflation is tolerated or income

is effectively redistributed or controls are enforced. Brazil in the 1950s and Cuba during the first two years of the Revolution are illustrations of the first and second provisos, respectively, while Currie is making a strong plea for trying out the third.

What seems hardest to accept is the insistence that not only land reform, but almost any allocation of resources for agricultural growth is to be avoided and the correlative assurance that the supply of agricultural output is highly elastic. While the orthodox, alarm-ringing FAO line on this subject may be subject to doubt, Currie merely asserts the opposite point of view and does not pay any attention to the evidence of inadequate growth in agricultural production implicit in recurring relative rises of food prices. He dismisses the need for agrarian reform in Colombia with the simple assertion that "the progressive farmers are generally large, and the small farmers are stagnating or retrogressing" (p. 163) in that country. It does not seem to have occurred to him that the first part of this sentence is perfectly compatible with the statement "the large farmers are *not* generally progressive"—and therefore with the need for land reform. Currie's apparent belief that in agriculture size is closely correlated with productivity is strangely uninformed.

The second part of the book is a detailed description of how the "break-through plan" would operate in Colombia. It contains interesting computations of the population that could be released from agriculture on certain assumptions about widespread "technification," i.e., by assuming that yields per employed person will rise to levels which have been reached by efficient farmers—a novel way of estimating disguised rural unemployment. These calculations were made in the early 1960s when Currie first put forward his "break-through plan," then known as "Operation Colombia." The greatest value of these proposals has been that they have led to a lively public discussion and even to some empirical research of which only a small part is to be found in the book. It is to be regretted, for example, that the information which was collected on underutilized capacity in the manufacturing and construction industries was not included.

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The First Industrial Revolution. By PHYLLIS DEANE. New York: Cambridge University Press, 1965. Pp. viii, 295. \$8.00; paper, \$2.95.

This is a disappointing book, adding little to the author's important work with W. A. Cole (*British Economic Growth, 1688-1959*), which was novel in scope and method. The present book is a useful, up-to-date, teaching- and learning-aid for lecturers and students; its scope is comprehensive and there are the beginnings in it for any conceivable lecture or essay about the industrial revolution. It is already in wide use, both because of its subject matter and also because of its author's prestige, but is altogether thinner than Miss Deane's earlier volume, and, moreover, often reads like a summary of it. As a shortish book, it does not rival, either in elegance or understanding, T. S. Ashton's *The Industrial Revolution* of 1948. As a quarry of information, it is

patchy. Most surprising of all, it is conventional in method; there is little in it of "new" or quantitative economic history; few statistics and not much theory. In spite of Miss Deane's claim in the Preface that she draws heavily on "economic and social historians who have been looking at the past through development spectacles," and that English industrialization has "a special relevance" to countries "currently finding it difficult to begin or to sustain a process of industrialisation," neither *economic growth* nor *underdevelopment* figure prominently in the subsequent text. Perhaps the explanation lies in the character of the book as "a course of lectures given for undergraduates"? Hence the sixteen neat chapters (two chapters a week for one term, or one a week for two terms, chapters of near-equal length—average 17 pages—and consisting of an hour's lecturing), each consisting mainly of a very general selection of information from well-known, usually secondary, sources. Perhaps this is the role of the Cambridge lecture, but the result in print is a series of bald and indecisive summaries which together do not give a coherent picture of "the first industrial revolution." This volume will be much used by students of history, although it will seldom be by the elbow when the facts have been summarized and the thinking begins; it has little of interest for the economist.

The main shortcomings of *The First Industrial Revolution* as a serious contribution to industrial revolution studies are structural and analytical. The structure of the book is neither chronological nor functional, and three general chapters fail to give pattern or synthesis to the other chapters. A logical structure would have been to have discussed and defined "an industrial revolution," to have analyzed the factors which caused the English industrial revolution, and to have considered why the revolution occurred first in England rather than elsewhere. The phenomenon to be explained was the sustained increase in the growth of total output at a rate which was revolutionary compared with what went before. This was, obviously, the result both of a remarkable increase in the productivity of human effort and also of a notable increase in productive factors. Fortunate factor endowment had much to do with England's growth, but the relative roles of increasing factor supplies (land, labor and raw materials, especially coal) and increasing productivity (the result of changing technology and organization, and of improved physical and human capital) are not adequately discussed. Nowhere does Miss Deane make clear what the industrial revolution was—in terms, for example, of the changing composition of output, or of the changing structure of the economy—or why growth occurred. And where the role in growth of particular factors is discussed, she is almost always inconclusive. Agriculture, for example, is first given a causal role—to provide food for a growing population, to inflate purchasing power for industrial products, to provide capital for industry—and then has it taken away. "The transformation in agriculture was contemporaneous with the transformations in industry, trade and transport, and is best regarded as part and parcel of the same process." It is much the same with other factors which Miss Deane considers were relevant to English growth, and whose relationships are described with phrases that are vague and analytically unhelpful: "contribute to the effectiveness of," "help to precipitate," "made a massive contribution to," etc. Thus, the process of growth remains

elusive, which is surprising, considering that there was a most interesting and original analysis of the mechanics of eighteenth century growth in *British Economic Growth, 1688-1959*. Here, instead, are a number of largely unrelated chapters on particular topics; here is a description of the English economy before industrialization but no analysis of the structural relationships; and here, although there is some identification of the relevant variables of English economic growth, there is little attempt to show how they changed and interacted to produce growth. And so Miss Deane ignores or avoids some of the leading problems raised by the historians of the industrial revolution and by the economists of growth. Was there "a take-off"? Was the revolution a revolution with a clearly defined turning point, or was it the culmination of an evolutionary process? Miss Deane weakly argues that "to understand the process of economic change one needs to take both approaches into account." Did the industrial revolution proceed from a broad or narrow base? Was it an example of balanced or unbalanced growth? Were there necessary prerequisites? Does Miss Deane's ordering of chapters—on demography, agriculture, commerce, and transport, in that order—imply allegiance to a stages theory of growth? Is English economic growth relevant, as the Preface implies, to the problems of the growth today of underdeveloped economies? These and other important questions are not answered. At least on the last question, it should be stressed that any justification for the study of the first industrial revolution based on its usefulness for growth theorists and planners of today represents a misguided rationalization to make history seem relevant when it is almost entirely irrelevant. England in 1700 had a more advanced society and economy than most underdeveloped countries of the modern world had before they began painfully to grow; and England's example, for this and other reasons, is almost certain to mislead rather than to instruct those interested in modern underdevelopment.

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A Study of Contemporary Ghana. Vol. I, The Economy of Ghana. Edited by WALTER BIRMINGHAM, I. NEUSTADT, AND E. N. OMABOE. Evanston: Northwestern University Press, 1966. Pp. 472. \$12.50.

Here is a book of the type for which beleaguered teachers of economics in the universities of the underdeveloped areas cry. Prepared by three contributors (Killick, Szereszewski, and E. N. Omaboe, only one of whom is listed on the cover), it constitutes a broad survey of the economic structure, institutions, and major policy issues of a particular nation—Ghana. It not only gathers widely scattered material from a variety of less readily accessible sources but also provides new data and original effort in a number of previously neglected areas. Moreover, it assumes very little previous training in economics on the part of the reader. Apart from providing a mine of statistical information for those having little familiarity with the Ghanaian economy (it contains some 140 tables), it can serve as a first-rate text for undergraduate economics students and other general readers in Ghana. It is the first book of its type in

tropical Africa. One must hope that it may serve as a model for similar efforts in other nations.

It is difficult to convey the degree of comprehensiveness of this volume. Let me very briefly summarize what is in it. The general macroeconomic picture of recent economic performance and the sectoral and regional structure of the economy are sketched in the first quarter of the book. This part also includes a summary chapter on consumption patterns. This is followed by wide-ranging discussion of labor and capital inputs in Ghana, occupying nearly another quarter of the total. The longest part of the book is a more detailed consideration of the principal sectors—agriculture, mining, industry, finance, etc. Some relatively slight (for an avowedly socialist state) consideration of the governmental role in the economy makes up the concluding section.

Its approach is workmanlike and undramatic. It has few daring ideas and no fancy tools. Its scholarly caution about the limitations of the available statistics and the tentativeness of conclusions drawn from them are thoroughly admirable; although it must be said that they so permeate the analysis that the fledgling economist may be driven by them to despair. These factors make much of the book fairly dull reading. Particularly is this the case in the long and inconclusive analysis of labor productivity and in the fruitless attempt to measure total capital stock by adding "conventional" measures to the discounted value of returns from future cocoa production. At the same time there exists some gems of information collection and analysis; those of most general interest are probably the chapters on cocoa and the Volta Project. How many know, for instance, that the Valco smelter in Ghana will produce aluminium out of alumina, sent from America, which is itself processed from Jamaica bauxite, while Ghanaian bauxite is exported for processing to Scotland?

A good test of the usefulness of such a book is the extent to which its analysis is rendered invalid and its material irrelevant during the publisher's "gestation period." On this test, despite the recent upheavals in Ghana, this volume stands up quite well. Ghana's economic difficulties of 1965-66, undoubtedly a major factor in the government's downfall, were already visible to the writers in 1964, though they themselves were not always in full agreement on the issues.

The imposition of import controls towards the end of 1961, for instance, was seen by Killick as a "short term expedient" which despite its disadvantages was, when coupled with price controls, "one of the very few lines of reasonably effective action that is left open" (p. 428). In his view, forced reductions in the demand for imported consumer goods "will tend automatically to create additional savings" (p. 427), although he admits that if restrictions appear to be permanent this analysis will no longer apply. Against this, set Omaboe's more pessimistic and unfortunately more accurate view: "Import controls have probably come to stay and with them the previously most effective safety-valve for inflationary forces in the economy has been sealed" (p. 29).

Inflation was already recognized as a major and growing problem in 1963 and 1964. Killick argues persuasively that even if rapid inflation were to raise the savings rate, which is by no means certain, it would not raise the growth rate since the key bottlenecks were (and presumably still are) those of foreign

exchange and high-level manpower rather than that of savings (pp. 417-22). The fall in real wage rates as a result of the extraordinary wage restraint, which itself was the result of the "unity of purpose of the union leaders and the CPP" (p. 143), was already very evident in 1963. At the end of that year the real wage rate was already well below the levels of 1939, though it was to fall still further. The authors might have provided a little more explanation of how this degree of "squeezing" was achieved. Despite their illegality, strikes did, after all, take place in Ghana.

There were other signs of impending difficulties. Unemployment in 1960, a good year, was already 10 to 15 per cent of the male wage-labor force (p. 149). The tendency of food production to lag and the possibility of distribution bottlenecks was also already recognized (p. 228). So were the failures of most of the public corporations.

A full account of the economic policies of the CPP government in its final years of power still has to be written, but Omaboe, in the closing pages of this book, throws out many hints as to the difficulties as viewed from within at the time. Here he refers to "differences which sometimes develop between the Government and the planners"; he notes that "civil servants have had to struggle hard to catch up with the politicians" and that frequently "some commitment is entered into before the civil servants are called in and they are therefore handicapped in the application of their skill and experience" (pp. 460-61). The inadequacy of the machinery for economic and financial decision-making, more crudely the unwillingness of the political to listen to the technical seems to have been largely responsible for the failure of economic policy and development planning in Ghana.

Killick, Szereszewski, and Omaboe deserve great credit for their painstaking work on the Ghanaian economy. Perhaps they can be encouraged shortly to return and add the even more interesting story of the 1964-67 period to the basic structure of their account.

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Growth without Development—An Economic Survey of Liberia. By ROBERT W. CLOWER, GEORGE DALTON, MITCHELL HARWITZ, AND A. A. WALTERS, WITH ROBERT P. ARMSTRONG, JOHNETTA COLE, ROBERT E. COLE, AND GEORGE LAMSON. Evanston: Northwestern University Press, 1966. Pp. xv, 385. \$12.50.

Foreign Investment and Development in Liberia. By RUSSELL U. McLAUGHLIN. New York: Frederick A. Praeger, 1966. Pp. xiii, 217. \$15.00.

In 1961 and 1962 a team of economists conducted a detailed and comprehensive survey of the economy of Liberia. In their report to the Liberian government, they concluded that, while the growth of output had been rapid, little basis for future growth had been or was being created; and further, that this unsatisfactory state of growth without development was largely attributable to bad government. For obvious reasons, the government of Liberia did

not release this report, but in shortened and edited form, this study by Clower and his associates has now become available. Development economists and African specialists will welcome the detail this work presents on national income accounts, foreign concession agreements and performance, institutional arrangements with respect to land and labor, educational policies, and labor surveys. The roughly two-thirds of the book which deal with these specific aspects of the process of economic change in Liberia contain hitherto inaccessible descriptive detail and perceptive analyses.

In the more general parts of this book, the authors recognize "... that a certain amount of *undirected* [italics supplied] social and political change has accompanied ..." the sharp structural changes in the economy (p. 245), but they argue that Liberia remains "... a prime example of both an 'enclave' and a 'dual' economy" (p. 4). Transformation of the subsistence sector has been prevented by the persistence of "... deleterious government practices carried over from ... pre-World War II Liberia" (p. 384). Hence, their argument continues, *deliberate* changes in government administrative, fiscal, and educational policies are needed in order to alter the social, economic, and technological dimensions of tribal life, and in order to distribute national income in a manner consistent with social justice. From the authors' pessimism regarding the likelihood of such changes follows the conclusion that the outlook for development in Liberia is at best uncertain.

The essence of the argument of Clower and associates is a preference for orderly, deliberately planned growth which generates a distribution of income and political power which is fair by Western standards; such growth is called "development." Liberian growth has relied on *laissez faire* incentives and has been substantially undirected. It has been marked by corrupt and wasteful practices, has yielded a highly skewed income distribution, and *it has been remarkably rapid*. Although there are obvious pitfalls in such a comparison, the growth experience of the post-Civil War United States could be characterized in much the same way. There is no scientific evidence that such a growth process results in less rapid achievement of a high standard of living by all of the population or causes more social conflict and disruption on the path to affluence. The Liberian experience may provide an example of a currently unfashionable, but nonetheless effective, approach to the problem of underdevelopment. Hence the thesis of *Growth without Development* is based in part on an unacceptable value judgment, and in part—as I shall point out below—on a mistaken belief that political and social institutions in Liberia are static.

McLaughlin's short and heavily footnoted book is derived from his 1958 doctoral dissertation, but it has been expanded and revised on the basis of his experience as an economic advisor to the Liberian Office of National Planning during 1963 and 1964. His extensive references usefully complement the Clower book, which, contrary to what one expects from a survey, almost totally neglects all other work on Liberia and West Africa. The first three chapters of *Foreign Investment and Development in Liberia* present in general terms the social and geographic background, and then describe the growing role of foreign enterprises in the economy. Next, there are chapters on labor; savings, investment, and exports; external assistance; and finally, a summary chapter.

The latter chapters are the more interesting, since a more concise and equally current general description of the structure of the economy has been available for some time.¹

In contrast to the thesis of *Growth without Development*, McLaughlin argues that "social and institutional factors which (are) . . . causes of underdevelopment are undergoing change in Liberia" (p. 196), and that heavy foreign private investment and public assistance has provided a basis from which a self-sustained process of economic development can emerge. In his view, development of the nation's human resource potential, and domestic entrepreneurs who will engage in domestic capital formation, are the crucial needs. The chief merit of McLaughlin's book is its emphasis on the continuing modification of social and political institutions which has resulted in significant changes in the lives of ordinary Liberians in the last decade. Correspondingly, it is the inability of Clower and his associates to recognize the rapid social changes occurring in a backward social structure which constitutes a major flaw in their work. I do not see how one can ignore the social effects over the last decade of a doubling of money sector employment and a doubling of money sector real wage rates for unskilled labor, together with a growth of urban population at a rate of roughly 10 per cent *per annum*. If anything approaching these rates of change is maintained, the subsistence sector will have disappeared in a generation or two.

Another serious fault of *Growth without Development* is that it is badly out of date. Since all the material in the book dates from 1962 or earlier and no attempt has been made to update it, the incautious reader may frequently be misled. For example, half of the total resource transfer to Liberia through foreign assistance has taken place since 1962. This makes Chapter 12 on Foreign Aid and its appendix obsolete.² Again, LINEC (p. 141) has been dead since 1963, and the Mt. Coffee hydroelectric project, which was to have been financed by LINEC, has been largely financed by AID. By far the largest public investment project in the country, it will come on line in 1967. Chapter 5 on Planning and Fiscal Policy and its appendices are likewise obsolete: government revenues have been functionally classified for several years; the collection and dissemination of statistics have vastly improved; and, contrary to the view that "It will always pay Liberia to import steel . . ." (p. 101), Liberia has a comparative advantage in steel production, and negotiations are in progress to establish a West African steel mill at Lower Buchanan. Revision of the debt service schedule was completed in early 1964, and annual payments are about two-thirds of the \$15 million figure given (p. 26).

Two relatively technical points should be made with respect to the national accounts data for 1960 presented in both books. First, the imports figure in the national accounts exceeds the f.o.b. figure for imports in the Liberian trade

¹M. A. Qureshi, Y. Mizoe, and F. d'A. Collings, "The Liberian Economy," *I.M.F. Staff Papers*, July 1964, 11, 285-326. Strangely, neither book under review refers to this article.

²Incidentally, the popular practice, employed in both the books under review, of measuring foreign assistance by *obligations* rather than *disbursements* or *expenditures*, is one to which I take strong exception.

statistics by some \$14 million, but is specified to be f.a.s., which should make it smaller than the f.o.b. figure. Moreover, the logic of the national accounts requires that c.i.f. figures be used. Secondly, the difference between gross national product and gross domestic product is net factor income paid to nonresidents. Since the earnings of expatriates were roughly \$30 million, and many, if not a majority of them, were effectively nonresidents, the difference between GDP and GNP would appear to be roughly \$55 million rather than \$35 million. This correction reduces per capita national income to \$100 from \$125, which is surely a significant change.

In summary, McLaughlin's book lacks an index and needs editing. Its high price makes it unsuitable as an introduction to the Liberian economy. For the African specialist it is lacking in detail and for the development economist it offers few new insights. *Growth without Development* has serious faults, but it is well written and may be read with profit.

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Economic Development and Export Growth: A Study of Northern Rhodesia, 1920-1960. By ROBERT E. BALDWIN. Berkeley: University of California Press, 1966. Pp. xviii, 254. \$6.00.

This study is a welcome addition to the literature of case studies of economic development. While the author's primary concern is with the factors determining the pattern of economic expansion in a single country, his argument is developed within a wider framework. Baldwin's central thesis—presented earlier in his *Economic Journal* article of 1963, but here developed more fully and supported with detailed empirical findings—is that the technical characteristics of the production function in the pioneer export industry are crucial determinants of the pattern and pace of economic change in an underdeveloped economy. This approach permits a broad differentiation between two types of underdeveloped economy: (1) those in which the export base is organized around large-scale enterprises created by external capital and enterprise; and (2) those in which the export base is built up around the outputs of peasant producers. Though his book is intended to deal systematically only with an economy of the first type, its organizing theme is a useful counter to the unfortunate (though still prevalent) propensity of some commentators to generalize about underdeveloped economies as if they comprised a homogeneous group.

As Baldwin successfully demonstrates, the export technology hypothesis has considerable explanatory power in the case of Northern Rhodesia (Zambia since 1964). Dating from the 1920s, the base of monetized economic activity has been built up around copper mining, an activity which, in the circumstances, was highly capital intensive. In the formative stages, three inputs were required for the successful conduct of operations: capital, skilled labor (supplied from abroad), and unskilled labor (supplied locally). Such productive combinations permitted a rapid growth of monetized geographical prod-

uct, but in other respects these arrangements blunted the spread of developmental stimuli. The resulting distribution of income was not conspicuously favorable to the local build-up of secondary lines of production. A substantial share of after-tax profit was typically repatriated and the wage bill of Europeans (which exceeded that of Africans by a considerable margin) was characterized by high propensities to import. In addition, the extent to which a highly capitalized export industry could generate backward linkages was severely limited. In only a few cases was local production of the intermediate products required by the dominant export industry economically feasible.

Though the technology of the dominant export industry can take one a long way toward an understanding of the Northern Rhodesian case, it is not alone sufficient to account completely for the resulting economic structure. Baldwin quite properly notes that social and institutional forces which have supported a "dualistic" economic pattern have also been significant in shaping the outcome. Though "dualism" has been manifested historically in the allocation of governmental expenditure and in agricultural marketing arrangements, its impact has been most visible in the wage labor market. Originally, a gulf between the wage scales of Europeans and Africans reflected a divergence between the demands of the mining industry for skills and the capacity of the local population to satisfy them. Later, this pattern was propped by artificial supports and, for a considerable time, the advancement of African wage earners was frustrated. Events of the past decade have erased much of the discriminatory basis of wage employment. Yet, as Baldwin correctly points out, the allocative distortions associated with an anachronistic wage structure survive. He writes: "A small group of Europeans and urban Africans, who possess monopolistic labor power, have gained, at the expense of Africans in the rural areas and in the low-paying service industries in urban areas. The latter group does not have the economic or political power to prevent the massive misallocation of labor resources which is being frozen into the economy" (p. 108).

Baldwin's account of the impact of export technology on the growth process, it should be noted, does not rest on an assumption of fixed coefficients in export production. On the contrary, he insists that, over a substantial range of activities, there is considerable scope for variation in factor combinations. The bulk of one chapter is devoted to an analysis of the adjustment of the copper mining groups to changing terms of factor supply—in particular, to upward pressure on African wage rates during and after World War II—and it makes excellent reading. This process has generated a redefinition of jobs within the industry and the opening of higher-rungs on the skill ladder to African miners, but it has also been associated with a substitution of capital for labor.

The export-technology thesis—even when qualifications have been added—is nevertheless strong enough to override an alternate hypothesis on arrested development in Northern Rhodesia. The line of interpretation which links retarded economic advance with the failure of Africans to respond positively to the emergence of new economic opportunities is explicitly rejected by Baldwin with the aid of fresh data from the records of the copperbelt mining companies. His attempt to demonstrate a positive supply response by African farm-

ers to price incentives is less satisfying. Lacking data for Northern Rhodesia, Baldwin puts multiple regression analysis to work on material gathered by the Southern Rhodesian Department of Native Agriculture. The reliability of these data is even more dubious than the author's numerous caveats allow for.

In Baldwin's view, the "educative" impact of unfamiliar techniques on labor efficiency offers partial compensation for the failure of a high-technology export base to generate more dynamic demand stimuli for growth. Within the confines of the dominant export industry, the point is well taken. It must be emphasized, however, that the numbers directly affected are small, both absolutely and relative to the size of the population of working age; in 1962, for example, the copper industry engaged fewer than 35,000 African employees. Meanwhile, the bulk of the population in traditional agriculture has largely been bypassed during the postwar period of remarkable expansion of export receipts and of money national income.

As a well-documented and well-argued discussion of a special type of economic expansion, this book can be read with profit by students of the underdeveloped world.

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Statistical Methods; Econometrics; Social Accounting

The Identification Problem in Econometrics. By FRANKLIN M. FISHER. New York: McGraw-Hill Book Company, 1966. Pp. xi, 203. \$8.95.

This addition to the Economics Handbook Series consists of six closely written chapters on the identification problem, a topic which receives only limited attention in textbooks on econometrics. The material which is covered in the book is based primarily on Professor Fisher's recent work on identification and includes several extensions of his previously published results. As such, the book provides an excellent survey of the current state of the identification problem.

The first two chapters are devoted to a discussion of the problem of identification and the usual textbook results. The identification problem is introduced within the context of a single-market model. The discussion of this model in both the stochastic and nonstochastic cases sets the stage for the following investigation. In Chapter 2 the rank and order conditions are derived for identifiability of an equation under linear restrictions on the coefficients which enter the equation. An interesting discussion of identifiability and the use of two-stage least squares and k -class estimators is also included in this chapter. In particular, the way in which these estimation procedures break down when applied to an equation that is not identified is described.

The remainder of the book includes material which is referred to only casually, if at all, in most econometrics textbooks. Chapter 3 is devoted to a discussion of the use of restrictions on the size of the variances of the disturbance terms in the model to (nearly) identify one equation with respect to another

equation or set of equations in the simultaneous equation model. The effect on identifiability of zero restrictions on the off-diagonal elements in the variance-covariance matrix of the disturbance terms is described in Chapter 4. This leads naturally to the theory of recursive and block-recursive systems. Again necessary and sufficient conditions for identifiability under coefficient and covariance restrictions are derived for general systems and applied to the special recursive and block-recursive cases.

Identifiability criteria for nonlinear systems are developed in Chapter 5. Two types of nonlinearities are separately considered: systems which are linear in the disturbances and parameters but nonlinear in the variables, and systems which are linear in the parameters, variables, and disturbances, but in which the parameters are subject to nonlinear constraints. These two types of nonlinearities are shown to arise in the nonlinear case when the equations of the system are expanded in a Taylor's series. Necessary and sufficient conditions for identifiability in each case are then derived. The final chapter is devoted to a brief discussion of "other topics" including autocorrelated residuals and tests of identifying restrictions.

In the author's words "the book is intended for students of econometrics and is meant to be read in conjunction with the study of simultaneous equation estimation." The reader is therefore assumed to know the rudiments of matrix algebra and to possess a modicum of statistical knowledge. Although estimation is mentioned briefly in several parts of the book, the material is confined almost exclusively to the essentially mathematical problem of identification.

The exposition is enhanced by a fairly liberal use of simple examples to illustrate the basic problems and theorems. Comprehension of the results is facilitated also by supplementing the mathematical proofs with geometric and intuitive explanations. Despite this it is the reviewer's feeling that many readers will not find this an easy book to master. The interests of most readers might best be served by studying carefully the first two chapters and then working through the remaining material as specific research problems arise. The practicing econometrician will find that this book provides a comprehensive statement of the currently known results on identification in simultaneous equation models.

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Income Distribution in the United States. By HERMAN P. MILLER. A 1960 Census Monograph. Washington, D.C.: Supt. Docs., 1966. Pp. viii, 306. \$2.25.

This is the first monograph from the *1960 Census*, and the profession is indebted, as it was for the 1950 Census Monographs, to the Social Science Research Council's sponsorship, and to funds from the Russell Sage Foundation. This time the Equitable Life Assurance Society also provided some financial support. Herman Miller, who wrote the income monograph on the 1950 Census is again the author, and a highly appropriate choice, since in the meantime he

has produced a number of excellent special studies of income and a popular book on the subject.¹

The yeomanlike but necessarily dull task of presenting basic background information and data is enlivened by clear summaries of the more interesting findings. Decimal census data are supplemented by annual data from current population surveys. The reader first discovers that from 1947 to 1960 there has been very little change in the inequality of the distribution of money income among "families and unrelated individuals," to use the cumbersome Census method of indicating that they have not removed one-person families for separate treatment. The apparent stability remains whether one uses the share of income going to the top 5 per cent or 20 per cent or the Gini coefficient, even when one looks at subgroups of the population by age, location, employment status, whether the wife worked, or family size. The focus on inequality does divert attention from the income revolution during the period to which Katona has drawn attention, vastly increasing the amount of discretionary income and the proportion of people whose incomes are above some minimal level of adequacy.

There follows a description of trends in the demographic characteristics of the different income groups, classified by current dollars, then constant dollars, and finally by income quintiles. The changes are small and in the expected directions. A more interesting tabulation shows, for 1959 only, the sources of income of those in different income groups.

Then come three chapters of detail on incomes of those in different occupations and industries, serving to dramatize the fact that between 1939 and 1949 the greatest relative gains in income were made by the lower paid workers, while during the period from 1949 to 1959 the greatest relative gains were made by the higher paid workers. The author refrains from any extensive speculation about the relative roles of war, peacetime affluence, or the party in power in producing these trends. The detailed occupational data provide some striking evidence of the difficulties certain groups experience during periods of rising prices and incomes of their own salaries are difficult to change. It was not only the civil servants who were worse off, but also the clergymen, who fell five decile positions in spite of increased church activity and membership during the period. The trends in dispersion, and in incomes by industry group are less interesting.

The reader who has plowed through all these carefully worked-over and meticulously presented data, wondering why education was never mentioned, is finally rewarded by the best chapter in the book—one on income and education. Its main contribution is not the data from the 1960 Census, but the use of trend data to select the same cohorts (generations) at different points in time (years) and prove that age differences from a single cross-section under-

¹H. P. Miller: *Income of the American People* (New York, 1955), "Annual and Lifetime Income in Relation to Education," *Am. Econ. Rev.*, Dec., 1960, 50, 962-86; *Rich Man, Poor Man: The Distribution of Income in America* (New York: 1964), *Trends in the Income of Families and Persons in the United States 1947 to 1960*, Tech. Paper No. 8, U.S. Bureau of the Census (Washington, D.C. 1963). The last item contains a number of detailed tables not included in the monograph.

state the income increases people can expect because they leave out the influence of general upward trends in earnings. Indeed, it is possible to separate the increases experienced by a particular cohort with a particular level of education into those resulting from the passage of time, and those from the accumulation of experience and seniority.

Finally, there is an extensive methodological appendix providing data on various evaluation studies comparing Census income data with data from other sources, from the Current Population Surveys, and from reinterviews conducted by the Census bureau.

There is little to criticize about what was done. The editing and checking was obviously well done. The qualifications required in using and interpreting dollar money income, measured before most taxes and after most transfers, are carefully stated, although the reader is left with the impression that their relative importance is unknown. Other data do exist that allow one to "quantify the qualifications" at least roughly.²

At one point, a surprising impression is left that, poverty being relative, we shall always have it with us. This is a counsel of despair, since it is clearly possible to define poverty meaningfully as having an income less than some fraction of the average income of those with the same sized family, say a third. Such a definition is unaffected by inflation or change, yet poverty so defined could be eliminated. There will always be people below the average, but there need not be anyone below a third of the average!

In a few places the reader is left dangling. A multiple regression using education and some other things to explain 1958 income data from the Current Population Survey is mentioned, and the coefficient of multiple correlation given, but none of the regression coefficients nor any measure of the importance of education nor any reference. A few sentences later, reference is made to a National Science Foundation study showing that "only" 75 to 80 per cent of the 17-year old boys who rank in the upper 30 per cent of their high school class go to college, but no data are given by income groups or on the proportions with lower rankings who go to college. The original data show that there is an income effect within ability rankings, but they also show that very few of the low-ranked high school students go to college. The implication is that, if there is wastage of talent, it reflects the opportunity cost of not investing in the education of some talented youth, rather than a wastage of educational resources educating those without talent.³

Every reviewer yields to the temptation of describing a book he would have preferred to see written, unfair as it may seem. There is a crying need for

² See J. Morgan, M. David, W. Cohen and H. Brazer, *Income and Welfare in the United States*, New York 1962.

³ For basic data see the testimony of Willard Wirtz, *Hearings on the Employment Opportunity Act of 1964*, Subcommittee on the War on Poverty Program, House Committee on Education and Labor, 88 Cong., 2nd sess., March 19, 1965. See also C. B. Nam and J. D. Cowhig, "Factors Related to College Attendance of Farm and Non-farm High School Graduates 1960," in *Farm Population Ser. Census-ERS (P-27)*, June 15, 1962, which also shows a relatively high correlation between high school grades and IQ on the one hand and college attendance on the other.

some things that are not in this book, and since there will certainly be a 1970 Census Income monograph, we should urge them for next time. First, we'd suggest less attention to occupation. The definition of an occupation is difficult, its meaning unstable, its dimensions multiple, and its attachment to an individual loose. More attention to education, and where that education was received, is in order. This is particularly so in view of current arguments about the quality of education. Second, more attention to the things that are causing changes in income would help: undoubling of families, changing numbers and types of extra earners, more couples still working, but whose children have left home, and reduced unemployment and increased availability of overtime and second jobs.

Third, more attention to the welfare implications should lead to more detailed tables by family size and by number of earners. And more attention to the distinction between the private and social return to investment in education calls for analysis of *hourly* earnings, if one wants a measure of the potential social payoff unalloyed by the impact of unemployment, extra work and second jobs, or the tendency for high-salaried people to take their vacations. We should not attribute the low incomes from unemployment in 1959 to lack of education rather than social policy, nor the possibly increased incomes of the uneducated by 1969 if they have all the work they want then, to a reduction in the social return to investment in education.⁴

Finally, if we are to pay continued attention to the distribution of income, it is time someone attempted to assess its meaning and changes in its meaning, under the impact of the changing importance of nonmoney rights to retirement programs, unrealized capital gains, changing impact of the tax and transfer system, etc., paying attention to and attempting to quantify some of the issues so persuasively raised by Richard Titmuss.⁵

In the process, it may well be necessary to make use of data other than those produced by the Census Bureau in order to allow a richer interpretation of the Census data.

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⁴See J. Morgan and M. David, "Education and Income," *Quart. Jour. Econ.*, Aug. 1963, 77, 423-37.

⁵R. M. Titmuss, *Income Distribution and Social Change*, London 1962.

Linear Programming. By MICHEL SIMONNARD. Englewood Cliffs, N.J.: Prentice-Hall, 1966. Pp. xiii, 430. \$12.00; text, \$9.00.

Linear Programming is a topic already covered by an impressive array of texts-and-references. The translation (and extension) of the Simonnard book by W. S. Jewell provides a valuable addition to the existing collection of good books (in English) on linear programming.

The Simonnard book is favorably distinguished by its treatment of the mathematical and computational aspects of linear programming. The strategic problems of topical coverage and sequence, the tactical problems of proof and argumentation, and the articulation of the whole by a precise and economical

system of notation are all solved admirably in this book. The reader seeking a lucid, comprehensive exposition of the logical and computational mechanism of linear programming should be well pleased with the Simonnard book. In my judgment, the first ten chapters of this text—which cover general linear and integer programming—are not surpassed, as mathematical exposition, by any existing text.

All this is achieved at the expense of content likely to be dear to most economists. Economic interpretation is severely restricted in scope. While a few essential points such as the interpretation of dual variables as shadow prices are developed in a competent but cursory way, there is no pretense of a more than anecdotal approach to economic interpretation. There is, for example, no discussion of the economic ramifications of the decomposition principle. Discussion of applications of linear programming to specific managerial and technical problems is similarly restricted. These exclusions are of course part of the design of the book, and a section in the first chapter does briefly summarize major areas of application and provides some references.

Two characteristics of the book would seem fundamental to determining its appropriate use as a text: (i) it contains no exercises, and (ii) it makes comparatively severe demands on the reader's background in linear algebra and on his mathematical maturity. For students with sufficient mathematical preparation and/or capability this should provide an excellent text and reference.

The content of the book is divided into four main parts: (i) general theory and methods of calculation for linear programming; (ii) integer programming; (iii) theory and methods for special structures—mostly transportation networks; and (iv) three appendices that provide needed results in linear algebra, the theory of convex polyhedra, and graph theory.

Although the mathematical and computational orientation of the Simonnard book makes it an unlikely first choice reference for most economists, the economist with a special interest in linear programming is very likely to find in this book new insights and best expositions of several fundamental topics in linear programming. Beyond this, the Simonnard volume emerges in W. S. Jewell's translation as a deft and careful piece of intellectual craftsmanship.

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Economic Systems; Planning and Reform; Cooperation

A Reappraisal of Marxian Economics. By MURRAY WOLFSON. New York and London: Columbia University Press, 1966. Pp. xvi, 220. \$6.75.

In this book the author attempts many things: a critical survey, from a broadly positivistic position, of the philosophical background of Marxian economics, including a sketch of the background in European philosophical history as well as Marx's own philosophical position; a discussion of what he conceives to be the crucial technical aspects of Marxian economics from value theory through cyclical crises; and a discussion of post-Marxian theories of, among other items, revisionism, imperialism, economic growth in developed

and underdeveloped economies, collective bargaining, and the effects of U.S. foreign investments. All this in a rather slender volume of 190 rather loosely printed pages of text.

A century of criticism and counter-criticism has shown that what Marx "really meant" is obscure. No account of Marxism can be written that cannot be subject to the criticism that (in the eyes of the critic) important passages and possible interpretations have been neglected. In a sense anyone can be his own expert on Marxism. (And nothing is more deceptive than to assert, seemingly straightforwardly, that one is assuming that Marx meant what he said. What did he "really" say?) Thus in reading this review one should bear in mind that questions of omission and interpretation are to a degree matters of taste.

Wolfson's position differs from many recent writings by anti-Marxists, sympathetic non-Marxists, and Marxists, in that he feels that fruitful studies of Marx cannot take him piecemeal. The basic philosophical background is intimately tied to his philosophy of history, which produces his theory of value, which determines his theory of exploitation, which leads inevitably to immiserization, crises, and the breakup of capitalism. The author assigns himself the task of examining this structure for its bearing on what he takes to be the crucial socio-economic issues of our time.

The initial philosophical discussion (Ch. 2) will require careful reading, because of its brevity, unless one is already fairly well versed in the history of European philosophy. This is a (minor) disadvantage since I believe that Wolfson touches upon an important truth in his emphasis—contrary to most writers—on the philosophical background of Marx's treatment of value. While doubtless derivative from Ricardo, the entire question of value vs. price and the transformation problem is difficult to imagine unless Marx started from a belief that the purpose of science is not the investigation of "superficial" empirical or phenomenal forms, but the essential noumena which "lie behind" appearances.

From there Wolfson devotes three extensive chapters, which form the main body of his work, to an analysis of Marxian economics in the narrower sense. The first (Ch. 2) deals with value defined as the labor content of a commodity. Contrary to most authors he chooses to ignore entirely the vexed questions of value and price, relative capital-labor ratios, and the transformation problem. Nevertheless, his critique of Marx generally follows neoclassical lines. He points out that ratios of exchange do not follow the "substance" of value (labor time) because of diminishing returns to land, the effects of tastes on the demand for commodities and the supplies of different types of labor, and differences in ability affecting supply.

In another major chapter (3) Wolfson turns to the value of labor power or the wage. If I interpret him correctly he argues that the crucial question is whether, given savings propensities and rates of population growth, technological change is sufficiently biased in a labor-saving manner to produce increasing misery. He concludes that Marx does not demonstrate that this will necessarily happen.

At this point the discussion becomes somewhat murky to this reviewer

caused by some combination of Wolfson's misunderstanding of Marx, obscure exposition, or weaknesses in theory, and/or, perhaps more likely, my own lack of understanding. He appears to argue not only that technological change will not necessarily produce increasing misery, but that the labor theory of value requires the wiping out of surplus value in equilibrium; "since *ex hypothesi* the capitalists perform no service requiring sacrifice under perfect competition they would receive no profit" (p. 110). He contrasts the labor theory of value with a supply and demand theory and "entrepreneurial productivity." But Marx did in fact have a supply and demand theory; the dispute about "productivity" is merely terminological. To measure quantities in labor units, as Wolfson has previously interpreted the labor theory of value, may be cumbersome but it cannot change anything substantive. Indeed he quotes Marx on how the capitalist responds to rising wages by reducing accumulation (with a lag) and keeping capital scarce (and therefore "productive" to the neoclassical economist). The lag produces the "real" cycle of Volume I which is analytically distinct from the aggregate demand cycle of Volume III—a distinction which the author does not appear to make.

Since Wolfson concludes that labor threatens to swallow up, in Marx's scheme, the total social product in competitive equilibrium, he turns to the cycle and crisis discussion in Volume III for the clue to increasing misery and capitalist breakdown. Increasing misery requires increasingly severe crises, and again he concludes that Marx has not established such a tendency. The conclusion is partly based on the logical confusion in having simultaneously declining rates of profit, constant wages, and technological progress; it is partly based on the fact that it cannot be put to a refutable test.

To this reviewer, Wolfson, like many commentators, underestimates the extent to which the breakdown can be derived with the apparatus of Volume I. There is no *logical* reason why technological change could not produce increasing misery, and with it a degree of political instability that would end in revolution as described in this volume. Of course there is no empirical evidence that there is even a technological tendency in this direction. But this does not prevent the idea from reappearing regularly, from Sismondi to Robert Theobald, whenever unemployment comes up for public discussion.

There follows a discussion of Leninism and revisionism applied to virtually the whole gamut of modern domestic and international economic questions. A critique of these points cannot be undertaken here, but in the reviewer's opinion the treatment is marred by gaps in the theoretical reasoning. In general Wolfson supports the revisionist view that collective bargaining and full employment policies remove the necessity or desirability of revolution. Wolfson's own opinions on the determinants of the distribution of income can be indicated by his argument that it is plausible that

... wages are negotiated on the basis of bilateral monopoly in which a single seller of a commodity confronts a single buyer of it . . . there is air of greater reality about a class bargaining wage contract than one which starts from the labor supply composed of individuals' willingness to work or rest and a demand by atomistic capitalists based on the increase in output they can obtain by hiring an additional employee (p. 114).

Consequently he is inclined to believe that the higher standard of living produced by capitalism is "in large part . . . due to the pressures of the organized labor movement itself" (p. 189).

If one were forced to choose between this model of wage determination, in this century and last, and "perfect competition" with which Wolfson contrasts it, this reviewer would opt for the latter. But of course one is not required to. There is a curious double standard that the author applies to Marx's theory and his own. Dealing with Marx he applies the supply and demand analysis of neoclassical economics. Considering that in the U.S. economy approximately one-quarter of the labor force is unionized and that obviously collective bargaining is broken up into thousands of separate bargains, why does this apparatus not apply?

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Goals, Priorities, and Dollars. By LEONARD A. LECHT. New York: The Free Press; London: Collier-Macmillan, 1966. Pp. xiv, 365. \$6.95; paper, \$2.95.

The main point of this "first study" is to provide information that will be helpful in charting the course of economic policy over the next decade. In it, the word goals is used synonymously with economic needs and aspirations. As indicated by chapter headings, they consist of the major components of gross national product; a number of great society programs such as urban development, social welfare, health, and education; and some miscellaneous objects of expenditure such as space travel, agricultural subsidies, and foreign aid. Confining goals thus to the objects of expenditure bypasses the confusions and policy conflicts that might derive from including such alternatives as price stabilization or economic freedom.

The result is a series of chapters which set forth facts and reasoning intended to justify estimates of the expenditures needed to achieve sixteen "goals" in 1970 and 1975. In each, tables are provided to compare actual data for 1962 with the projections to 1970 and to 1975 in constant 1962 dollars. These chapters in part embody the findings of a series of "research memoranda" prepared by members of the National Planning Association staff, all being rewritten by the author to fit a common pattern.

Throughout, the discussion conforms to the concept of progress implicit in GNP numbers, and underlying almost all of the estimates is the projection of trends, with heavy emphasis in the determination of those trends on the experience of the last decade. This in effect sets the study up as another in the NPA series of long-range projections, and it accordingly leans heavily on previous works, as indicated by numerous footnotes and by such expressions as "a high consumption economy." No reconciliation is made to adjust these figures to the estimate of feasible production, which, on the basis of a growth rate of 4 per cent is estimated at almost \$1,000 billion in 1975. For this reason, the specific numbers provided may be somewhat less useful than those in the previous volumes.

The general procedure is first to project a base below which future expendi-

tures should not fall by adjusting 1962 totals upward for changes in population, and then to calculate the goals in terms of the improvements over 1962 standards which we may expect to achieve in the next decade. It is apparently assumed that we shall be pursuing enough of these goals so that effective demand will at least match our production capabilities. Except for this assumption, the problem of how we are to get there is ignored.

The basic conclusion is twofold: we are not bound by any stricture of scarce resources from achieving further great progress in meeting our wants; but we cannot have everything because the total needs exceed our production capabilities. If anything, the report seems to lend stronger support to the former position; for the total of our "aspirations goals," with much irrelevancy and waste in the estimates, exceeds feasible production by only 15 per cent.

This last impression is enhanced by the fact that this medley of "guesstimates" and proposals projects major percentage increases in everything except national defense and agriculture. The costs of remedying social disutilities, such as urban sprawl, with its excessive transportation requirements, or air and water pollution combine with better diets and living quarters for the poor as parts of a single grand total. There is a naïve acceptance of the world as it is and may be expected to develop. Everything goes, provided only that it conforms to established norms of acceptability. To illustrate: "Expectations of steadily rising living standards . . . are cultivated by mass advertising, by the easy availability of consumer credit, and by the emphasis on "getting ahead" or at least "keeping up with the Joneses" (p. 53). Putting up everything that adds to the dollar total, without qualitative judgments about what contributes to welfare, seems to embody an embarrassing lack of discrimination.

There is hardly any attempt to resolve potential conflicts. "Consumer information services" are proposed, and their cost is added to the consumption total, without regard to the fact that their main usefulness should be the reduction of other items that go into current consumer spending. The chapter on housing states, "experience since World War II points to the large merchant builder as the strategic element in furthering innovation in residential construction" (p. 208); but then ends with the pious hope that spending for housing "is more likely to develop a meaningful pattern for urban living if it is pursued as part of an overall development plan for our metropolitan areas" (p. 214).

In short, the book is in no sense a plan for the realization of our goals. There is hardly any effort to discover or devise solutions, and the problems themselves are described mainly as a justification for extending the expenditure estimates into new high territory. The failure to be positive is aggravated in two ways: first, by excessive use of rather vague statements that something "could be done" or "would open up opportunities for," even though specific recommendations may be intended; and, second, the use of the collective "we," or "our," in contexts where it is difficult to distinguish the NPA staff from such others as the "consensus of national leaders," the "experts" in a given field, or "knowledgeable people" in general.

These defects might have been avoided at least in part if the book lived up to the promise in the "Priorities" part of its title. Despite the fact that it

embodies the work of the Center for Priorities Analysis, it says nothing about priorities beyond specifying that priority decisions will have to be made.

Nevertheless, the volume as a whole evidences the growing need for planning under conditions of sustained prosperity in an increasingly complex economy, and the chapters on goals point to diverse activities in various fields that are at least partial attempts to foresee and resolve social and political as well as economic problems. The experts everywhere who are undertaking this role mostly speak in protest against what is wrong and, less often, but still commonly, give analytical attention to how conflicts may be resolved. The present study would have been much better if it had dared to be forthright in following their example.

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Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

The Flow of Capital Funds in the Postwar Economy. By RAYMOND W. GOLDSMITH. New York: National Bureau of Economic Research, distributed by Columbia University Press, 1965. Pp. xxi, 317. \$7.50.

This book, the latest in the long and distinguished series of monumental statistical studies by Raymond Goldsmith, principally summarizes much of the work of the National Bureau's Postwar Capital Market Study. It draws on the immense bodies of data assembled by the large number of researchers and assistants who worked under Goldsmith's direction in the attempt, largely successful, to bring up to date and to extend Goldsmith's pathbreaking data estimates in *A Study of Saving in the United States* (Princeton, 1956) of stocks of wealth and flows of saving and investment classified by principal balance sheet item and by major transactor group. The more recent data-gathering efforts were also, and perhaps primarily, devoted to estimating experimentally a complete flow-of-funds matrix for the United States on both annual and quarterly bases as well as a detailed national balance sheet in order to provide "considerably more detailed, more accurate, systematically arranged, and integrated data on gross capital market flows, asset holdings, and interest rates. This is the field to which the National Bureau's Postwar Capital Market Study has tried to make a contribution to which this report is limited" (p. 74). The book mainly focuses on the data of capital market flows and asset holdings. It contains no detailed or systematic presentation of interest rate data but does make several attempts to use the flow and the balance sheet data. In the main these attempts are unsuccessful.

The data, presented in 110 tables and 27 charts and discussed throughout the book's ten chapters, are but a sampler of, and introduction to, the vast body of data now available in other publications of the Postwar Capital Market Study, notably *Basic Data on Balance Sheets and Fund Flows* (Princeton, 1963), the R. W. Goldsmith, R. E. Lipsey and M. Mendelson, *Studies in the*

National Balance Sheet of the United States, Vols. 1 and 2 (Princeton, 1963), and the David Meiselman and Eli Shapiro *Measurement of Corporate Sources and Uses of Funds* (Princeton, 1964), as well as the data now assembled and reported on a current basis in the Federal Reserve Board's flow-of-funds system of social accounts (see also *Flow of Funds in the United States 1929-53*, Washington 1955). The book, however, sets out to be more than an encyclopedic set of tables, for it also attempts to place the 1945-1958 postwar experience in historic perspective as well as to analyze many of the features of capital markets in the United States. The book compares some of its findings with earlier U.S. experience previously reported by Kuznets in *Capital in the American Economy: Its Formation and Financing* (Princeton, 1961) and Goldsmith in *A Study of Saving and Financial Intermediaries in the American Economy since 1900* (Princeton, 1958). This sense of historical relevance, one of the outstanding features of other Goldsmith efforts too, should be especially valuable to the new generation of economists and financial analysts—and to their teachers—whose knowledge of the pre-1929 financial and economic world generally rests on stray bits of impressionistic and inaccurate hearsay.

Unfortunately, the contribution of all these data, and the accounting statements in which they are filed, to improved understanding of financial processes and to the analysis of the capital markets is disappointing, especially in view of the substantial investment in estimating flow-of-funds accounts and the long awaited application of these data to the analysis of financial and monetary behavior. These and related data have been here for more than a decade—at least since publication of the Federal Reserve's accounts in 1955. In the attempt to make the accounts more useful and useable additional detail has been added, the accounts have been put on a quarterly basis and adjusted for seasonal variation, and the findings published on a regular basis in the *Federal Reserve Bulletin*. Yet despite this vast and continuing outpouring of data from the Federal Reserve's flow-of-funds statistical salt mill, to the reviewer's best knowledge not a single important substantive contribution in the fields of money, finance, and investment behavior has resulted from the availability of the data or from the special accounting format used to assemble and to classify the figures. Indeed, the burst of creative and fruitful work in the past decade in these fields has generally bypassed the huge data-making effort, even though these data were meant to be used in the same quest.

Perhaps the nature of the data-gathering exercise explains the tonnage of the harvest but the meager value of the crop thus far. It has largely been measurement for its own sake, so that not even Koopmans' caricature of many National Bureau efforts as measurement without theory is an accurate description of the prodigious flow-of-funds data-gathering, for here we have had measurement without problem.

The sources and uses of funds accounting format and some of the purposes to which the accounting statements are put may puzzle some readers and mislead others. One reason is that it is quite incorrect to represent, as Goldsmith does in Chapter 4 and elsewhere, the ratio of each listed source to total sources as if the numerical proportions were also indicative of the relative importance of individual items in explaining decision-making or market phenom-

ena, for example, in asserting a share of total sources accounted for by retained earnings, bank loans, or the like.

It is difficult to interpret the numerical proportions themselves because the totals and most of the separate items in the sources and uses accounting identities are neither clearly nor uniquely specified. Instead they are often highly responsive to the availability of data and to accounting practices, which are also often arbitrary. For example, the item "Total Sources of Funds" really means something like the "Sum of the Items Listed Above." The sum itself, even in principle, is not a uniquely measurable quantity such as the tonnage of coal mined in the United States during the first quarter of 1967, or the value added by Peruvian corporations in 1911. Moreover, this sum does not even represent the empirical counterpart of an economic concept in the sense that Net National Product or other of the components of the National Income Accounts are alternative measures of aggregate income, except perhaps that "Total Sources of Funds" may be the empirical counterpart of something like the supply of loanable funds, a concept which also has many of the same problems of concept and of measurement. For an example of problems posed by individual items in the accounts, depreciation may be listed as a source of funds when gross capital expenditures is listed as a use of funds or, depreciation may be netted against gross capital formation on the uses side, giving net capital formation. The latter accounting practice alters the numbers in the accounts describing capital formation as well as the item, "Total Sources of Funds." For these purposes there is no special reason to choose one treatment rather than the other, which is why the sum itself has neither analytical nor substantive content. Further, because there are no standard accounting conventions generally followed in this area by social accountants—or private ones, either—nor need that the same accounting conventions be followed by the same social accountant in treating different sectors, aggregating the figures for different transactor groups compounds the difficulties. The number finally arrives at for "Total Sources of Funds" for the economy as a whole can be made to take on virtually any numerical value depending on the aggregation, the consolidation, and the netting procedures that are adopted, just as the analogous figure for "private debt" can be made almost any number depending on how consolidated the debt statement is, even up to the point of having net private debt equal zero when the private sector (in a closed economy) is placed on a completely consolidated basis.

No doubt the scanty substantive and analytical contributions thus far from the flow-of-funds data and format have come as a great disappointment to large numbers of economists and financial analysts, many of whom have long held out hopes that these accounts would be the analog for finance that the National Income Accounts have been for aggregate income in many simple Keynesian models. In these models the level of income is essentially a function of the relationship among its components. Aggregate income as well as the data necessary to analyze changes in aggregate income are found in the accounts, and by means of Keynesian theory the accounts become a self-contained, self-explanatory body of data. Indeed, much of the current structure of the National Income Accounts is rooted in these Keynesian analytical con-

cerns. Reciprocally, much of the development of the Keynesian analysis and its widespread appeal stemmed from the availability of these data. Whatever the merits of the simple Keynesian system happen to be, the flow-of-funds paradigm has unfortunately not yet worked out, perhaps because it was not appropriate in the first place. In the flow-of-funds accounts total sources (total uses) is not the analog of aggregate income and there is no theory how to relate components to predict the total.

The ten chapters of the book are divided into two somewhat unrelated parts. In one part are the initial four chapters, themselves largely unrelated, which are mainly devoted to a presentation of the rationale for having assembled the data. This part of the book also summarizes some of the statistical findings. The other part of the book devotes one chapter to each of the five main postwar capital market instruments: U.S. government securities; state and local government securities; corporate bonds; corporate stock; and residential mortgages.

Chapter 1 is an attempt to evaluate the economic functions performed by capital markets and to describe the main U.S. instruments and institutional participants, using data largely drawn from earlier Goldsmith studies. Chapter 2, labeled "A Framework for Capital Market Analysis," turns out to be devoted mainly to a discussion of the accounting and data gathering problems of constructing a multisector, multiasset set of national flow-of-funds accounts. The framework is little more than a partial set of accounting identities, and, following current fashion, some of them are expressed in symbolic form using algebraic notation. No hypotheses are framed and the accounting framework itself does not suggest any. Moreover, the framework never makes clear what specific problem or problems the social accounts are designed to explain, except perhaps to be the statistical filing cabinet holding the numbers for some vast Walrasian financial system full of "interesting" facts, but lacking the appropriate price and yield data associated with the quantities involved.

Chapter 3, "The National Financing Task: The Volume of Gross Capital Expenditures," presents data in both current and constant prices on the composition of gross and net capital formation over the 1946-58 period classified by principal type of capital good and by acquiring sector. The chapter ends with several interesting tables comparing several trends in net capital formation of the 1946-58 period with earlier periods in American history, some as far back as 1869. Although some of these data are informative and provocative, I have some misgivings about Goldsmith's nomenclature when, in defining his terms at the opening of the chapter he states, "In a closed national economy, the financing task can be reduced to diverting just enough funds from current income to equal total gross capital expenditures on tangible assets which have a length of life exceeding the accounting period of one year and usually set at somewhere around two or three years" (see p. 80). This statement derives from the identity that total uses equal total sources, uses here defined as the sum of gross capital expenditures plus acquisition of financial assets, and sources here defined as gross savings (the sum of depreciation and net savings) plus external sources, all consistently defined. Thus, gross capital expenditures identically equals gross savings plus external sources. In

a closed economy with no government, the identity reduces to the familiar investment-savings identity, which, of course, tells us virtually nothing about aggregate *ex ante* investment decisions, *ex post* capital formation, interest rates, share prices, and so forth. In addition, not only does the terminology suggest some confounding of resource use and financial or accounting practice, but the "task" gives no role to or cognizance of such contemporary phenomena as large-scale consumer credit, government budgetary deficits, and Federal Reserve activities as central bank and as regulatory agency.

Chapter 4, "Internal and External Financing," discusses the financing of capital expenditures in the descriptive context of this form of sources and uses of funds accounting statement, but with more detail for the U.S. economy which is broken down into seven sectors: (1) nonfarm households; (2) agriculture; (3) unincorporated business; (4) nonfinancial corporations; (5) finance; (6) state and local governments; and, (7) federal government. The additional accounting detail includes a finer breakdown of financial uses among short-term, long-term plus equity sources, and also with a separate item for inventory profits.

The fact that the sources and uses of funds accounting statements often include a separate line for capital expenditures does not necessarily mean that the accounting statement *per se* is an efficient means for explaining the volume of investment outlays. In essence, the chapter is a "write-up" of the sources and uses accounting numbers, again in useful historic context, but with little analytical or descriptive relationship to the large and growing body of important studies of capital formation which have been made over the past decade.

The second part of the book consists of one chapter devoted to each of the principal capital market instruments, plus a summary chapter. The specification of the capital market follows conventional lines, so that detailed treatment of such short-term instruments as commercial paper, bank loans, and the like is excluded. Several of these chapters are based on published Postwar Capital Market Study monographs, the Roland Robinson *Postwar Markets for State and Local Government Securities* (Princeton, 1960) and Saul B. Klamman's *The Postwar Residential Mortgage Market* (Princeton, 1960), both of which include much analysis of market behavior as well as a large amount of institutional detail. Goldsmith's attention is primarily directed to the questions of who is issuing what to whom and who is acquiring what from whom. Although these discussions and the accompanying tables and charts pay some attention to institutional detail, the primary emphasis is on recounting the details of the flow-of-funds and national balance sheet accounts. As elsewhere, the emphasis is on quantities; prices and yields receive scant mention, either as causes or effects.

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Commercial Bank Behavior and Economic Activity. By STEPHEN M. GOLDFELD. Amsterdam: North-Holland Publishing Co., 1966. Pp. 212. \$7.50.

Professor Goldfeld's book describes a 32-equation, quarterly model of the U.S. economy, estimated on data from mid-1950 through mid-1962. Goldfeld

explains that his primary motivation in constructing the model was to "examine commercial bank portfolio behavior, to relate investment and consumption expenditures directly to financial variables, and to investigate the impact of monetary policy on both financial and nonfinancial variables." Accordingly, Goldfeld first presents a group of equations dealing with commercial banks. These explain changes in bank holdings of excess reserves, borrowed reserves, short- and long-term governments, holdings of tax-exempt securities and the posted rates on commercial and industrial loans. There are separate demand equations for country and city member banks. Goldfeld's disaggregation of free reserves into excess and borrowed reserves and by bank class represents a refinement over earlier treatments. The author finds a speed-of-adjustment coefficient of about unity for city bank holdings of excess reserves and about 0.6 for country banks. Speeds of adjustment for borrowed reserves for both classes of banks, however, remain rather surprisingly slow as they have in earlier studies using a simple stock-adjustment model. It might be noted that Goldfeld's equations for bank holdings of the various kinds of securities do not seem to feed back on the other equations of his model.

A second group of equations seeks to explain quarterly changes in the non-bank public's holdings of coin and currency, demand deposits, time deposits, and in its demand for commercial and industrial loans. There are also two term-structure equations explaining the spreads between bills and intermediate and long-term governments. The nonfinancial sector of the economy is represented by equations for fixed investment, inventory investment, and durable and other consumption expenditures. Other components of GNP are treated as exogenous (as are total private holdings of short and long governments and municipals). The model is closed with the appropriate GNP and banking identities. In the first part of the book Goldfeld experiments with some alternative specifications of the various behavioral equations using ordinary least squares. The final model, which contains a number of nonlinearities, was estimated by two-stage least squares.

The financial sector is hooked into the real sector through the presence of changes in commercial and industrial loans and the long-term government rate (multiplied by current GNP) in the fixed investment equation, and by the presence of the business loan rate and changes in commercial and industrial loans in the inventory equation. The coefficients of these variables are all larger than twice their respective standard errors in the equations finally selected by Goldfeld, and he regards them as "important determinants for both fixed and inventory investment." In addition, the level of the narrowly defined money supply proves significant in the equation for durable consumption outlays. Neither of the final consumption equations contains an interest rate variable. Oddly enough, Goldfeld does not have a separate equation for residential construction, evidently lumping this GNP component in with his exogenously determined GNP residual. Another—less surprising—omission is any attempt to measure the influence of financial conditions on state and local outlays detected by several investigators. In any event, the effects during a given quarter on key endogenous variables of given changes in monetary and fiscal policy variables occurring during the same quarter are derived from the reduced

form of the model linearized at 1962 values of the variables. Goldfeld notes that where comparisons were possible, the results were generally consistent with those of de Leeuw's work in the same vein.

In constructing this model covering a rather large collection of financial and real variables, Goldfeld has had to explore an extensive body of literature in such widely diverse fields as the determinants of member bank borrowings, the market for commercial and industrial loans, and the demand for capital goods. He appears to have done his homework very thoroughly. In the banking field at least, where most of the reviewer's experience lies, he seems to have recognized and attempted to face a remarkable number of the major relevant issues outstanding. Moreover, the author handles the various estimating problems which his model presents with evident finesse and the results in terms of R^2 s, " t " values, and Durban-Watson ratios are generally quite good—though he notes difficulties in applying some of the standard tests to a nonlinear model estimated with two-stage least squares. Finally, the exposition is uniformly lucid. All in all, the book represents an impressive achievement, and anyone interested in the influence of monetary variables on the real economy will want to consult it.

While Goldfeld's work presents an excellent approach to a quantitative treatment of the linkages between monetary and real variables, however, it does not focus much attention on the resolution of existing controversies in this area. One would like to know, for example, why he chose to derive a demand for money function from the Tobin-Teigen formulation rather than from other possibilities that have proved successful elsewhere, or exactly why he gets an important influence of interest rates on fixed investment when some other thorough investigations have not. No doubt a project that would have provided answers to all such questions for a model of the size and complexity Goldfeld constructs would have been an impracticably large task for any single author. Nevertheless, so many conflicting results have been published in some of these areas that a painstaking competitive testing of the outstanding alternatives (together with checks on the robustness of the results in the face of variations in the sample period) would now appear essential if a solid basis for choice is ever to be established.

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Mexican Financial Development. By DWIGHT S. BROTHERS AND LEOPOLDO SOLÍS M. Austin: University of Texas Press, 1966. Pp. xi, 236. \$7.50.

Mexico in the past decade has experienced both economic growth and stability, a substantial achievement for any country, and for a developing country located in Latin America it takes on the proportions of an economic miracle. It is no wonder then that this event has been accompanied by the recent appearance of a number of books on various aspects of the Mexican financial system.

This latest book, the result of a collaboration between a Mexican and an American, is divided into two principal parts. The first part is a concise well-written history of the development of Mexican financial institutions and pol-

icies for the period 1940-1960. The second part consists of an analysis of these developments. There is also an introductory chapter on developments prior to 1940 and a brief concluding chapter listing current Mexican monetary problems which is only loosely integrated with the main parts of the book.

The two most striking developments in the Mexican financial structure in the period on which the authors concentrate is the government's decision to shift the financing of the public sector's deficit from the Central Bank to the private financial system and the extraordinary growth of the *financiera* (a type of investment bank). The financing of government deficits with the private financial system helped reduce the inflationary impact of these deficits even though the central bank had to step in from time to time to support the banks and other financial institutions. The important consequences of this change in public debt management policy are only lightly touched upon. The new policy of private government financing resulted in a substantial expansion of the financial system. By largely expropriating the growth in the money supply to finance the deficit of the public sector it reduced the major source of inflationary pressure. The improvement in financial stability however contributed to the increase in the rate of private saving, a large part of which the expanded financial system was able to channel into public and private investment so that part of the increased demand of the public sector for the credit of private financial institutions was offset by increased private saving. The Mexican policy produced a successful type of "forced investment" in the public sector of voluntary saving in contrast to past unsuccessful attempts at financing the public sector through "forced saving."

However, this policy was not an unmixed blessing. To the extent that it was successful the government preempted a larger amount of the resources of an already small capital market than it otherwise would have, had it resorted to an alternative policy of increasing fiscal revenue. Also the larger proportion of private savings channeled through financial intermediaries probably (there are no statistics) resulted in a greater concentration of industry and income because of the close relationship between particular financial institutions and large industrial groups. The greater investment by these large industrial groups and also by the government in capital intensive projects may, in part, have resulted in the reduction in the marginal productivity of capital upon which the authors place great weight in explaining the slowdown in growth at the end of the 1950s. While little is known of how the private sector is financed in Mexico because of the failure of the official financial statistics to identify a private sector as such, it is widely believed that medium and small businesses and farmers were faced with greater credit stringency than other groups in the economy and had to rely largely on self finance.

All the elements of this story are to be found in the book; however, the authors have carefully smoothed off all the rough edges of their arguments which might provoke controversy, and a reader without some familiarity with the Mexican scene may easily miss some key points.

The growth of the private *financieras* is explained largely as a result of the restrictions placed on bank portfolios by the Bank of Mexico through legal reserve requirements. The banks and other groups organized *financieras* which

were free of these restrictions. They could earn a higher yield on their assets and pay a higher rate of return on their liabilities. Their liabilities, being pegged at par and ultimately supported *de facto* by the Bank of Mexico, proved extremely attractive investments in a period of relative financial stability. The exceptional liquidity of the liabilities of the financial system made financial stability not so much a choice as a necessity.

The authors present a convincing model to explain the failure of counter-cyclical monetary policy in Mexico before 1954. They point out that major fluctuations in the level of domestic expenditure were dominated by the behavior of the export sector and that Mexican wealthholders were very sensitive to the risk of devaluation. Any attempt by the monetary authority to offset reductions in domestic expenditure of external origin resulted in an increase in imports and a worsening of the balance of payments. This induced devaluation-fearing Mexicans to take their liquid capital abroad and reduced the level of international and bank reserves restoring the rather stable relationship between the money supply and income.

The authors' explanation of the success of the expansionary monetary policy followed after 1954 is less complete. They believe that the Bank of Mexico came to recognize the limitations set by the process described above and, as a consequence, directed monetary policy toward maintaining internal and external equilibrium and employed selective credit controls and specialized credit institutions to achieve its development objectives. They do not give sufficient emphasis to the change in the government's policy toward private foreign investment and the over-devaluation of the peso in 1954 which yielded a substantial margin of safety in the balance of payments in the following years.

I personally was disappointed by the failure of the authors to bring to light any new data (such as that prepared for the Mexican Development Plan and its evaluation by the Committee of Nine and World Bank), which would remove the public discussion of Mexican financial problems from the realm of speculative economics. The authors rely on well-known sources containing many serious deficiencies which are meticulously described. As partial consolation, however, the book does contain an excellent bibliography on the Mexican financial system.

This reviewer's exercise of his privilege of being critical should not detract attention from the fact that this book by Brothers and Solís is the best general introduction to the Mexican financial system and its development available in English, one that both new and old students of the fascinating Mexican experience can read with profit.

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Theorie der Geldpolitik. By ALFRED BOSCH AND REINHOLD VEIT. Tübingen: J. C. B. Mohr, 1966. Pp. 104.

Inflation in world markets, attempts to maintain internal price stability, the preservation of equilibrium in the foreign exchange market with fixed exchange rates, international liquidity, insistence on a full-employment economy, and an autonomous wage structure—these diverse elements bedevil mone-

tary managers and make monetary policy a controversial subject. Perspectives on these problems may differ depending on one's situation. Here we have an excellent presentation of one European point of view, written from the perspective of a country embarrassed by the acquisition of reserves rather than by their loss.

The principal thesis of this book is developed, initially, on two assumptions: namely, that a country produces only internationally traded goods, and that the demand for money is the classical case where the desired cash balances are a certain percentage of the volume of transactions. From the first of these assumptions, it follows that prices are determined on the world market. A country that is small in relation to the rest of the world cannot determine its price level. In every such country there is a certain quantity of money which will bring the balance of payments into equilibrium. Because of the second assumption, this quantity can replace the transactions-balance demand for money. The quantity which results in balance of payments equilibrium is designated by the authors as the equilibrium quantity.

If world prices rise because of inflationary forces outside a country, and if the balance of payments is to remain in equilibrium, the monetary authorities of a relatively small country must increase the quantity of money. Should the quantity of money not increase, then the additional quantity of money necessary for equilibrium will be acquired by the development of an export surplus. Should the monetary authorities create more than the equilibrium quantity, the excess would flow to other countries in the form of a deficit in the balance of payments.

The initial assumptions are later modified by introducing goods which do not enter into international trade, and also international capital movements. These modifications do not, in the authors' opinion, change the essence of their theory. Thus, they conclude that monetary policy determines only the balance of payments, not the price level. Monetary policy has no influence on the level of employment, since the level of employment at the internationally determined price level depends on the wage policies in each of the countries involved. Briefly, the price level is internationally determined; the level of employment determined by wage policies; and the balance of payments determined by monetary policy.

This point of view directly contradicts what is generally considered correct by monetary authorities of the Federal Republic of Germany. In developing their theory, the authors comment on monetary policies in Germany, Great Britain, and the United States. They also state their views on the development of the international monetary equilibrium problem; and they advocate, finally, the creation of an international monetary authority on whom would be pushed the problems that are hard to solve a priori.

This small volume is written with great clarity and simplicity. The thesis is provocative and should stimulate further discussion. German monetary controversies gave rise to the inquiry. On one side of the dispute—if I may put the divergence in its sharpest form—are those who believe that a country should be able to keep its price level stable, even with a fixed exchange rate, in spite of an inflation in the rest of the world. According to this book, an

inflation is "imported" into a country by an excess of exports on the part of the country trying to keep its price level stable. This export surplus may compel the central bank to accumulate foreign exchange and this may increase the liquidity of the commercial banks, expand the quantity of money, and thus lead to inflation. However, if the central bank should compensate for this increase in liquidity, if it should sterilize the additional reserves resulting from additions to foreign exchange holdings, then it should be possible to stop the internal inflation. This is the supposition which, by and large, the German Bundesbank follows. The authors belong to a group which insists upon the direct influence of world prices on the domestic market; and, presupposing, as always, fixed exchange rates, consider as insignificant the ability of the central bank to defend itself against inflation.

Both views probably fail to make a correct appraisal of all the facts. It is an undoubted service to insist upon the fact—which is almost constantly neglected—that an inflation abroad has a direct influence on the domestic price level. This the authors have done; but they may have gone a bit too far. In their own numerical example (p. 22) where domestic goods are introduced into the analysis, it is evident that, in spite of the world inflation, the price level in a small country remains for a long time below that of the rest of the world. This is so even though it is assumed that the quantity of money does increase as a result of the export surplus. The period during which the domestic price level remains behind the world price level can be lengthened considerably, if the central bank manages to prevent the influx of money from having its usual effects. Certainly a country in such a situation cannot preserve a lower price level forever, because the country cannot afford to put at the disposal of other countries a constantly increasing surplus of exports; nor can the central bank over a long period prevent the expansive effects on the monetary system of export surpluses. For a considerable period of time, however, the restraint exercised by a country's monetary authorities can succeed; and a country has, besides, an influence in damping world inflation through the repercussions which arise when it draws to itself international reserves.

The authors of this book do not deny the points here raised in criticism of their thesis. They weigh the factors differently; and, in expounding their principal argument, minimize the significance of these factors. The two extreme positions on this matter can be unified into one more general theory, *if* one separates the short-run point of view from the long-run. Then the proposition of these writers that one country must acquiesce in a world wide inflation can be designated as the long-run point of view.

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Money, Banking, and the Economy. By JOHN A. COCHRAN. New York: Macmillan, 1967. Pp. xvii, 605. Prob. \$8.95.

There is relatively little that one can say about a competently done undergraduate text; thus, the reviewer is forced into one of the three major varieties of "reviewermanship." The first, and clearly the most satisfying, is to mount a

vicious attack upon the author's lineage, sobriety, and competence—concluding the review with a single pithy epithet, aptly chosen to doom the book and the author. The second is to demonstrate vast patience and scholarship by counting misspelled words, improper usage, faulty footnotes, etc. The third is to compare the book at hand with the one the reviewer would have written, had he the inclination, or the time, or the necessity. I shall endeavor to unite these three approaches within the constraint so cruelly imposed by the length limitation.

The issue is, of course, whether there are important reasons to prefer Professor Cochran's text to the others in the field. On balance, Cochran's book will be a sound competitor. The historical and institutional material is superb—the second and third chapters efficiently dispose of what we need to know about the history of monetary standards and the current American standard. The next seven chapters deal with commercial banking questions; they are well organized, well written, and very much up-to-date. However, of particular interest are the next three chapters, a section on money and the capital markets. Chapter 11, entitled "Flow of Funds in Financial Markets: Sources and Uses of Funds," presents a concise and competent summary of the mysteries of that social accounting system and Chapters 12 and 13 do a fine job on the money market, bringing out forcefully recent innovations of importance. In short, the first half of the book is good, possibly excellent.

The main problem arises in Parts IV and V—dealing with "Central Banking" and "Monetary and Income Theory." Chapter 15 in the former section deals with Federal Reserve credit and commercial bank reserves and, in my opinion, emphasizes and details the wrong things. Perhaps it is true that the contributions of Meigs, Brunner and Meltzer, Polakoff, Dewald, and others need not be treated in an elementary text on monetary economics (I think they should be), but at the very least they should be mentioned in selecting a bibliography for further reading. Instead, Cochran treats the serious student to seven references to bank reviews. Cochran also gives us a rather confusing picture of member bank borrowing. On the one hand, when "... the discount rate is markedly below the short-term Treasury bill rate, it might seem apparent that the Federal Reserve might be trying to encourage member banks to borrow..." (p. 294). On the other hand, member banks "... are supposed to feel themselves under pressure when they are borrowing..." (p. 290). A straightforward introduction of the "profit" and "reluctance" hypotheses, along with the admission that current empirical work has not yet sorted out the two effects very well, would have been preferable. Also, the discussion of the effect of discount rate changes on market expectations lacks precision.

The role that money plays in national income analysis is treated in one short chapter (Ch. 17). Despite the fact that such material as short-run vs. long-run consumption functions and the "super-multiplier" is introduced, the chapter omits presentation of a simple Keynesian equilibrium—yet, question four on page 338 asks something about the LM schedule, nowhere explicitly introduced. Although it probably is true that Keynesian comparative statics is adequately covered in most principles texts (and, we hope, courses), considerably more could be expected in a money and banking text—for example, some

of the recent econometric work on the demand for and supply of money could usefully be introduced at this point (or in Ch. 20, "The Quantity Theory of Money Revisited").

The dust jacket says that "economic growth is given particular attention." This statement presumably refers to Chapter 18, which is entitled "Economic Growth." Harrod, Domar, and Rostow are discussed, but this seems out of place in a book devoted to monetary matters. The only thing that the monetary authorities must do in this connection, apparently, is to decide "... how much reserves to furnish to the banking system in order to permit a certain rate of growth in the money supply ..." (p. 339). We do not, unfortunately, have much more to say in this neglected area, but it seems to me that one need not "drag in" the topic if that is the case.

The last part of the book deals with various public policy issues and is exceedingly well done. Chapter 23, on "Fiscal Policy and Debt Management" explains the three types of government budget clearly and concisely and raises a number of salient questions about the management of our ever-expanding public debt. Chapter 24 discusses the public policy proposals of the Commission on Money and Credit, a welcome resuscitation of moderate reform proposals that have largely been ignored. Chapters 26 and 27 deal with international monetary questions and institutions and Chapter 28 raises some "final questions" about "Money and Banking in an expanding economy."

In summary, Cochran has written a text that deserves serious consideration for the elementary monetary economics course. It will probably be selected by those who wish to stress institutional material; it is probably the best available text from that point of view. There are also other favorable selling points, but the paucity of theoretical material is a serious shortcoming. In closing, since Cochran opens each chapter with a selection from *The Wealth of Nations*, let me say: "... I am always willing to run some hazard of being tedious in order to be sure that I am perspicuous; and after taking the utmost pains that I can to be perspicuous, some obscurity may still appear to remain upon a subject ..."

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Public Finance

Effects of Corporation Income Tax: Papers Presented at the Symposium on Business Taxation. Edited by MARIAN KRZYZANIAK. Detroit: Wayne State University Press, 1966. Pp. 268. \$8.95.

Of the numerous uncertainties that plague fiscal economists, few can rival that concerning the incidence of the corporation income tax. The second most important tax in the country, it is believed by some to burden corporations and their shareholders while others argue that it is largely, or entirely, shifted forward onto consumers or backward onto workers. Nor is this the full extent of expert disagreement. In their 1963 econometric study [9], Marian Krzyzaniak and Richard A. Musgrave suggested not only that corporate tax increases

tend to be overshifted but that corporate tax decreases may be significantly undershifted. This raised the intriguing prospect that corporate stockholders could become better and better off if only they could persuade an understanding legislature to keep raising and lowering their tax rates at suitable intervals of time. Such startling results were not likely to remain undiscussed for long, and the volume of papers under review here is the first in what may well be a long line of studies dealing with the incidence of the corporation income tax. In it the policy-maker will find only a few, rather tentative, answers, but the economic researcher will want to consider seriously many of the points made before embarking on his own analysis of the effects of the corporation income tax.

In the leading and longest paper in the book Krzyzaniak skillfully manipulates four long-run, neoclassical growth models, using Cobb-Douglas and CES production functions to determine the impact of both general and partial profits taxes on the rate of return earned on capital and on the share of profits in total national income. The analysis begins with a useful section on "Indicators and Measures" (pp. 25-29) in which precise definitions of different degrees of tax shifting are derived, and throughout the subsequent discussion careful attention is paid to the sensitivity of the shifting process to different values of the important economic parameters, such as the elasticities of substitution between factor inputs and, in the case of a partial profits tax, between the products of the taxed and nontaxed sectors of the economy. The results, which indicate a high degree of shifting in the long run resulting from a tax-induced decline in saving and investment, are difficult to evaluate, however, because of the patently unrealistic assumptions made in the mathematical models. Moreover, everything depends on the nature of the impact of profits taxation on incentives and abilities to invest, and, because of the long-standing work of Domar and Musgrave [1, 10] and the more recent analyses of Hall and Jorgenson [3, 4], one may question Krzyzaniak's basic assumption that profits taxation necessarily does depress society's saving rate. Be this as it may, it is interesting to note the extreme slowness with which the shifting process operates in the models discussed. It would be a far-sighted politician (or statesman) indeed who would be seriously concerned with economic effects that required 40-50 years to achieve as much as one-half of their full potential, and over 100 years to reach 90 per cent of it!

In a short paper drawing on theoretical and empirical work that he and his associates have published, or are about to publish, elsewhere [5, 6, 7], Harberger derives a rough, quantitative measure of the efficiency loss resulting in this country from differential intersectoral tax rates on income from capital. Simplifying an exceedingly complex situation into one involving only a highly-taxed, mainly corporate, sector with an average 1953-59 tax burden of 62.7 per cent of gross income and a lightly-taxed, mainly noncorporate, sector with a 1953-59 tax rate of 31.0 per cent, Harberger estimates that this tax differential induced a 17-30 per cent capital outflow from the corporate sector (i.e. a decline in the capital stock from \$23.5-\$27.5 billion to \$19.5 billion), which in turn imposed an efficiency loss lying, to take his most probable case, between \$1.5 and \$2.5 billion a year. These findings, tentative as they are,

constitute a welcome innovation in this area of tax analysis. Future research may confirm or deny the existence of a relatively large tax-induced shift of capital into such industries as oil and gas, residential housing and agriculture (Krzyzaniak himself makes an inconclusive attempt in a long footnote on pp. 50-51 of his paper), but in the meantime Harberger's analysis reinforces the importance of knowing more than we do about factor and product elasticities of substitution and about the effects of disaggregation on the sizes of efficiency losses (or gains) from resource reallocations.

Another short paper, by Musgrave, presents a taxonomic analysis of the "Effects of Business Taxes Upon International Commodity Flows" that points up some of the costs of our present uncertainty concerning the incidence of production, consumption, and profits taxes. If these are shifted forward to consumers, neutralizing fiscal adjustments may be desirable to offset the effects on commodity flows of production and corporate profits taxes; if, on the other hand, tax burdens remain on, or are shifted backward to, factor incomes, both profits and general production taxes would be neutral in their impact on commodity flows. Unfortunately, as Musgrave notes, rigorous empirical tests of these conflicting propositions are still very much in their infancy, and as a consequence trade policies are likely to remain controversial for some time to come.

The remainder of the book—some 125 pages—is devoted to a critical evaluation of the Krzyzaniak-Musgrave (hereafter K-M) study of corporate tax incidence [9] by Richard E. Slitor and Richard Goode, including a defense by K-M and a rejoinder by the two critics. It has been some time since a debate of this sort has enlivened the pages of fiscal economics, and though no final answers are provided, many valuable guidelines for future studies are laid down. The attack on the K-M doctrine that increases in the corporate income tax are overshifted in the short run is developed along three major lines. The first involves a severe, and on the whole, convincing, criticism of the validity of the K-M theoretical model. Goode notes that one of the dependent variables, the rate of return on corporate capital, is subject to large errors of measurement which may have a significant effect on the empirical estimates obtained, and both Slitor and Goode complain about the imprecision in the definition of the variable used to measure effective tax rates. By including excess profits taxes in its numerator and the losses of deficit companies in its denominator, they argue, the tax variable fails to isolate the rate changes that can logically be expected to induce corporate attempts at tax shifting. Still more disturbing, however, are the questions raised concerning the three nontax independent variables used in the basic K-M model. To Slitor the prior-year change in the ratio of consumption to GNP is only a "pseudo-accelerator," and Goode wonders why, if the variable does represent the acceleration principle, it was not defined as the ratio of the change in consumption to GNP, or as the rate of change in GNP or in some other general measure of economic activity. The second independent variable, the prior-year inventory to sales ratio in manufacturing, is a plausible measure of the pressure of costs on profit margins, but, understandably, Goode concludes that ". . . on economic grounds, the one-year lag is curious" (p. 214). The third variable, the current-

year ratio to GNP of federal, state, and local noncorporate taxes, minus government transfers to domestic recipients and grants-in-aid to state and local governments, is perhaps the most perplexing of all. On the one hand, it might represent the impact of rising (or falling) governmental operations on profits and hence have a positive coefficient, and on the other, it might reflect the negative influence on corporate profits of higher noncorporate taxes, government expenditures being taken as given. In the K-M study it is the latter result that apparently emerges. All in all, it appears that the K-M theoretical model emerges from this attack badly shaken, if not largely demolished.

The second major criticism of the K-M study is based on the familiar econometric problem of multicollinearity. Have K and M, the critics ask, really isolated the effects of the tax variable from the effects of those other variables that influenced corporate profit rates during the period of analysis and were also correlated with corporate tax rates? Is not the estimated size of their tax rate coefficient highly sensitive to the specifications of the empirical model? On neither question is there a meeting of the minds of the disputants. Slitor does test the validity of his criticisms by using an "economic pressure" variable (the Knowles ratio of actual to potential GNP [8]) to supplant or supplement the independent variables used by K and M in their basic model, but the results he obtains, indicating a drop in the shifting measure from 134-141 per cent to 87-99 per cent, are ambiguous. They do, to be sure, show a considerable degree of instability in the shifting coefficient, but they do little to refute the K-M conclusion that the corporate profits tax is fully shifted in the short run. In addition, the disputants argue inconclusively over whether or not inclusion in the model of an unlagged pressure variable creates a simultaneous-relations problem and hence results in biased statistical estimates.

The final set of criticisms concerns the plausibility of the K-M findings. To Slitor shifting on such a scale ". . . long- or short-run, defies credulity" (p. 167), and Goode finds it hard to believe, as do I, that had corporate tax rates remained constant between 1936-39 and 1955-57, the before-tax rate of return on the total capital of manufacturing corporations would have *fallen* between the two periods. Yet the K-M model predicts, for example, that if 1955-57 tax rates had applied in 1936-39, the before-tax rate of return would have been 16.66 per cent, compared to an actual rate of 7.46 per cent in 1936-39 and one of 16.33 per cent in 1955-57 (p. 217). Similarly unorthodox is the K-M "ratchet effect" under which profits tax changes fail to reduce corporate income on the way up but do augment it on the way down. The latter finding, however, is based on a single, and rather special, statutory tax rate decrease—the pre-announced removal of the Korean excess profits tax in 1954—and hence, in the words of Slitor, ". . . it hangs by a statistical thread" (p. 171). Another aspect of the general plausibility test concerns the "price umbrella" effect that is implied by full and rapid shifting of corporate tax increases. Here the argument is that profitable firms, by increasing their sales prices and gross profit margins in order to shift the tax, will provide inefficient firms with a better economic environment in which to continue to operate. Slitor tests this hypothesis by relating changes in the relative size of the deficit corporation group in manufacturing to tax rate changes for four periods between

1926-29 and 1957-60 (pp. 179-83). The results, however, are inconclusive because of uncertainty as to how much of the negative correlation found should be attributed to the tax rate changes rather than to other factors.

Perhaps the chief contribution of the original K-M study and the critical discussion of it in this book will be the stimulus provided to other researchers. Evidence to this effect is already available. A study by John G. Cragg, Arnold C. Harberger, and Peter Mieszkowski, prepared for a Brookings volume on "The Taxation of Income from Capital" [7], deals imaginatively with both the collinearity and the simultaneous-relations problems and emerges with statistically insignificant coefficients for the tax rate variable. Using a fully articulated theoretical model and sophisticated statistical estimation techniques in a paper to be published in this journal [2], R. J. Gordon also finds no evidence of corporate tax shifting in the short run. What one believes about this phenomenon, therefore, depends largely on which econometric analysis, if any, he regards as definitive. The only safe conclusion is that there will be many more attempts to unlock the enigma of the corporate profits tax.

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Federal Tax Policy. By JOSEPH A. PECHMAN. Washington, D.C.: The Brookings Institution, 1966. Pp. xxi, 321. \$2.45.

This book is intended for the "interested citizen." The citizen would have to be interested indeed to work his way through the detailed descriptions of each tax, the arguments for and against proposed changes, and the vast

amount of historical and statistical material. The greatest beneficiary of the volume is likely to be the writer of economics textbooks. The graduate student will be a close second, especially in view of the extensive bibliographical notes. All authors will look with envy on the carefully drawn charts and tables. Apart from anything else, this book proves conclusively that a large quantity of research funds will produce a large quantity of research.

Taxation economics can be no better than the theoretical structure on which it is built, whether implicitly or explicitly. In studying this book one gets an uneasy feeling that the economic analysis—the underlying “model” one might say—does not form an interconnected whole but is rather based on bits and pieces that have been accumulated over the past three decades without adequate attention to gaps and inconsistencies.

There is a tendency to make a dramatic generalization that is incorrect as it stands and then mention various qualifications and modifications which, if properly digested and integrated, would undo most of the damage done by the generalization. Can we impose so heavy a burden on the interested citizen or even the professor and student? In the discussion of saving and investment, for instance, we are told that budget surpluses at full employment contribute toward increasing the investment rate (p. 22). Surely it depends on what kind of taxes produced the surplus, what is done with the surplus, and what kind of taxes and expenditures constituted the rest of the budget. The author has previously pointed out the importance of the composition of expenditures and taxes (p. 20) and later mentions the need to “avoid taxes that impair investment incentives” (p. 23). All this is like saying, “Two plus two equals five except that it really equals four.”

In the summary of the same section, the author says, “At any given level of government expenditures, the level of economic activity depends on the *ratio* of taxes to expenditures” (p. 29; emphasis in original). Again, the specific nature of taxes and expenditures is ignored. And again, after the damage is done, we are told, “Saving and investment incentives may also be improved through higher depreciation allowances, investment credits, and other structural tax provisions” (p. 29). It is as if there were two opposing bodies of thought fighting for control and both were declared winners.

In considering the effects of balanced changes in expenditures and taxes, the author first presents a hypothetical analysis in which, by restrictive assumption, expenditure changes are allowed to have greater multiplier effects than tax changes (pp. 10-11). Reference is also made to the important role of monetary policy (pp. 11-12). Then qualifications are forgotten and we are told that expenditure changes have somewhat larger multiplier effects than tax changes (p. 13). The balanced budget fallacy thus persists. The outcome will actually depend on the detailed nature of the taxes and the expenditures and on the state of the capital market, to say nothing of the propensities of those affected. It cannot be said in general that spending will be more expansive than an equal reduction in taxation.

The author asserts that debt created under conditions of excessive unemployment “cannot be a burden on future generations” (p. 26). There are two sides to this question and neither side is represented adequately in the two-

page discussion. It would be better to omit this topic from a book on tax policy than to do less than justice to the significant contributions of recent years. Silence might have been better than brevity in this case.

The 1964 tax cut is reviewed and we are told, "The economic potency of tax cuts has thus been firmly established" (p. 9). A lot more than one successful case is needed before any sweeping generalization can be made as to what is "firmly established" empirically.

The author refers to the fact that "corporations have been able to increase their before-tax profits enough to avoid a reduction in the after-tax return, without increasing their share of income in the corporate sector" (p. 108). He then says, "These observations suggest that corporations have not increased rates of return before tax by marking up prices or by lowering wages, but by making more efficient use of their capital" (p. 108). But the macro data on income shares are based implicitly on price-times-quantity and on wage-times-quantity, not on prices and wages alone. Depending on the relevant elasticities, the macro data are entirely consistent with price and wage changes. Moreover, if price, wage or efficiency is changed as a result of an income tax, profit was evidently not being maximized before the tax was imposed (or raised). There is no reason to believe that price and wage policies were optimal and only efficiency could be improved. The same motives and conditions that prompted changes in efficiency would prompt efforts to change the other variables.

Of the many important questions of tax policy discussed in this book, we may refer to two: greater use of the sales tax and adoption of a shared-revenue plan to help the states. Speaking of proponents of general consumption taxation, the author tells us, "Their concern is to reduce progression, . . ." (p. 160). Two other positions in favor of general sales taxation exist. One of these looks at the tax-expenditure structure as a whole and sees that the net effect of expenditures, though financed by sales taxes, is (or could be) favorable to low- and middle-income groups. Refusal to adopt a sales tax which would make possible the spending (that would not otherwise occur, it is assumed) is actually inimical to the interests of these groups.

The other position that is favorable to sales taxes is that they can actually constitute a progressive element in the tax structure as a whole. The exemption of food and medicine substantially removes regressive aspects; and the tax extracts some contribution to public services from those who manage somehow, legitimately or illegitimately, to maintain a high standard of living and yet pay little or no income tax. A sales tax of this sort can supplement the income tax in ensuring the progressivity and equity of the revenue framework. That the income tax could use some help is suggested by the author's own figures (p. 256) which show \$34.8 billion of nonreported adjusted gross income in 1963, only part of which would be nontaxable if reported, and \$61.2 billion of personal income not includable in adjusted gross income because of conceptual differences.

At the state level, the author emphasizes the need for a credit against state income tax to remove the regressivity of the sales tax (pp. 161, 211). As suggested above, however, a good case can be made for the sales tax even in the

absence of a credit. Moreover, the sales tax need not be at a flat rate; it could be designed on a graduated basis with higher rates applying to big-ticket items.

A basic defect of the empirical analysis of regressivity is that current income is used as the sole measure of ability to pay (pp. 50, 150 ff.). No allowance is made statistically for assets or income beyond the current year although the possible significance of such allowance is mentioned (p. 154). Some persons who are retired or are between jobs may have large assets or income over a longer period and could help pay their fair share of governmental costs even this year. The intellectual resistance to inclusion of assets is well illustrated by a query posed by the author in the context of the negative income tax: "Should assets of the needy families as well as their income be taken into account?" (p. 75). But how can we label them "needy" before we look at their assets? If they happen to have large assets are they "needy"? It is hard to understand why those who pride themselves on being concerned with equity are reluctant to incorporate longer-term income or wealth in their measurement of the ability to pay current taxes.

The book ends with a strong plea for what has become known as the Heller-Pechman plan for unconditional grants to the states based on shared revenues. Pros and cons are given but the major defects of the plan do not stand out.

Any grants tied to a fixed percentage of the federal income tax revenues would be procyclical and would get more money to the states in prosperity than in depression. A trust fund or a variable percentage, if used, would introduce the element of discretionary authority and its political and economic complications. At best, the plan makes no contribution to the much-needed improvement of state tax structures and the elimination of multiplicity of tax forms and tax provisions. One is reminded of the episode in the musical *Call Me Madam* in which the Prime Minister of the mythical Duchy of Lichtenburg refuses to accept a large American loan because it would make unnecessary the basic economic reforms he had been advocating for years.

The author states that included among the opponents of the revenue-sharing plan are "those who . . . have little faith in the willingness or ability of state governments to use the funds wisely, . . ." (p. 229). From this we may infer that proponents of the plan have much faith in the willingness and ability of the state governments to use the funds wisely. They do not have a monopoly on such faith, however, among those who are seeking to solve the fiscal problems of federalism.

An alternative to the revenue-sharing plan is a thoroughgoing tax credit scheme. All state and local taxes would be credited against federal income tax liability up to a certain percentage of state and local taxes (or other limitation). The credit could be given immediately as in the case of unemployment insurance taxes (hence, we may call it an "instant tax credit") thereby avoiding liquidity problems for the taxpayer. And there could be inducements to achieve conformity and interstate uniformity in matters of detail. The states would not only spend the money themselves, they would raise it themselves. This puts the tax credit plan doubly on the side of the angels.

As may be expected, there are very few factual errors in a book that was

read in full by 13 tax experts and in part by 28 more. A few misleading or incomplete statements have crept in. They are mentioned here because of the possibility that the volume will be used as a book of reference on tax provisions and that the errors will find their way into other books and perhaps even articles in learned journals.

The author says, "In 1964, the [dividend] exclusion was raised from \$50 to \$100 (\$200 on joint returns)" (p. 138; similarly, 134, 238). The filing of a joint return is not sufficient to ensure a \$200 exclusion though dividends be ample. Husband and wife must each receive \$100 in dividends in their individual names or in their joint names to ensure the maximum exclusion.

We are told that "fewer than ten shareholders" are required for partnership treatment (p. 138). The requirement is "no more than ten." The tenth man is in.

Reference is made to the fact that, "Gains on the sale of owner-occupied houses are exempt if they are applied to the purchase of a new home within twelve months" (pp. 90-91). But if the new home is newly built there are eighteen months allowed after the old house is sold.

Under the 1948 law, we are told, "Half of all inter-spousal transfers was eliminated from the bases of the estate and gift taxes" (pp. 184-185). This is misleading as to estate taxes. The author himself mentions at several points that the marital deduction in the estate tax is *all* that is given to the spouse, up to a limit equal to half the estate.

"Income in kind" is listed as one of the categories of personal income not included in adjusted gross income (p. 256; also, 260). In fact, some forms of income in kind are included in adjusted gross income. One of these is pay received in property instead of cash. The employee must report the fair market value of the property as wages (except in the case of Christmas turkeys).

The excess profits tax of World War II is stated as having been as high as 95 per cent (p. 238). This is true but there was also a provision for a rebate of 10 per cent of this.

Finally, in quoting the Sixteenth Amendment, the author (or typist) incorrectly uses the singular of "taxes" and the plural of "source" (p. 236).

There is no use making a federal case out of all this even though many a federal case has been made out of much less. The errors are minor. The 41 tax experts whose help is acknowledged should not sue; nor should they be sued.

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Tax Reform and the Alliance for Progress. By RAYNARD M. SOMMERFELD. Austin: University of Texas Press, 1966. Pp. xiv, 217. \$5.00.

Since World War II the newest and possibly the fastest growing field of public finance is the formulation of appropriate tax policies for the underdeveloped countries of the world. Despite the growing volume of professional literature dealing with particular issues and countries, and despite also the increasing number of conspicuously good scholars undertaking research in this field, no one has had the temerity to write a comprehensive study comprising

both theoretical and applied aspects of tax policy in underdeveloped countries. Even though Sommerfeld has limited the scope of his research to Latin America, the task still remains much too ambitious. He would have been well advised to have tried something less demanding.

The monograph has a pragmatic purpose. In the absence of detailed fiscal studies of each Latin American country, the author has attempted to suggest guidelines for tax-reform efforts which would serve the goals of the Alliance for Progress. Appropriately, he begins, after an introductory chapter, with an examination of the three basic documents establishing the framework for the Alliance in order to obtain directives in the form of tax policy goals. Unfortunately, however, these documents were written by bureaucrats and politicians rather than by social scientists, with the result that some tax policy goals are either not mentioned (e.g. efficiency in the utilization of resources) or they are couched in meaningless terms unless translated into their precise tax policy equivalents (e.g., "demanding more from those who have most" is actually a mandate for an optimum distribution of income). When Sommerfeld fails to identify and discuss the policy goals which are implied but inadvertently omitted from the Alliance documents, and when he discusses the other goals superficially (stability is considered to be only a problem of price stability), the result is that his study has no sound theoretical frame of reference. Without having a clear rationale for tax reform, it is quite impossible to make recommendations for improvement that are consistent, logical, and purposeful.

After this haphazard discussion of tax policy goals, the author presents a chapter dealing with some general characteristics of Latin American tax systems—the level of taxation, relative importance of direct and indirect taxes, tax sharing, and the widespread tendency in Latin America to earmark tax revenues. About one-half of the remainder of the book is concerned with particular taxes (individual chapters on income taxation, wealth taxation, and other taxes), while the other half includes chapters on tax-exemption schemes, tax administration, and a summary of findings and recommendations.

While the failure to develop a sound framework for tax policy design is the principal shortcoming in this study, there are several other faults that detract from the monograph's serving its purpose as a guide for tax-reform efforts in Latin America. Although customs duties are the most important single source of tax revenue in most Latin American countries, and appropriate tariff policy one of the most crucial issues of tax reform, this subject is summarily discussed (in about two pages) with the observation that "Very little useful generalization can be made about tax-reform efforts in regard to customs duties since these taxes are frequently imposed for trade control, and only secondarily for revenue purposes" (p. 127). Although one of the principal sources of income tax revenue is corporations, and this revenue is of strategic importance for the allocative function of providing resources for public investment purposes, an elimination of the double taxation of corporate income is recommended (p. 80). Although monopoly influences are pervasive in Latin America, and this problem is often exacerbated by the prevalence of windfall gains due to inflationary pressures and frequent monetary devaluations, the abolition of excess profit taxation is recommended (pp. 131-32). One would hope

that Latin American countries would not take too seriously the advice also given that they should consider a poll tax (p. 133). In the light of these and other deficiencies, it would be superfluous to elaborate on the errors of fact, the use of secondary sources, and the spotty bibliography.

With some foreboding, Sommerfeld observed at the outset of his study that "It is only hoped that this work will not be obsolete before the last pages are written." Unfortunately, this too has happened to some considerable degree. In the early days of the Alliance, it was realized by the Joint Tax Program of the Organization of American States, the Inter-American Development Bank, and the Economic Mission for Latin America that the countries of Latin America would require broad fiscal policy guidelines to accomplish the economic goals set forth in the Charter of Punta del Este. For this purpose a conference was convened in 1962, at which the luminaries of the public finance world addressed themselves to all the major issues to be faced in restructuring the revenue systems of Latin America. Although these papers were not published until 1965,¹ mimeographed copies of the conference proceedings were distributed in Latin America as early as 1963, three years before Sommerfeld's book was published.

Lest it appear that this reviewer is being too harsh in his criticisms, it would be well to conclude with a few observations on the prerequisites for a successful completion of the assignment Sommerfeld set for himself. Since probably the paramount goal of tax policy in Latin America is economic development, one must have somewhat more than an incidental knowledge of the processes of economic growth and how development can be induced in underdeveloped countries. Since tax policy is interrelated with fiscal policy, with monetary policy, and in fact to some degree with every facet of economic policy, one must be as much an economic generalist as a specialist in public finance. All of this technical equipment would not be sufficient, however, unless it were combined with a wealth of institutional knowledge of Latin American social and economic conditions, and there is really no way to obtain this except by field research over an extended period of time. One must also be prepared to work with skimpy and unreliable statistics, with documents that are in a foreign language, and with material that is seldom available to the public. Little wonder under all of these circumstances that Sommerfeld fell short of his target.

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¹The Joint Tax Program of the OAS, IDB, and ECLA, *Fiscal Policy for Economic Growth in Latin America*. Baltimore 1965.

Income Distribution and the Federal Income Tax. By BENJAMIN A. OKNER. Michigan Govt. Studies No. 47. Ann Arbor: Institute of Public Administration, The University of Michigan, 1966. Pp. xiii, 148. \$3.00.

Professor Okner has added to our general knowledge of the redistribution accomplished by the federal individual income tax. By means of computer simulation performed with a subsample of 1962 income tax returns, he has

broken new ground in quantifying the redistributive effects of the numerous provisions, including the tax rates schedule, of the tax as it exists. He has been able to develop "before and after" Lorenz curves for the present tax law, the pre-1964 tax law, and each of the outstanding features of the law such as the exclusion of certain forms of income from the tax base, the deduction of certain expenses, and the exemption of a certain amount of income per person. He concludes by computing the shift of income that would follow from adoption of what he considers a more nearly ideal definition of a tax base. (This is referred to below as the Okner reform.) The principal findings are summarized in the accompanying table, which is set up to show possible deviations from the norm of the 1964 Revenue Act.

| Change in Tax Law | Percentage Increase (+) or Decrease in After 1965 Tax Area of Inequality | Share of After Tax Income of Tax Filing Units with Total Income | |
|---------------------------------------|---|---|-----------------------|
| | | Under \$3000 | \$500,000 and Over |
| Change to regressive tax rates | a | a | a |
| Abolish income tax | +17.70 | 7.694 | b |
| Change to flat rate of 19 per cent | + 1.83 | 8.072 | .707 |
| Abolish personal exemptions | + 1.12 | 8.031 | .609 |
| 1964 Revenue Act | 0.00 | 8.158 | .579 |
| Return to 1963 tax law | - 0.05 | 8.204 | .581 |
| Introduce negative tax rates | - 0.97 | 8.501 | .577 |
| Increase level of personal exemptions | a | a | a |
| Disallow all nonbusiness deductions | - 1.11 | 8.265 | .525 |
| Abolish all exclusions | - 1.47 | 8.292 | .336 |
| Eliminate income splitting | - 1.67 | 8.294 | .585 |
| Adopt Okner reform | - 3.27 | 8.454 | .283 |
| Use total income as tax base | - 4.26 | 8.411 | .286 |
| Change to more progressive tax rates | a | a | a |

Sources: Column 1, text tables and derivations from data cited in text of book under review. Columns 2 and 3. Appendix Tables E-1 through E-5, *ibid.*

(a) Not discussed in book under review.

(b) Not available.

It shows, for example, that substituting a flat rate of 19 per cent for the present set of progressive rates would increase income inequality by 1.83 per cent; it would reduce low-income taxpayers' share of income only slightly, but would increase high-income taxpayers' share of income by 22 per cent. On the other hand, the table shows that using total income as the tax base would reduce after-tax inequality by 4.26 per cent and cut the top group's share of income by almost one-half. The table shows the direction of the effect and relative significance, as far as redistribution is concerned, of several possible changes in the law as set forth by Okner. I have also listed in the table three types of changes that he did not consider.

The tax structure, as now designed, has some benefits for everybody. As Okner points out (p. 76), it has personal exemptions for the low-income groups, nonbusiness deductions and income-splitting for the upper-middle income people, and special capital gains provisions for the rich. After reviewing the nature of and the rationale for these provisions in detail, Okner concludes that considerations of equity dictate a wholesale reform of the tax base. He would include full capital gains, imputed rent, social security benefits, and presently tax-exempt interest in the base, scrap the optional standard deduction, and disallow deductions for interest paid and taxes other than state income and sales taxes. He would replace the present scheme of personal exemptions with a \$150 per person tax credit. By means of such a reform he would reduce income inequality by 3.27 per cent. (See table.)

While the import of the simulation effort was to produce the detailed quantifications summarily reviewed above, the main arguments for and against the Okner reform are not those concerned with vertical redistribution, but rather those having to do with horizontal equity, which is not so easily quantified. It is often stated, but seldom observed, that vertical equity measures presume that horizontal equity is already accomplished. Following this logic, Okner defines his inclusive tax base to achieve what he regards as horizontal equity and then is pleased to discover that it would produce as a by-product more redistribution toward equality than does the present tax base. He does note that the reform would enable considerably lower tax rates to raise the same revenue, but he makes no claim that lower nominal rates are in themselves important.

So far as horizontal equity is concerned, Okner contributes little that is original, although he does provide succinct reviews of the literature on each tax provision. As we have noted, he does add to the quantitative dimension of knowledge concerning vertical equity. However, it seems to me that to complete his study, he should have asked several additional questions. First among them is: what re-ranking of taxpayers might follow from various reforms? Is it not likely that methods of reporting and realizing income will change with the tax law? Okner makes no mention of responses that high-income families might make to a decision to eliminate income-splitting or to remove the special treatment now accorded capital gains. In other words, he ignores the fact that the pre-tax distribution itself is to a certain extent an artifact of the existing tax system.

A second question that would have been worth asking, and one that certainly lends itself to simulation, is the following. How much does the vertical redistribution resulting from a given tax law depend upon the per capita level and inequality of pre-tax income? A third but more difficult one to deal with is: what difference in redistribution would follow from adopting a tax accounting period of more or less than one year?

A fourth question that needs to be taken up involves varying the definition of the income-receiving unit and asking what the relationship is between the tax unit and the family unit under different possible tax provisions. There is only limited meaning to a ranking, without regard to the size of the unit, of families and sub-families represented by tax returns. There would be more

value in knowing the distribution and redistribution within a standardized family size group. Along this same line, it needs to be noted that because the author took no account of the 15 per cent of persons not represented on income tax returns, he was unable to give more than a page of vague speculations to one of the more interesting proposals to expand the redistributive effect of the income tax, namely, the reverse or negative income tax.

While Okner has not dealt with all the questions implied by the title of his book, he has taken advantage of early access to a primary data source and of competence with computer technology to provide us with valuable insight into some significant relationships between the income distribution and our leading tax. For that, he has put all public finance students in his debt.

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International Economics

Le développement d'un marché européen des capitaux. Rapport d'un Groupe d'Experts constitué par la Commission de la CEE (European Economic Community, *The Development of a European Capital Market, Report of a Group of Experts appointed by the EEC Commission*). Pp. xlii, 408. Brussels: European Economic Community, 1966.

In 1950 or 1951 after the proposal of the Schuman Plan I suggested in a lecture that functional integration, by commodities or through customs union, was likely to be a half-way house in which the Six could not dwell long. Either they forged ahead and achieved wider economic union, possibly going so far as to merge their economic sovereignties, or they would probably slip back into national policies carried out independently. In particular it seemed to me that employment, wage, price, tax, and foreign-exchange policies could not be carried out separately while a significant number of or all commodity and factor markets were joined.

This may or may not have been a correct forecast, depending upon whether 17 years is a long time. Integration has broken down in coal policy, but it has survived the devaluation of the French franc in 1957 and 1958 and the revaluation of the guilder and Deutschmark in 1961. And harmonization has been proceeding. It has proceeded despite the report of the Tinbergen Committee, that it was not required in commodity taxation [3], and with the assistance of the reports of other distinguished economists, such as the group led by Fritz Neumark in general taxation [2]. The latest of these is the important and impressive report on the integration of the capital market of the European Economic Community, prepared by a Group of Experts under the chairmanship of Professor Claudio Segré of the Commission staff.

The Group of Experts maintains that harmonization takes place in response to the pressure of problems (p. 80) and that it is inevitable (p. 243), needed not only to merge the investment resources of the separate countries in the Community but to increase their volume (p. 209). Many of the faults in the functioning of domestic capital markets, and institutional weaknesses, can be

overcome by their progressive integration (p. 256) since the faults of functioning of national security markets are the result principally of their narrowness and compartmentalization (p. 280). Integration is especially needed to provide more "transformation" (financial intermediation) which is not an end in itself but is sought as a remedy to excessive public preference for liquidity (p. 56). The fundamental disequilibrium on European markets is the chronic excess long-term demand for credit over the supply, which leads to state intervention to keep the supply reserved for home borrowers (p. 99). Such financial intermediation as does take place diverts liquid savings to government investment, mortgages, and sometimes government enterprise (p. 55), the last of which has especially large demands for credit because of its practice of holding prices low (p. 132). Wider markets will permit the development of specialized institutions for underwriting securities and maintaining secondary markets (pp. 212, 243). In particular, banks perform too many functions such as investing for their own account, distributing securities and providing security advice to customers. In this they run the risk of conflicts of interest (p. 242).

The Group of Experts is not the first to make these points. Earlier in the field have been U. S. Secretary of the Treasury Douglas Dillon, in a speech at Rome in May 1962, and the Joint Economic Committee of the Congress, for which the U.S. Treasury prepared a background report, omitting, however, any reference to the important Amsterdam market [4]. Separate national studies have been undertaken in France and Belgium. But the Segré report is by all odds the most complete, the broadest, and the most detailed, the report with the most far-reaching recommendations on a wide range of issues.¹ Its 30 tables bring together the most up-to-date and detailed statistics comparing various aspects of the capital markets of the Six, and frequently those of the United Kingdom and the United States, as well. It is, as I have said, impressive and important.

Capital-market institutions and their regulation evolved differently in different countries owing to historical differences in experience and the accident of diverging paths to protect the saver and to maintain the viability of institutions during the depression, war, and postwar recovery. There are some general trends, such as excessive public liquidity preference, the necessity for governmental financial intermediation, and the tendency of government to reserve savings for its own use. But the differences are striking. Take insurance companies, for example. Insurance companies and pension funds provide 3 per cent of net financial saving in France, 9 in Italy, 10 in Germany, 14 in Belgium (20 in the United States), 39 in the Netherlands (and 40 per cent in Britain) (Table 12). With its high liquidity preference, "which cannot be changed," France buys term insurance rather than straight life (p. 47). Life insurance companies separately furnish average annual increments in reserves per capita for the capital market of \$2.50 in Italy, \$4.70 in France, \$10.70 in Belgium, \$12.30 in Germany, and \$21.60 in the Netherlands. The respective percentages of average annual increases in reserves to gross domestic capital formation are 1.2, 1.4, 3.7, 2.9, and 4.4 (Table 27).

¹I have not seen a parallel report issued by the Organization for Economic Cooperation and Development.

With these differences in flows, there are wide differences in the latitude allowed to life insurance companies in investing their reserves in equities (p. 236) and in foreign securities (p. 335). In Luxemburg both equities and foreign securities are proscribed to life insurance companies. In the Netherlands, domestic life insurance companies are subject to no restrictions with respect to either, although foreign life insurance companies operating in the Netherlands are not allowed to invest abroad. A complex variety of rules, limits, and requirements has been laid down separately in Belgium, France, Germany, and Italy. The well-known concern of the Dutch monetary authorities that Dutch savings should serve Dutch needs on a priority basis, with some limit to foreign access to the Dutch capital market, is not ignored in the report (pp. 99, 131). The statistics on life insurance, plus the infrastructure needs of localities, furnish its explanation.

The Group of Experts believes that the Community's capital market can be integrated without the emergence of a single dominant financial center (p. 226). They adduce a long list of specific proposals for harmonization, although in some fields such as bank regulation, more study is called for before specific rules can be laid down (p. 243). They also recommend reform, such as the improvement of company accounts (annex to Chapter 11), and refer in frequent places to a new directive under preparation in the Commission to relax remaining exchange controls.

Among the most interesting recommendations is support for the proposal of Professor Pfleiderer, president of the provincial central bank of Baden-Wuerttemberg, that the spread around parities in exchange rates be eliminated to reduce exchange risks (p. 346). The suggestion is straightforward, in contrast with the irony with which Professor Harry G. Johnson put it forward several years ago [1], and makes clear that proposals for bands and flexible rates are essentially directed to international disintegration. The Experts recognize that a flat rate of exchange is a step on the way to monetary union, when all exchange risks within the Common Market, they suggest, would be eliminated (p. 340). This requires harmonization of monetary policy, which they favor (p. 326). The deeper question is not addressed, however, whether harmonization is enough so long as economic sovereignty is retained by the constituent members. Is harmonization a three-quarter-way house which cannot be occupied indefinitely? Suppose the member countries differ among themselves as to the weight to attach to such targets as full employment, price stability, or balance-of-payments surpluses (possibly for accumulating claims in the United States which can be converted into gold)? Or suppose a major structural change reduces the demand for the output of a country. If factor movements failed to produce a new equilibrium would it be condemned, like Appalachia, to seek adjustment through deflation alone, without the assistance which devaluation can provide? The Experts, of course, cannot be called upon to look beyond harmonization, which is their assignment, but a reviewer may be permitted to raise the question.

There is one other delicate issue on which the Report goes only a certain distance. Directed to the question of integration of the Six, it does not ask whether a wider integration with New York, London, or the Euro-currency

and Euro-issue markets would be more efficient. There is no discussion of the analogue in capital markets of trade-creation and trade-diversion, the extent, that is, to which the development of a European capital market limited to the Six is a second-best solution, inferior to the integration of a world capital market. There is a section on the Euro-issue market (pp. 216 ff.); it is mentioned that European countries have difficulty in maintaining independent monetary policies so long as they are connected to the Euro-issue market, but that they cannot restrict access to this market for fear of handicapping the competitive position of national companies (p. 222). The role of the American network of banks in the Community is noted (p. 163) and the spread of American firms they serve (p. 164). Reasons are given why European banks do not have similar networks, although growth and American competition may stimulate their construction. Harmonization of taxes within the Community must keep an eye cocked at tax levels in third countries or EEC capital will leave the Six for abroad (p. 202). The European investor has a clearer picture of the position of American corporations than of European because of the fuller company reports the former publish (p. 264). 195 American securities are quoted on the Amsterdam exchange as opposed to 30 issued by Holland's EEC partners; even in Brussels and Paris American securities are almost as numerous as EEC, 23 to 44 for Brussels, and 32 to 40 for Paris (Table 27).

Economic integration among the Six has already gone a considerable distance, with the help of outside factors. The Report recognizes the role of the Euro-dollar market in tending to equalize interest rates (p. 84). Parallels can be found in Mediterranean immigrants in the labor market, and American corporations in the markets for certain commodities. To the extent that the EEC frees up regulations and permits wider capital movement, will distinctions be drawn between the Six and the outside world? Is the recommendation for permitting the *caisses centrales* which invest savings banks funds on a national basis to buy foreign securities without limit, and local savings banks and *caisses* to buy them in limited amounts (p. 333) restricted to Community securities? It does not say so.

The Report of the Group of Experts is a landmark in the discussion of a vital question of current international economics. Its importance extends far beyond the Community, and it is to be hoped that in an intellectual trade-creating gesture, the Community will make it available in translation in that medium of exchange, widely used in the world, English.

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Latin American Economic Integration—Experiences and Prospects. Edited by MIGUEL S. WIONCZEK. New York: Frederick A. Praeger, 1966. Pp. xiv, 310. \$7.50.

A Latin American Common Market? By SIDNEY DELL. New York and London: Oxford University Press, for Royal Institute of International Affairs, 1966. Pp. ix, 336. \$8.80.

These two books, particularly the Dell volume present useful historical and analytical background concerning economic integration in both South and Central America, though the many changes and developments that have occurred since they were written have somewhat reduced their timeliness. The Wionczek volume never really had a chance: most of its nineteen essays were originally written in 1964 (a brief essay by Tinbergen was written in 1959!), and the volume originally appeared in Mexico in a Spanish language edition. The Dell volume was written in April 1965. Recent analytical writing, notably by Harry G. Johnson, carries the debate over trade discrimination further than in either of these books.

The Wionczek volume is a collection of nineteen essays; the authors include both economists and officials. In general, it is an undistinguished volume, though several of the essays, particularly Wionczek's own contributions, are of value. Nevertheless, it is a book that warrants attention from economists interested in Latin America or in the inter-relation of trade and development. The central theme of the volume is set forth by Wionczek in his introductory essay: "The integration doctrine elevates to the multinational plane the thesis that economic development is impossible without industrialization. According to this thesis, the sustained growth of an underdeveloped economy depends on the degree to which an active process of substitution of imports by domestic production can be promoted, so as to extend the country's capacity to import to cover the acquisition of an optimum volume of capital goods and technology." This theme is familiar and has been better presented and defended elsewhere. Yet the essays in this volume convey the flavor of economic policy debates from the point of view of true believers and demonstrate how successful Raul Prebisch, Gunnar Myrdal *et al.* have been in getting their ideas bureaucratized. Economists can continue to debate the ideas themselves but, for better or worse, economic policy in Latin America and elsewhere in the underdeveloped world increasingly reflects them. For true believing free traders no good can come of this. For others, the problem is more complicated. The essays in this volume confront the reader with the variety and complexity of the issues involved in Latin American economic integration. By their inadequacy they lead the concerned reader to realize how much yet remains to be done,

theoretically as well as practically before a coherent doctrine of economic relations among less developed countries (LDC's) can be developed. Economic theory today still gives little guidance to the policy maker interested in knowing which of the many forms of economic integration is most desirable, what sorts of compensation arrangements are necessary and desirable, how much reliance should be placed on market forces and how much planning must be done, which countries should band together economically, and a host of related practical issues.

The Dell volume is somewhat more current and a good deal more ambitious intellectually. Superficially, it looks like a vigorous defense of economic integration as a means for stimulating economic growth. For example, the Preface states that "The rate at which these countries [all LDC's] are able to move towards tolerable living standards may depend to a great extent on their ability to form larger and more viable economic units." But the sense of mission is considerably diluted, and the author confronts us with a perplexed and troubling problem to which no pat answer is given. Dell stresses that regional integration is no substitute for "political, economic and social reform," and that to make integration work, joint multinational planning is necessary. "But if regional planning is unrealistic for Latin America, so also is a common market in that area unrealistic" (p. 212). Nationalism is a powerful but not a benign force: Dell finds it leading directly to protectionism and *étatisme*—both forces with which regional integration must find a *modus vivendi* if it is to be effective politically and economically.

Dell has written a thoughtful and useful book. But it stops short of a direct confrontation of the potential conflict between the economic and political requirements of economic integration and development. Economic nationalism may look to economists like a shabby trick that is being played on the peoples of the developing world, but is this really the case? Political stability is not easily come by, yet it is an important, perhaps essential, requirement for sustained economic progress. If the price of such stability is a higher degree of protection or government control than is economically optimal, it may still be worth paying. In any event, it is this potential conflict that lies at the heart of the problem of Latin American economic integration and that has still had inadequate scholarly attention. More narrowly, once economists make their peace with economic nationalism, there is much that they can do in helping to analyze its cost. Indeed, economic integration in Latin America may prove to be less significant as an engine of growth than as a means of reducing the costs of protection.

It should be noted that neither volume offers a very useful compilation of statistical or bibliographical material. The Wionczek volume has a skimpy bibliography (English language titles only), no index, and very little statistical information. The Dell volume has an index, a somewhat better bibliography, a few tables and two very useful documentary appendices containing Article XXIV of the GATT, the Montevideo Treaty, the General Treaty on Central American Economic Integration, and other material.

CHARLES A. COOPER

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Debt Servicing Capacity and Postwar Growth in International Indebtedness.

By DRAGOSLAV AVRAMOVIC, ASSISTED BY RAVI GULHATI. Baltimore: Johns Hopkins Press, 1958. Pp. xvi, 228. \$5.00.

Debt Servicing Problems of Low-Income Countries, 1956-1958. By DRAGOSLAV

AVRAMOVIC AND RAVI GULHATI. Baltimore: Johns Hopkins Press, 1960. Pp. xvii, 74. \$2.50.

Economic Growth and External Debt. By DRAGOSLAV AVRAMOVIC AND ASSO-

CIATES. Baltimore: Johns Hopkins Press, 1966 [c.1964]. Pp. xiii, 207.

\$6.00.

These three books cover the work over the past several years of Mr. Avramovic and his colleagues at the World Bank on the debt servicing problem of less developed countries. The first two are largely factual presentations showing the increase in debt servicing payments since World War II together with the growth in such relevant variables as income, savings, and export earnings. For the period 1946-1955, Avramovic concludes that debt servicing was not a serious burden on the less developed nations. These charges increased somewhat more rapidly than export earnings and income, but public debt service amounted to only 3.7 per cent of the export value of goods and services in 1955. Thus, "in general it appears that gains in production and trade . . . during the first postwar decade not only enabled the borrowing countries to maintain service on their obligations, but also provided a foundation for further productive investment."

In the second and third books listed above, which cover the period after 1955, Avramovic begins to emphasize the rapid rise of debt service charges in relation to the export earnings of the developing nations. As a recent OECD study has pointed out, the ratio of public debt to the export earnings of these countries rose steadily from 3.8 per cent in 1956 to 10.8 per cent in 1965. One result of this has been mounting pressure by the less developed countries to be granted easier terms on their borrowed capital. The last book in the group is in part a response to such pressure. It was prepared in response to requests by OECD and UNCTAD for a paper discussing the factors determining a country's debt servicing capacity. Unlike the other studies it is largely analytical.

For the typical primary producing country with limited export possibilities and yet a reasonably high growth target, Avramovic sets up seven specific conditions which have to be met for the mechanism of international lending on "hard" terms to work successfully. These are: (1) Creditors must agree to lend continuously despite high service ratios and despite the rigidities in the balance of payments of their debtors; (2) Debtors must manage their external accounts in a way which enables them to pay their bills when due; (3) Fluctuations in export earnings of the debtors must be greatly moderated; (4) The "cash squeeze" problem of countries which have to repay an extremely large proportion of their debt over the next few years must be solved; (5) The return on capital must be higher than the international rate of interest; (6) The plough-back of profits must be high enough so that at some point domestically generated savings exceed domestic investment requirements and thus leave a surplus which can be used to meet service payments; and (7) In-

vestment must be sufficient to enable total output to grow at a satisfactory rate. In a final essay Avramovic states that all of these conditions could be reduced to one, namely: a high level of investment that yields a high rate of return.

There is no doubt about the difficulty of specifying in any simple way the determinants of such an elusive concept as a country's debt servicing capacity, and Avramovic and his colleagues approach the problem from several different viewpoints. For example, J. P. Hayes in a very interesting essay explores the matter by using a growth model of the Harrod-Domar type. Assuming some constant growth rate, a given initial and marginal domestic saving rate, and a fixed capital-output ratio, he analyzes the growth of external debt in relation to the interest charges on this debt. A "critical" interest rate is defined as that rate at which the external debt will increase at the same rate as gross domestic product. Interest rates higher than this are considered to result in a problem of unmanageable indebtedness in the long-run. The author concludes from this analysis that there are in fact a number of less developed countries with fairly low saving rates and high capital output rates that are unlikely to be able to sustain more than low per capita growth rates unless interest levels on their indebtedness averages substantially less than 5-6 per cent.

One problem in using simple growth models with fixed capital-output and marginal-saving ratios for analyzing a subject such as this is that they tend to divert attention from variables over which governments may have considerable control and which may be very important in determining a country's transfer ability. Although Hayes specifically points out the limitations of these models, there is a tendency throughout the book to assume that the less developed countries themselves have very little room for policy manipulation. There is, for example, no discussion of what types of exchange rate policies, fiscal policies, or monetary policies may help or hinder a country's effort to meet its transfer burden. There is emphasis upon the importance of using foreign funds in a productive manner, but even a rapid rate of growth is no guarantee that a government can meet its debt obligations. Fiscal, monetary, and exchange rate measures must be followed that produce an export surplus from the situation of rapid growth, and this is by no means a simple and easy goal to achieve. Probably Avramovic and his associates felt that these policy considerations were too well known to be worth very much discussion. However, their generally very balanced and sensible analysis of the problem might usefully have been broadened by more attention to appropriate policy measures that could be taken by the less developed countries themselves.

ROBERT E. BALDWIN

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International Financial Aid. By WOLFGANG G. FRIEDMANN, GEORGE KALMANOFF, AND ROBERT F. MEAGHER. New York and London: Columbia University Press, 1966. Pp. xiv, 498. \$13.75.

The major part of this book consists of four long chapters. The first reviews the methods and policies of the principal aid-giving countries, the second surveys the experiences of a number of aid-receiving countries, the third examines

several aid-supported projects and programs, and the last is concerned with general policy issues. These four big chapters are sandwiched between two shorter ones, on the magnitude of aid and the future of aid.

Much of the most useful material is on the individual projects and programs. The discussion of loans to Colombia and Chile (written by Raymond Mikesell) is especially instructive. These pages document in revealing fashion the great difficulties involved in negotiating, implementing, and administering foreign loans. Even where all participants are in general agreement and eager to get on with the job, delays seem almost inevitable, and actual costs invariably exceed estimates by a substantial margin. No new theories or generalizations emerge from this material, but what does come through clearly is that the mere mechanics of public lending are as troublesome as are the more frequently discussed theoretical aspects.

The other chapters are considerably less helpful. The first long chapter on the institutions and policies of donor countries is largely descriptive of rather pedestrian matters. There is a great deal of information on names of aid agencies, size of loans and grants in recent years, paid in capital, and so on, but the reader is unlikely to gain much understanding or insight into the logic or rationale of aid giving. Where some explanation is attempted, the result often sounds like a government handout. The German emphasis on loans, for example is said to be "due to the conviction that charity is neither desirable nor in the interest of developing countries, since it would tend to inflate demand and shield the recipients from the hard realities of economic life" (p. 75).

The chapter on aid-receiving countries covers nine individual countries and one region (East Africa) in 100 pages. Since there is no central hypothesis or model to guide us, the result is necessarily a rather cursory view of the problems these countries have in obtaining and using foreign aid in their development efforts. The chapter on policy matters includes comments on a wide range of issues: repayment arrangements, tied aid, project vs. general development aid, bilateral vs. multilateral, etc. There are isolated points in this chapter of interest, but the chapter does not build from the preceding descriptive materials nor do the authors bring out in a very clear fashion the essential nature of the problems. For example, the tying of aid by the United States (and other countries) is attributed to balance of payments problems, but no mention is made of how or why aid aggravates these problems. The reader unacquainted with world liquidity problems, reserve currencies, and all that, will not be satisfied (or enlightened) with the assertion that aid is tied to help protect the balance of payments. Or, the reader who might have learned that the productivity of capital in capital poor, labor rich economies is high may wonder why repayment of a foreign loan is such a problem. Such a reader will learn from this book that it is a problem, but he will not learn why.

Instructors of development courses may find the chapter on projects and programs (Ch. 5) useful as collateral reading for their students. They (and others) however must look elsewhere for a searching review of the economics of foreign aid.

HENRY J. BRUTON

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Business Finance; Investment and Security Markets; Insurance

Optimal Financing Decisions. By ALEXANDER A. ROBICHEK AND STEWART C. MYERS. Englewood Cliffs: Prentice Hall, 1965. Pp. x, 166. \$2.50.

The recent growth of specialization and knowledge in the field of financial management has given rise to numerous "satellite" books. These books are designed to examine a particular subject in depth and are used in conjunction with a more general text. *Optimal Financing Decisions* is such a satellite book. Its stated purpose is to "present a systematic application of financing theory to the problem of the firm's financing decisions."

The authors take a normative approach to problem solving. They attempt to "erect a theory of how rational investors *should* value stocks." When armed with this theoretical framework, the financial manager will be aided in the decisions that confront him.

For analytical purposes, the book is divided into two parts. The first two-thirds of the book develops a theory of stock value by examining each of the major factors affecting this value, namely, income, leverage, dividends, growth, and uncertainty. The final third of the book then brings together all these factors to form a basic valuation formula and to study problems of short- and long-term financing.

Thus, it is necessary to understand the methods by which the basic valuation formula was derived. First, three methods of measuring income are examined; the accounting method, the internal rate of return method, and the discounted present value method. Of these three, the discounted present value method was selected as being most appropriate.

Next, the general problem of leverage yields interesting, though at times confusing, presentations of the Modigliani-Miller hypothesis and the "traditional" point of view. Modigliani-Miller argue that given certain constraints, the value of a firm is independent of the relationship of debt to equity. According to the traditional viewpoint "stockholders' value per share can be increased by judicious use of debt." If, however, the leverage becomes too high, the value per share will decline. After twenty-nine pages of exhaustive argument, it is ironic to find that both positions lead to a similar conclusion, that there is some degree of leverage which will maximize the value of the firm. Unfortunately, there is no agreement as to exactly what combination of debt and equity will satisfy that condition.

The discussion of debt and equity is followed by an enlightening discourse on dividend policies and the reinvestment of earnings. Building on the earlier concepts, the authors conclude that the value of a share of stock is the discounted present value of its expected dividends. This, then, leads into the issue of growth, which centers largely around the works of M. J. Gordon. Gordon assumed that the dividend payout ratio and the rates of return of future investment were constant. He also found these factors to be highly significant in explaining variations in stock prices. It is argued, therefore, that investors put great weight on past historical trends, such as the dividend record, in ascertaining the risk and value of a company's stock.

The final factor affecting stock value is uncertainty. In this connection, a detailed study is presented of risk aversion, utility, and the use of "certainty equivalents" which reflect the attitudes of individuals toward risk.

In the final third of the book, all of the previously mentioned factors are brought together in a basic valuation formula. Essentially this formula consists of the discounted present value of expected dividends and is not an equation to be solved, but one that is intended to be used by financial managers as a guide in formulating policy.

Finally, the book turns from the abstract world of model building to the more mundane problems of the real world. Problems of short-term and long-term financing are examined in light of the basic valuation formula. Furthermore, the authors effectively demonstrate the use of simulation techniques, linear programming, and the Monte Carlo method as systematic approaches to problem solving.

In conclusion, the book represents a valuable addition to financial literature. It brings together in a cogent and useful manner numerous hitherto unrelated theories. Furthermore, it offers the reader an opportunity to see the application of quantitative tools in making financial decision. Finally, its in-depth analysis of conflicting theories and techniques will act as an invaluable guide to the serious student of finance.

BENTON E. GUP

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Industrial Organization; Government and Business; Industry Studies

Performing Arts—The Economic Dilemma. By WILLIAM J. BAUMOL AND WILLIAM G. BOWEN. New York: Twentieth Century Fund, 1966. Pp. xvi, 582. \$7.50.

Because of this book's subject matter, it will interest many readers who take no notice of most volumes reviewed in this journal. Baumol and Bowen are excellent ambassadors for the economics profession. Their study may convince skeptical humanists that economic research can illuminate the conditions of the arts and also that some leading economists can write readable prose. The book is an important contribution to public discussion of the quality of consumption, as distinguished from its quantity. Readers will be rewarded not only with insights and information about large issues but also with factual tidbits with which to enliven their dinner table conversations.

The study presents an impressive amount of information about live professional performances of orchestral music, chamber music, opera, ballet, modern dance, and the theater. It includes data on costs and revenues of performing groups and the characteristics of their audiences, mainly in the United States but with ample comparative data on Great Britain. Much of the material is from primary sources, and its collection was complicated by the large number of organizations and the informality and incompleteness of records. The authors refer to "months of searching through dusty files in cramped backstage

offices, hundreds of hours spent collecting questionnaires from audiences in every part of the country and every type of performance, many interviews with producers, managers and choreographers, and a massive correspondence." The statistics are skillfully used in a text that should not be too abstruse for general readers, to whom the book is primarily addressed. Extensive appendices (about 125 pages) give additional details and information on methods.

In deflating over-enthusiastic reports about the "cultural boom," Baumol and Bowen note that most of these fail to allow for population growth, the declining value of money, and rising income and consumption levels. From 1929 to 1963 real per capita expenditures on arts admissions hardly changed; in relation to disposable income they declined about one fourth, from 0.15 per cent of disposable income to 0.11 per cent.

The audience for the performing arts is found to be drawn from "an extremely narrow segment of the American population." Its members are well educated, have high incomes, are predominantly in the professions, and are mainly in their youth or early middle age. Sample surveys indicate that in 1963-64 more than half of men and almost one-third of women in the audience had done graduate study (compared with 5 per cent of men and 2 per cent of women in the whole urban population). The median income of the performing arts audience was twice that of the urban population. Differences among audiences for the several art forms are minor. British audiences are similar to American in economic and social composition.

A principal thesis of the study is that the arts suffer from cost pressures similar to those experienced by many other service industries. Although performers' earnings are often low, their compensation tends to rise over time along with the general wage level. But in the arts, unlike other industries, opportunities for technological progress are narrow, and rising wages are not offset by increased productivity.

Except for the Broadway theater, the professional performing arts are collecting much less in admissions than they are paying out in operating costs. The "income gap" ranges from an average of 15 per cent of expenditures of regional theaters to 46 per cent for 25 major orchestras. The total gap in 1963-64 is estimated at \$20 million to \$23 million, not including outlays for new buildings. Baumol and Bowen expect the gap to widen as costs continue to rise faster than ticket prices.

Up to now the income gap has been filled mainly by private contributions with only minor direct assistance from government. The question arises whether philanthropy will be adequate in the future or whether local, state, and federal governments should substantially increase support of the performing arts. Baumol and Bowen are formally neutral on government support but appear to be sympathetic.

Four general or external benefits which may justify government support of the performing arts are mentioned: (1) national prestige; (2) indirect benefits to other business such as restaurants, hotels, and shops; (3) maintenance of the arts for the enjoyment of future generations; and (4) educational contribution. National prestige may seem a nebulous or unworthy consid-

eration, but as the authors remind us it has figured in many government decisions, including the expenditure of billions for space exploration. The educational argument is basically that exposure to live performances is necessary to develop taste and a high quality of civilization. The second argument mentioned above—the indirect benefits to other businesses—seems to me fallacious, since there is no indication that these benefits are peculiar to the arts or that they should be paid for by taxpayers rather than the business beneficiaries. Nor can I see that the future-generations argument is an independent one.

Granted that there are general benefits, it seems clear that the major benefits accrue to the audience. The government is already providing substantial indirect assistance to the noncommercial performing arts organizations in the form of tax exemptions and tax deductions for contributions. To the extent that government contributions exceed general benefits, an elite will be subsidized by taxpayers who are on the average poorer and less well educated. If subsidies follow the present distribution of artistic performances, they will be concentrated along the East and West Coasts, especially in New York City. In view of the speculative nature of all appraisals of general benefits, the lines appear to be drawn for a long and vigorous debate.

RICHARD GOODE

International Monetary Fund

The British Monopolies Commission. By CHARLES K. ROWLEY. London: George Allen & Unwin Ltd., 1966. Pp. 394. 50s.

An economist assigned the task of designing the "hereafter" would probably specify nothing but linear relationships as well as an identity relationship between *ex ante* and *ex post*. In "this best of all worlds" making and evaluating public policy would be more precise than it is in the "here and now," where relationships between variables, if known at all, are extremely complex. The *ex ante* concepts of the economist do not match the *ex post* data available to the administrative or judicial agency. Thus the concepts of the former yield limited insight into the problems of the latter. While there would be no need for such a book in the world which was so briefly described above, this book surveying the performance of the British Monopolies Commission is a needed and valuable addition to the literature.

The economist dealing with problems of industrial organization is seldom able to state, with precision, the relationship between structure, on one hand, and performance and/or behavior on the other; however, it is generally agreed that there is some relationship. The individuals or agencies charged with shaping and enforcing public policy in this area are neither able nor willing to wait for the day when the economist is able to establish the relationships to the satisfaction of all. Instead, policy must be determined and enforced on the basis of some rather vague assumptions as to the basis and nature of the relationship between industry structure and industry behavior and/or performance.

In the United States, the Antitrust Division, the FTC, the courts, and other

regulatory commissions all attempt to influence industry behavior and performance by using industry structure as the independent variable. In Great Britain, the Monopolies Commission and the Restrictive Practices Court function on the basis of the same premise. The work being reviewed is a rather detailed report on one such agency—the Monopolies Commission, and, as such, is a valuable companion to *The Restrictive Practices Court* by R. B. Stevens and B. S. Yamey (London, 1965). The two give the reader a rather exhaustive picture of the practices, problems, and results of these two agencies charged with the supervision of much of British industry.

In the present work, the author commences with the history of the government's attempts to deal with the dominant firm including its international trade policy and the contribution of the common law. He moves on, in the second part, to a detailed discussion of the structure of the Commission and its method of operation. The actual inquiries of the Commission are explored in the third and fourth parts of the book and are divided into trade association inquiries and dominant firm inquiries. The last three sections of the book discuss, in turn, statistical measures of economic performance, economic consequences of government action, and final conclusions and recommendations. This approach permits both the author and reader to examine the economic aspects of public policy. This is, to the reviewer, at least, a much more interesting and significant problem than is the investigation of the administrative problems of regulation.

Of the various issues raised relating economic analysis to the problems of public policy, the discussion of costs and prices was most interesting. The Commission has devoted considerable effort to the relationship between the two in its investigation, but the author still found significant shortcomings in their efforts. He may have been unduly critical of the Commission for there are several problems for which the Monopolies Commission is not responsible and about which it can do little.

First, there is the conceptual problem in economic analysis. In the *ex ante* world of economic theory, the relationship between cost and price at the time of the investment decision is well known and rather precisely defined. But there is no reason to believe that the same relationship should be found in contemporary company records. Second, there is the problem of accounting—particularly the accounting associated with joint products. Accountants generally assign joint or overhead costs on the basis of direct costs of one type or another. But a firm would be rational to assign such costs on the basis of what the products will bear. For example, a firm producing two products one of which is sold in a highly competitive market and the second in a market dominated by a price-fixing ring would be expected to assign a disproportionate share of the overhead and joint costs to the latter. Third, there is the difficulty inherent in the inaccuracy of the accounting process itself. The regulatory agency which examines cost and price data of the investigated firm may well find figures which were computed with the taxing agency in mind rather than an agency with the responsibility of determining the optimal degree of competition. Faced with these problems, then, one should not be surprised that the regulatory agencies have had considerable difficulty in determining the ap-

propriate cost-price relationship. In fact, one might be justified in expressing some amazement at that which has been accomplished.

The basic question raised in this book, along with other studies of regulatory commissions, is whether such a public agency can, with any confidence of success, act in such a way as to move the industrial structure toward some optimal state of social organization. Is it possible, furthermore, to define an objective function for society in such a way as to indicate what structural adjustments are needed in order to move that society to (not just toward) that optimal state? Further, given the current state of economics, is it possible for such an agency to compare the current and proposed industrial structure with all possible structures? The best that can probably be achieved is to compare the existing structure with one or two alternatives—that is, engage in only local search.

Because of the nature of the work of such agencies, economists can find a great deal to criticize. The author in his discussion of the analytical level of economic analysis criticizes the Commission for its analysis of resale price agreements, economies of scale, impact of advertising, and many other aspects of the Commission's investigatory activities. On a more practical or applied level, the Commission is criticized because there is no effective method of following up and assessing consequences of decision, enforcement is inadequate, and it has had varying degrees of support from the party in power. In sum, one gets the picture that the particular method of approaching the problem of industrial structure has considerable merit, but, in practice, the implementation of policy has been far from satisfactory. The recommendations of the author are directed, for the most part, toward making that implementation more effective. He seems to be convinced that this is possible.

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Atti della Commissione Parlamentare di Inchiesta sui Limiti Positi alla Concorrenza nel Campo Economico. Camera dei Deputati, Pols. 1-9. Roma: Servizio Studi Legislazione e Inchiesta Parlamentari, 1965. Pp. 3382. L 35.000.

This nine-volume Parliamentary Report on Competition is filled with interesting and valuable material on the structure of the Italian economy, its degree of concentration, and the principal forms of restriction on competition. The study was initiated in Parliament on April 19, 1961, and was designed to serve as a basis for antitrust legislation in Italy. Its completion had the active support of the Center-Left government (Christian Democrats and moderate wing Socialists).

The stated purposes of the inquiry were to ascertain the principal restrictions on competition; to examine the immediate causes of the restrictions; to evaluate the effects of the restrictions on economic development; and to suggest measures to eliminate the causes and effects of business restrictions. The Center-Left sponsors of the study, as well as of the proposed antitrust bill, have taken an interest in competition to improve the market mechanism and to bring business rules in Italy in harmony with those of the Common Market.

They do not support economic competition as a basis for economic and political freedom in accordance with American tradition and doctrine.

In the past, studies on cartels and business restraints in Italy have been made mostly by Communist groups to condemn the present system, not to provide a basis for public policy. With the present study, however, Parliamentary leadership for the first time declared that "antimonopoly law" is a proper instrument for the state to use in the economic field. This represents a significant change in public policy in Italy.

The Report emphasizes that the study is in an important way a *pioneer* investigation, for in Italy there has been no tradition of making industry studies, studies on business restrictions and cartels, either in the universities or by governmental bureaus. The Parliamentary inquiry was limited by the fact that many knowledgeable persons—in the universities and in business—were not willing to participate in the inquiry. Questionnaire returns from labor and business were very few, very poor, and of little use. A further limitation was the fact that in Italy there is no legislation or formal control over business agreements, so that evidence on the probable existence of restrictions was not available. Despite these limitations, the Parliamentary inquiry developed in the nine volumes a substantial amount of information on restraints in the Italian economy.

In its investigation, the Parliamentary committee arranged with experts for the making of some 30 monographic studies on the Italian economy. Eight of the monographs are included in the nine volume report. They cover (1) Industrial Concentration, (2) Patents, (3) Insurance, (4) Sugar, (5) Pulp and Paper, (6) Fertilizer and Insecticides, (7) Commercial Credit, and (8) Public Contracts with Private Firms.

The volume on Industrial Concentration will be of special interest to U.S. economists not only for the data presented but also for the statistical methods used. Industries covered include sugar, matches, cast iron, iron alloys, steel, aluminum, lead zinc, automobiles, typewriters, artificial fibers, bottled gas, and electricity. Industrial concentration has been an historical fact in Italy because of the small domestic market, long-existing financial concentration, and the policy of autarchy followed between the two wars. Industrial concentration, the report states, has not increased greatly during the past 25 years. Since World War II, the large companies have expanded mainly by entering other fields. The large companies, it is observed, have advantages in obtaining credit and in lower interest costs.

Concentration in Italy, as in the United States, has promoted unified selling. In a few areas, such as motorcycles, typewriters, and calculating machines, price competition does exist although sellers are few. In general, however, the report concludes that concentration has promoted unified selling, usually through the use of basing-point methods of pricing, managed and administered by gentlemen's agreements or cartel arrangements.

It is significant to note that the economies of the United States and of Italy are both characterized (1) by substantial firm concentration in the basic manufacturing industries; and (2) by unified selling with use of basing-point methods of pricing. Italy has no antitrust law and no antitrust action; the

United States has a complement of antitrust legislation and fairly active antitrust enforcement. Industrial concentration and geographical separation of firms and plants, however, have given rise to the common use of basing-point pricing and unified selling, regardless of antitrust.

Much material for the Parliamentary study was secured from questionnaires given to Italian professors of economics and law, as well as to various scholars abroad. In the main, the questionnaires asked for (1) copies of studies or research already done on markets and business restrictions; (2) suggestions on the main areas for study; (3) information on the most frequent forms of restrictions on competition; and (4) remedies for economic policy.

According to the Italian professors of economics, the principal sectors in Italy having restrictions on competition are the electrical industry, chemicals, cement, industrial machinery, sugar, and steel. The forms of restriction thought to be most frequently used are public regulation, particularly licenses and concessions (as in public transport), cartels or ententes, financial concentration, concerted practices (gentlemen's agreements, tacit understandings), integration, mergers, and abuse of economic power (price incursions). Although there is no formal law in Italy against *cartels* (written agreements on price and output enforceable by the civil law), such agreements are not generally openly practiced, because the Communist-dominated CGIL union members are likely to detect such agreements and use them "to make scandal" in Parliament. Thus, evidence in the report on specific cartels, as well as on gentlemen's agreements, reflects only the opinions of those answering the questionnaires.

According to the questionnaire replies, the restrictions on competition manifest themselves in unified price policy (as illustrated by basing-point pricing in cement and sugar), restrictions on entry, distortions on the use of resources, limitations on production, and limitations on investment. To secure further evidence on restrictions, the economists recommended studies on price and costs, the composition of boards of directors of corporations, corporation financial records, stock exchange transactions and prices, bookkeeping methods, a study of ownership concentration in the largest companies, mergers, and interties of foreign investments.

As remedies for restrictions on competition, the professors of economics recommended the further creation and use of public enterprise in competition. At the present time, state-owned enterprises exist in numerous, traditionally private sectors. Such enterprises account, for example, for 55 per cent of steel production, 13.5 per cent of cement, and 25 percent of petroleum distribution activities. The economists also suggested further nationalization programs. In 1962 the entire electric power and light industry—some 462 companies—was nationalized, except for a few municipal systems and industrial self-suppliers. Nationalization has also been proposed for the cement industry. It is estimated that approximately 40 per cent of all industrial output in Italy comes from state-owned firms and the nationalized sectors.

In Italy, public enterprises (government firms) have been used mainly to assist in developing an industry (airlines and shipping), to increase capacity as in steel, or to provide essential services as telephone and electricity at low

prices and rates, rather than to fight monopoly. In the case of cement, public investment reportedly did "disturb the equilibrium." However, presently, the general rule is that government firms "meet" the prices of other firms.

Further remedial measures suggested by the Italian economists are the enactment of antimonopoly legislation, the reform of corporate statutes creating and regulating share companies, an improved fiscal and monetary policy, and the continued and further use of direct price control.

At the present time, maximum price control is exercised by the Comitato Interministeriale dei Prezzi (CIP) for such products as cement, coal, lime, glass and various chemicals, grain, rice, sugar beets, sugar and molasses, electric energy, gas telephone service, hotels and pensions. Direct price control was adopted as a wartime measure. It has been continued to date, however, despite efforts of the controlled industries to repeal the legislation. With the strong influence of the Italian labor unions on government, repeal does not seem likely. In the price control program, prices are fixed on the basis of the high cost producers; and the efficient producers do not find "control" too burdensome.

The study on competition led to the introduction in Parliament, on September 2, 1964, of a bill Safeguarding Freedom of Competition. The bill provides for the condemnation of all agreements which fix prices, limit or control production, or allocate markets. This prohibition, however, the bill provides, may be declared inapplicable with respect to agreements which are found to improve production or distribution, promote scientific research, technical or economic progress, or which increase the level of employment. Dominant enterprises are forbidden to control the market in such a way as to impose unwarranted burdens or restrictions on consumers, customers, or suppliers. A dominant firm is considered to be one not subjected "to an efficient domestic-market competition." In essence, the bill parallels the antitrust provisions of Articles 85 and 86 of the Treaty of Rome (1958) applicable to interstate commerce in the Common Market. Sponsors of the bill believe that the Center-Left government will adopt an antitrust measure during 1967.

The planned antitrust law for Italy will undoubtedly serve to eliminate or modify traditional cartel agreements and provide a legal basis for regulating the conduct of dominant firms. The established pattern of concentration, however, will continue; and business firms will increasingly turn to mergers in place of cartels, business leaders report. Some cartel agreements will undoubtedly be authorized—as in the Common Market. There will still remain, moreover, the underlying problems of gentlemen's agreements and unified selling when firms are few. Indeed, the Parliamentary study points out that in numerous Italian industries today unified selling is being practiced through price leadership (*prezzi guida*) and price following, as well as by tacit pacts to match or align prices. Dominant firms are able to impose their price leadership by the menace or threat of "price incursions."

Despite the antitrust provisions of the Treaties of Paris and Rome, the cartel practices of basing-point pricing and unified selling still prevail in the Common Market countries. There is every reason to believe that they will also be continued in Italy. The general approach of antitrust in Western Europe is

to modify somewhat the formal traditional cartel behavior of established firms. It is not designed to regulate competition, to provide for fair competition, or to curb predatory and discriminatory acts. The purpose of antitrust in Europe falls short of seeking to promote competitive, independent enterprise as a basis for economic and political freedom. A further significant weakness of antitrust in Western Europe is the absence of any provision providing for private enforcement by means of treble damage proceedings. Enforcement rests entirely with government.

A problem faced by the Italian government, indeed by all Western governments—including the United States—in their efforts to improve the market mechanism, is a determination of the kind of competition called for by public policy. At present, it appears that public policy is willing to accept as a norm the concept of sales-effort competition, with prices aligned by the basing-point formula and managed by price leadership. The alternative, long advocated by economists since John Bates Clark, of competition as open, aboveboard, non-discriminatory, independent price competition (never perfect) is yet to be accepted. Basic, too, is the need to determine that, if business structure cannot be, or is not to be, modified, regulation of conduct must be exercised to provide behavior analogous to that provided by market-type competition. Antitrust in Italy, Western Europe, Canada, and the United States faces a common need to meet common problems.

VERNON A. MUND

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The New Economics of National Defense. By JOHN J. CLARK. New York: Random House, 1966. Pp. 242. \$4.95.

This book contains virtually no new material but is a summary of existing literature. Some of this literature is summarized quite well, but the discussion of more theoretical and analytical topics is rather unsatisfactory. For instance, it is asserted that price theory . . . "in its orthodox presentation has tended to assume that all relationships are *simultaneously* satisfied" (p. 9, my italics. What about Marshall's distinction between short run and long run? What about dynamic models?). Again, it is said that the use of the opportunity cost concept "carries inherently a degree of circular reasoning" (pp. 52-53)—a rather surprising statement. The section dealing with game theory is particularly confused.

United States Merchant Shipping Policies and Politics. By SAMUEL A. LAWRENCE. Washington: The Brookings Institution, 1966. Pp. xiii, 405. \$8.75.

This book is a worthy addition to the long list of publications of the Brookings Institution. It bears the usual characteristics of the published products of that venerable organization. It is carefully written. The research is meticulous. There is much evidence of the access to governmental sources of information enjoyed by scholars at Brookings. This was especially so for the author of this volume since, for at least three years during his career as an examiner in the Bureau of the Budget, he was assigned to "maritime problems."

Though some readers may be disappointed that Lawrence decided to direct his study "solely to consideration of U.S. government programs dealing with dry cargo shipping in foreign trade," most will agree that his decision was wise. A much longer book, filled with the kind of interesting, detailed data he assembled for this narrower report, would have been both exhausting and tedious for the reader.

Lawrence begins with a brief description of the merchant shipping industry, providing the usual kinds of information about its operations, structure, and regulation. This is followed by an account of the pre-World War II development of maritime policy into which he weaves observations on the political forces which helped to shape it.

After noting the all-too-familiar changes in the shipping industry—e.g., the growth of bulk cargo trade as compared with liner cargoes, the declining percentage of the total foreign trade of the United States lifted by vessels flying its flag, and the rising cost of replacing obsolete ships—Lawrence cites the military, commercial, and foreign economic policy developments which today affect the shipping industry.

Lawrence next turns to the administration of the Merchant Marine Act of 1936 since World War II. Not surprisingly, he states that considerations of national defense have provided the basic political support for the continued and increasing subsidies for shipping as provided in the Act. He also examines other types of support such as the variety of cargo preference schemes which set aside cargoes for American-flag vessels, and the registration of American-owned ships under "flags of convenience" (e.g., Panama, Honduras, Liberia) which enable shipowners to escape from certain costly requirements entailed in registering under the American flag while reserving these vessels for use by the United States during national emergencies.

In common with other students of maritime affairs, Lawrence concludes that the government program to support and develop a strong merchant fleet has not succeeded. He believes that the shortcomings of the program are the result of the government's yielding "too often to the industry's importunings for additional aid. In some instances, expediency has been substituted for prudent judgment in determining ways to meet alleged defense needs. In other cases, administrators of the maritime program have appeared to lose sight of its ultimate public purposes in their effort to be reasonably responsive to what the industry conceived to be its immediate need."

Strong words; and what does Lawrence propose? Three fact-filled chapters describing the complex machinery of government, the intricate web of interests involved in shipping, and the complicated relations between the government and the industry intervene before we are told. He then proposes, as the best of three choices, a program based upon "[a] subsidy program aimed at increasing the competitiveness of U.S. merchant shipping." It would be a "single comprehensive subsidy system covering all U.S. registered vessels in foreign trade" and would "be geared initially to supporting the current U.S. flag fleet."

The total subsidy to be distributed to all eligible shipowners would be a fixed sum. It would be allocated either through bidding by shipowners in ex-

change for performance of certain services or awarded "in proportion to net revenues earned during the year in foreign trade." Lawrence recognizes that there would be administrative difficulties under either method of distribution but believes these would be offset by the advantages of the competitive nature of both.

There are many other parts to Lawrence's proposal. Among them are relaxation of "buy American" provisions to allow U.S. ship operators to buy at least one-half their "supplies, maintenance, repairs, and other services . . . from foreign sources," and greater freedom to register vessels under foreign flags. He makes some very thoughtful recommendations for administering his proposed program—placing primary responsibility in the Department of Commerce "(or, if approved, the new Department of Transportation)" and assigning clear-cut roles to the Federal Maritime Administration, the Federal Maritime Board, the Department of Defense, and the Department of State.

To this admittedly jaundiced reviewer, much of what appears in Lawrence's fine book is not novel; nor is it probably meant to be. However, a substantial part of the book contains much that only an "insider" such as Lawrence could write about with such élan. The reader vividly senses the frustrating complexity of the federal government as Lawrence describes the forces and institutions at work. We can hope that his having made so clear the complicated political and administrative arrangements created in part by our maritime policies will provide additional stimulation to encourage serious consideration of his proposals.

WYTZE GORTER

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Land Economics; Agricultural Economics; Economic Geography; Housing

Spatial Dispersion of Economic Activity. By H. C. Bos. Rotterdam: Rotterdam University Press, 1965. Pp. ix, 99. f 21.50.

This book—originally a doctoral dissertation but in fact the seasoned work of a mature scholar—addresses itself to the following problem, first proposed by Christaller and Lösch, and formulated more rigorously by Tinbergen: Since "production, consumption, capital goods, and population are not spread evenly" in space but are concentrated in agglomerations of various size, can a "purely economic explanation" be given for the "systematic differences in the industrial structure of centers of different size?" The key to this question is seen in an associated planning problem: What is the most efficient assignment of activities to clusters of various size and distance, i.e., the optimum dispersion of economic activity?

After a brief exposition of some well-known empirical facts concerning spatial dispersion and after a (somewhat skimpy) summary of the theories of central places as developed by Christaller and Lösch and continued by Böven-ter, the author gives a careful presentation of the "Tinbergen hypothesis" which casts the general idea of central places in a more rigorous form:

- "1. In each center with an industry of a given rank all industries of lower rank are also located.
2. There is only one production unit of each highest-ranking industry in a center. This industry is the only one in the center exporting to other centers and to the agricultural area. The production of all other industries in the center is consumed within this center" (p. 21).

The first question to be answered is, under what conditions this represents the most efficient, i.e., least cost pattern of production in space? The author looks first at the optimum dispersion of a single industry both in a one- and a two-dimensional setting. The assumptions are the standard one of linear cost functions and the unorthodox one of a given demand per area independent of price. Explicit formulae can be given for the optimum market radius. These results are extended to the case where the number of centers is so large and the centers are spaced so closely that at each level demand may be assumed to be distributed continuously at an even density. At this level of abstraction "combination of plants of different industries in one center" produces no cost saving, and so a first ranking of industries can be found according to the optimum size of market areas regardless of mutual interaction.

Next savings in transportation cost resulting from combination of several industries in one center (vertical integration) are examined for several types of transportation cost functions: depending on total value of goods shipments, on the dominant flow, and finally (and conventionally) on distance. Certain combinations can be ruled out, and Tinbergen's hypothesis comes out rather strong in the case of a small number of industries, the only case examined in view of the size of the combinatorial problem.

Next the whole problem is recast as an integer program by restricting locations to a given hexagonal grid. A general analysis proves impossible, but an example produces a solution very close to Tinbergen's hypothesis.

Finally, intermediate commodities are introduced and the Tinbergen hypothesis is generalized accordingly. In this case the results of the analysis are less clear cut. Actually a review of relevant empirical facts would show that the Tinbergen hypothesis does violence to an important sector of the economy, viz., the processing of agricultural raw materials. Dairies, sugar factories, paper mills, cotton gins, tanneries, fruit canneries, etc., are all located in centers of low or sometimes lowest order and export their output to centers of higher order. The principle involved seems to be this: all processes that reduce weight and/or bulk and have modest scale requirements occur near the agricultural base. This principle is in line with broader locational analysis of transport economies and is consistent with the Tinbergen hypotheses in the case of industrial production when raw materials are imported through the metropolis or are ubiquitous: The main stream of industrial products does in fact move down the hierarchical ladder from higher to lower distribution centers as the Tinbergen hypothesis states.

While this book does not succeed in proving the Tinbergen hypothesis, it marks a great step forward in the formulation and the analysis of one of the most intricate problems in location theory: agglomeration. Many insights are

provided and numerous suggestions emerge for further research, both theoretical and empirical. For the student of location theory and spatial economics this book is a must; for the general economist it affords fascinating glimpses into an area of economic research that deserves to be wider known.

MARTIN J. BECKMANN

Brown University

The Conditions of Agricultural Growth: The Economics of Agrarian Change Under Population Pressure. By ESTER BOSERUP. Chicago: Aldine Publishing Company, 1965. Pp. 124. \$5.00.

Mrs. Boserup has observed, as have many others, that explanation of the rapid increase in world population since World War II "is to be sought, not in improved conditions of food production, but in medical invention and . . . other factors . . ." If the line of causation is not from increased agricultural productivity to increased population, perhaps it is in the reverse direction. Mrs. Boserup's book is based on that assumption, and most of its 111 text pages are devoted to attempting to prove that the assumption is true.

The proof rests essentially on a theory or a set of axioms about the way in which agriculture has evolved. There are: (1) Agriculture began (begins) in forested areas; (2) Where it is not necessary to remove weeds from among growing crops; (3) Grasslands are the result of farming with short periods of fallow; (4) Crops grown in the forest yield more food calories per acre than do crops grown in the grasslands; (5) Fertility of the soil is determined (exclusively?) by population density and by the system of agriculture; (6) At any time in the history of a community, all land belonging to the community is used in some way; (7) The system of land tenure is determined by population density and by the system of agriculture.

These assumptions about the development of agriculture and about the direction of causation lead to an evolutionary scheme for classifying agricultural systems according to the period of time that elapses between harvesting and planting on a particular plot. Mrs. Boserup finds five steps—forest fallow, bush fallow, short fallow, annual cropping, and multicropping—but recognizes that not all agriculture passes through all steps. Each step or stage is characterized by lower output per man-hour than the preceding one, with the consequence that a farming community does not willingly enter into the next stage until forced to do so by rising population. But total output in each stage is higher than in the preceding one because leisure time is reduced and more hours are devoted to farming, i.e., cultivators "get accustomed . . . to regular, hard work instead of a more leisurely life with long periods of seasonal idleness" (p. 64). In the long run, productivity of the total economy is raised as increasing population density facilitates the division of labor, the spread of communications and education, and public investment. The happy outcome of all this is that ". . . population growth often seems to be the cause of prosperity, in sharp contrast to the causation from prosperity to population growth and poverty, which was suggested by Malthus" (p. 98).

The argument is attractive, and there is much in it that seems to correspond

with history; if it were advanced as one of the possible lines of causation in agricultural change it would probably meet little serious opposition. But Mrs. Boserup means it to be *the* line of causation, and supports that thesis by a mass of assertions, mostly undocumented, about the nature of agriculture around the world and through history. Many of her assertions of fact are open to question—that irrigation is adopted because the shortening of fallow makes additional operations necessary (p. 26); that under short fallow there is virtually no agricultural work outside the plough season and the harvest season (she assumes that only small grains are grown) (p. 48); that short fallow systems are particularly susceptible to famine (p. 49); that members of primitive rural communities lack foresight and shun hard agricultural work (p. 54); that early systems of agriculture are characterized by large amounts of free time not required, or used, for economically productive activities (*passim*); that high food prices in Japan in the late 1940s resulted from increasing population density (p. 110); and that expanded production of export crops in the tropics was always at the expense of food crop production (p. 119).

More troublesome is the validity of her basic assumptions about how agriculture has evolved; some of these seem to be contrary to the evidence and none of them can be taken as proven. The analysis is also handicapped throughout by failure to recognize important differences between agriculture under temperate and tropical, particularly equatorial, conditions, and between the impact of population growth at 0.3 to 0.5 per cent per annum and recent rates of 2.5 to 3.0 per cent.

Mrs. Boserup's book suffers from two major defects, and they are likely to weaken the influence of many of her perceptive comments: it is argued too single-mindedly in support of one, exclusive proposition; and it is too short! The problem is too complex to be summed up in another counter-Malthusian type of hypothesis, and 111 pages are too few to explore the implications of many of the relationships referred to, or to examine carefully the evidence for their validity.

W. O. JONES

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The Economics of Agricultural Development. By JOHN W. MELLOR. Ithaca, N.Y.: Cornell University Press, 1966. Pp. xiv, 403. \$10.00.

This book fills an important gap in the economic development literature. For the first time a monograph is available that is suitable as a text for courses dealing with the economics of agricultural development taught at the undergraduate level.

The book is organized under three major headings: I—The Role of Agriculture in Economic Development; II—The Nature of Traditional Agriculture; and III—The Modernization of Agriculture. I covers both those changes that are internal to the agricultural sector and the changing relationships between agriculture and the rest of the economy, particularly those relationships expressed through the factor and product markets, during the development process.

Mellor has consistently devoted his research and writing to problems of agricultural development for over a decade. The authority which he brings to the subject is a major strength of the book. It also is responsible for the somewhat uneven treatment given to the several topics. On topics which Mellor has emphasized in his own research the treatment tends to be both authoritative and as sophisticated as treatment designed for an undergraduate text will allow. The chapters on "Income and Growth and the Effective Demand for Food" (Ch. 4); "The Farm in Traditional Agriculture" (Ch. 8); and "Labor Use and Productivity" (Ch. 9) are particularly good. In many other areas the treatment is less adequate. The chapter on "Planning Agricultural Development" (Ch. 21) is particularly weak.

It should be emphasized that this unevenness should not be taken as a criticism of Mellor's work but rather of the limited contribution by the profession as a whole to the analysis of the economics of agricultural development. The scholarly work resulting from the rather intensive involvement of agricultural economists in international work during the last decade and a half is pitifully weak. One response to this criticism might be that, working as applied economists, major attention was placed on getting agriculture moving rather than writing monographs. My own reaction is that the particular form of involvement, typically AID (and predecessor agencies) contracts for institutional development, failed to provide an environment that has permitted or encouraged effective research efforts.

A second major strength of the book is that Mellor is not blinded to the complexities of the agricultural development problem by single minded adherence to one of the several models which are currently competing for recognition in the effort to establish a "new development economics." He does not employ either the Rostow growth stage approach of the Lewis-Ranis-Fei two-sector approach as an exclusive organizing principle.

An elaboration of the Rostow stage approach is utilized as an organizing principle in Chapter 13, "The Economic Framework of Agricultural Modernization."¹ A Lewis two-sector scheme clearly is implicit in Chapter 14, "Labor Use and Productivity." At no point does Mellor attempt to present either (a) a careful criticism of the limitations of existing development models, or (b) a general analytical framework of his own. As a result the information which he presents, though generally interesting, frequently degenerates into a pattern of "conventional development wisdom" through a lack of any set of integrating principles.

In conclusion: (a) this is an important and useful book, (b) its weaknesses stem largely from the unsatisfactory nature of current development literature, (c) its strengths are largely a product of Mellor's own impressive research performance, and (d) it would be twice as good if it were half as long.

VERNON W. RUTTAN

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¹I have commented more extensively on this part of Mellor's work in V. W. Ruttan, "Growth Stage Theories and Agricultural Development Policy," *Australian Jour. Agric. Econ.*, 1965, 9, 17-32.

The Economics of Air Pollution. EDITED BY HAROLD WOLOZIN. New York: W. W. Norton and Co., 1966. Pp. 318. \$5.00; paper \$2.95.

The book is primarily a collection of essays, most of which were delivered at a seminar at The American University. In addition, it contains a staff report to the Senate Committee on Public Works and a copy of the Federal Clean Air Act with amendments. Three of the essays (those by Allen V. Kneese, Edwin Mills, and Thomas Crocker) pertain primarily to the nature of the "social problem" of air pollution and the alternative means by which it can be solved. Two essays (those by Edward Crowder and Helen Lamale) provide generalizations about the sources and uses of federal statistics with specific comments about data used in air pollution studies. A paper by Ronald Ridker is a good summary of many problems encountered in measuring the cost of air pollution. An essay by Lester Goldner is a case study of the proposals, political pressures, and legislative acts which contributed to the development of the Boston pollution control program. The remaining two articles are hard to categorize. Leslie Chambers sets as a goal not to "engage in a philosophical polemic on the nature of man, the cosmos, or even the biosphere." Toward this goal, however, he takes no more than one or two steps. Harold Wolozin indicates, in broad terms, positions taken by various people on such topics as the purpose of welfare economics, welfare functions, value judgments, cost-benefit analysis, public goods, etc. The report to the Senate Committee (80 pages) could have been omitted without great loss. The only things of interest in this document are lists (without documentation) of major incidents, damages and illnesses which someone has claimed are connected to various items on a list of possible pollutants.

The essays contain many interesting observations and generalizations about the nature of air pollution, externalities and the problems involved in the associated cost-benefit studies. In this respect the book will be of considerable value to anyone interested in these problems. It begins with Allen Kneese drawing from his experience in dealing with water pollution to indicate the parallel and divergent aspects of air pollution. He finds that similarities arise in the many problems of identifying relationships among pollutants, their sources and their effects. When it comes to problems of control, air pollution is considerably more difficult. All authors repeatedly contribute good examples of the problems.

On the other hand, the book contains little of general theoretical interest even though most of the essays are of a theoretical nature. Furthermore, in my opinion, these authors are guilty of a drastic oversimplification of the "social problem" they wish to solve. The contributors who deal with the problem (Kneese, Mills, Crocker, Wolozin and in some respects, Chambers and Ridker) present in essence, the same analysis and similar conclusions. Crocker provides an extension or two. The central thesis is most clearly stated by Edwin Mills. Consequently, I shall discuss his paper in detail.

Mills's argument runs something like the following: There is a "market failure" because costs "are inadequately imputed to the sources of the pollution." Aside from income distribution considerations, the problem is to find a way to "internalize" these costs. The alternatives are to tax (according to the

social cost) or bribe (foregone bribes are an opportunity cost to the firm). The tax alternative is preferable to the bribe (payment) for two reasons. (1) "There is no natural origin for payments. In principle, the payment should be for a reduction in the discharge of pollutants below what it would have been without the payment. Estimation of this magnitude would be difficult and the recipient would have obvious incentive to exaggerate the amount of pollutant he would have discharged without the subsidy." (2) "Payments violate feelings of equity many people have on this subject. People feel that if polluting the air is a cost of producing certain products, then the consumers who benefit ought to pay this cost just as they ought to pay the costs of labor and other inputs needed in production." Thus we choose the tax alternative and create an authority which undertakes cost-benefit analysis to determine the proper tax.

Now to me this analysis is incomplete at every step. The first step is a statement of the problem and the alternative solutions. Presumably there is a "market failure" in that at an "equilibrium" there exist gains from trade (the equilibrium is not a Pareto Optimum). Either some resources used (individual's freedom from the effects of pollutants) remain uncompensated or individuals would like to pay the producer for not exercising some of his rights (the right to produce the effects). The possible exchanges do not take place because of the free rider, hold out, or prisoner's dilemma phenomena typical of externality and public goods problems. Aside from distribution, the problem is to devise a means by which these gains from exchange can be realized. Mills argues that a tax on the producer will correct this "failure." Here he fails to explain that unless the tax revenues go to the damaged party some odd results can occur. The fact that "marginal social cost equals marginal social benefit" is of little consolation to an individual who remains uncompensated for choking on the "optimal" amount of smoke. It is easy to construct an example where, unless payment is made for the factors used (for lack of a better name call them "suffering from effects"), everyone is hurt by such a tax scheme. Even the names of the alternatives are really misnomers. Without additional rather elaborate arguments, the alternatives are either tax the people and bribe the producer (the bribe) or tax the producer and bribe the people (the tax). In either case, where the money comes from and where it goes must be an integral part of the analysis. It cannot be neglected as these authors suppose.

The second step of Mills's analysis is the choice between possible "solutions." Here I have two points to make. Mills does not indicate that the choice between taxes and bribes is actually a choice about property rights, or ownership. Furthermore, unless some type of income redistributing mechanism is specified, the choice between a tax or bribe involves a decision about the income distribution alone. I suggest that the "feelings of equity" which Mills observes are actually "feelings about dollars." While some people claim they own the right to be free of such effects unless compensated, others may claim the right to produce them unless compensated. I see no reason, since there is a distribution question at stake, for the "natural origin" of rights to be "zero effects" as a choice of the tax alternative presupposes. Secondly, the "exagger-

ation problem" Mills recognizes is not unique to the bribe alternative. It is symmetrical. If the tax alternative is chosen, the damaged individuals have the obvious incentive to exaggerate the amount of damage.

The final step in Mills's analysis is his "tentative" policy recommendation. The possible relative merits of this line of attack are not obvious since Mills does not address himself to some fundamental aspects of the problem which, explicitly or implicitly, any "solution" must involve. Several important considerations can be listed under the heading of property rights. In what terms should property rights be defined? Crocker argues that, with allowance for certain meteorological variables, property rights should be defined in terms of the right to take certain actions, e.g., the rate of pollution emission and the time interval of emission. This is an interesting proposal but it is not the only one worth examining. Rights can also be defined in terms of the effects of actions. Producers may or may not have the right to cause certain effects. That is, individuals may or may not have the right to be free from certain effects. There are also problems of specifying what will suffice as "proof" of the violation of a right. Finally, and perhaps most difficult, "who owns what" must be determined.

Aside from the important area of property rights there is the complex problem of determining the exchange mechanism. It should be one through which the gains from exchange are exhausted. The literature does contain some suggestions.¹ Even the courts can be viewed as such a mechanism. Although the cost of this system is great it is not clear that an equally accurate cost-benefit study is less expensive—in both cases the information to be found is the value of the damage. The essays in this book contain some observations in this respect. Crocker touches upon the possibilities of a model where groups bid for certain rights. The group size and cost sharing arrangement would be determined by an air pollution authority. Goldner's whole paper is devoted to the thesis that political action in the Boston case was due to a single major incident. He concludes that the object to be investigated should be the community decision framework. Ridker suggests that simply trying out controls and observing the electorate's reaction is worth considering. In any case, the relationship between the proposed "solution" and the initial "problem" should be made explicit.

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¹ O. A. Davis and A. B. Whinston, "On Externalities, Information and the Government-Assisted Invisible Hand," *Economica*, N. S., Aug. 1966 33, 303-18.

The Economy of the Israeli Kibbutz. By ELIJAHU KANOVSKY. Harvard Middle Eastern Monograph No. 13, Cambridge, Mass.: Harvard University Press, 1966. Pp. ix, 169.

Eliyahu Kanovsky's book offers a concise account of the fundamentals of the Israeli collective settlement—the Kibbutz. The first chapter of this book consists of a brief and accurate review of the nature and institutional structure of the Israeli agriculture. An attempt is made to familiarize the reader

with the noncooperative Jewish settlement, the Arab village, and the cooperative family farm—the moshav—which together with the kibbutz constitute the diversified framework of rural living in Israel.

The second chapter deals briefly with the historical and ideological background of the kibbutz movement. A review of the role of the kibbutz as a tool in the Zionist attempt to reestablish the Jewish community in Palestine in the past precedes an appraisal of the role played by the kibbutz movement in the state of Israel today. The author then proceeds to describe the internal organization of the individual kibbutz and the network of kibbutz federations.

The third chapter is devoted to a more detailed description of the operation of the kibbutz economy. The author is concerned with the determination of resource allocation, and the investment and financial operations with special reference to the internal mechanisms of the collective unit and the policy of the settlement and governmental agencies at the national level. The fourth chapter concentrates on recent developments in the kibbutz economy. Special attention is paid to the process of industrialization on the one hand and changes in the consumption patterns of the kibbutz household on the other.

Whereas the first four chapters are mainly descriptive, Chapters 5 and 6 attempt to analyze and appraise the kibbutz economic performance or in the words of the author, the kibbutz productivity and profitability. To show the kibbutz's relatively high productivity, the author quotes input and output indices for the average kibbutz in the 1950s. In terms of yield per man years the kibbutz is reported to be superior to the other forms of agricultural settlements in Israel. Chapter 6 deals with kibbutz profits. These are defined as the difference between gross returns and costs. Costs in the case of the kibbutz—which constitute a firm and a household at the same time—include its consumption expenditures. Thus profitability in this case is the result either of the firm's efficiency or the household's thriftiness or both. Profitability may be envisaged as a criterion of the firm's efficiency if, and only if, consumption expenditures are considered to be predetermined. On the other hand, profitability as a yardstick of thriftiness alone necessarily assumes a given efficiency of the firm unit. This of course is a major weakness if the attempt is to employ profitability as a major criterion of the economic behavior and performance of the kibbutz.

The author presents some evidence—all second hand—according to which the "average" kibbutz profitability is shown to be rather low. In an attempt to explain this phenomenon the author offers an array of propositions, which he summarizes in Chapter 7. As external factors affecting the kibbutz profitability the author mentions (a) inferior location, (b) weak financial structure, (c) inability of the kibbutz to attract new settlers, (d) adverse parity of agriculture prices, and (e) expensive educational policy of the kibbutzim. Among the internal factors the author mentions: (a) the structure of the kibbutz farm, traditionally and ideologically geared towards mixed farming, (b) a ban on the "exploitation" of (nonmembers) wage labor; (c) inefficient and costly consumption patterns, (d) a misleading bookkeeping system and (e) extensive and expensive political activity. This classification of the various factors listed above is arguable. For instance, neither the financial structure

nor the educational policy is really "external" to the individual kibbutz unit. Yet, the more serious shortcoming is the lack of a consistent attempt to identify the underlying relationships and to weigh the relative importance of the various factors.

Presenting the case of the kibbutz's "low profitability" the author compares the economic performance of the kibbutz with that of the family farm in the moshav. In this attempt—which relies upon rather limited information regarding the moshav—the author overlooks the important distinction between the individual member-farm unit in a moshav and the moshav village as a whole, an unnecessary distinction in the case of the kibbutz. For instance, the author emphasizes "defection" as a major kibbutz shortcoming but fails to recognize the inability of the moshav as a whole to cope with a similar phenomenon of outmigration. The author is correct in emphasizing the ideological impediments involved in the case of the kibbutz. However, similar elements affect the moshav as whole though not necessarily the individual family farm.

Considering ideology the author's approach is somewhat naive. The third chapter, for instance, opens with the proposition that the abolition of private property and the socialist tenet "from each according to his ability and to each according to his needs" are antithetical to the concept of profit maximization. While it is true that individual kibbutzim adhere to the "socialist" yardsticks in remunerating their own membership there is no adherence to "socialism" in the way they handle their affairs with the "outside world." This includes—contrary to Kanovsky's impression—inter-kibbutz relationships. In other words the rich kibbutzim—and some of them are quite well to do, despite the reported low profitability of the "average" kibbutz—do not share their property or net returns with the poor collectives. The kibbutz may be and generally is looked upon as a "profit-maximizing" economic unit, subject to some limitations which may have a lesser effect than some union regulations such as the seniority rule in a typical "capitalistic" enterprise.

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Labor Economics

Wage Determination in an Integrating Europe. By MALCOLM R. FISHER.

Study No. 6 relating to European integration published under the auspices of the Council of Europe. Leyden: A. W. Sijthoff, 1966. Pp. 104. f. 10.

This theoretical monograph is difficult to understand, partly because the author ventures into the field of psychology and partly because he fails to integrate his contribution into the existing body of economic doctrine. It has very little to do with wage determination in an integrating Europe.

I shall first present the most interesting new idea I was able to synthesize from the text and then I will sketch the particular framework in which the author clothed his idea.

The pure theory of the gains from international trade permits us to make the analytical distinction between production and consumption effects. Thus, if a closed economy is opened up to trade it is possible to identify the gains in

utility which are due to the more efficient production and increased world output, even if excise taxes or subsidies force consumers to continue paying the pre-trade prices for commodities. The increased utility stemming from the removal of these commodity-price distorting taxes and subsidies in turn is considered to be the consumption effect. Now when international trade is opened like this, the relative prices of factors are likely to change, as are the compensations for different labor skills. The welfare gains (or losses) accruing to labor of different skills can be considered as consisting of two parts, analytically similar to production and consumption effects. First, the change in welfare caused by the new money wages which accrue when workers are not allowed to switch occupations. Second, there is an additional change in welfare associated with the switching of occupations which, given the workers' preferences, normally have differing amounts of disutility attached to them for a given amount and intensity of work. This basic idea of differing disutility of work for alternative occupations has some interesting implications for the traditional theorems from the economics of integration, migration, and the pure theory of international trade. The author ventures into these areas, giving an adequate but not particularly lucid exposition of the standard literature, and attempts an analysis of these implications. In my view he has not been very successful.

The reason for this lack of success is that the author tries to set out a new theory of the supply of labor, which he admits is a "mere sketch" (p. 95), and which on closer examination turns out to be not a theory of the supply of labor, but an attempt to use psychology to explain the formation of preferences. After deploring that economists have considered all work as having negative and goods as having positive utility he suggests that as a result of his theory "job satisfaction then acquires an importance hitherto accorded to goods satisfaction alone" (p. 13).

Fisher does know that job satisfaction is incorporated in the traditional theory in the sense that jobs are chosen by individuals maximizing utility within the constraints of the wages paid and personal satisfaction obtained in alternative occupations, quite analogously to the theory of consumer choice where relative prices and personal tastes determine the purchase of commodities. Without saying so explicitly, the author has tried to develop a theory of preference formation as can be seen from the terms he uses to describe an individual's "capacity": "mental and manual ability; ability to assimilate and develop; versatility and adaptability; dependence on externals" (p. 16), which through training and experience become what he calls his "activity frontier."

The formation of tastes and preferences may well be a significant area for future economic analysis and lead to fruitful cooperation between sociologists, psychologists, and economists. Fisher's little book might be hailed as a path-breaking work, if future generations can understand sentences like this: "Work experience stresses the advantages, *ceteris paribus*, of a labor force of higher average age than would be justified in its absence if production possibilities in the nearer future are to be maximized" (p. 66).

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Critical Issues in Employment Policy. Edited by FREDERICK H. HARBISON AND JOSEPH D. MOONEY. New Jersey: Princeton University, 1966. Pp. viii, 163. \$3.00.

Dimensions of Manpower Policy: Programs & Research. Edited by SAR A. LEVITAN AND IRVING H. SIEGEL. Baltimore: The Johns Hopkins Press, 1966. Pp. 299. \$6.95.

"Hydraulics is to plumbing, as ——— is to manpower." The absence of an acceptable answer to the above question underlies the intellectual ferment in these two books. Until a clear, thoughtful response is forthcoming, much of the material in these works and the surfeit of writing on manpower will continue to be intellectual lint on the blue serge of policy (by government, foundations and other diverse sources).

Critical Issues in Employment Policy (referred to as I) is the compilation of papers and commentaries at a conference in Princeton in May, 1966. It is a relatively homogeneous work. The seven papers center on policy needs and cover both economic issues, such as the needs of local markets, and governmental issues of leadership and intergovernmental cooperation.

Dimensions of Manpower Policy: Programs & Research (referred to as II), seventeen papers, is a "do it yourself" *festschrift* by and for the twentieth anniversary of the W.E. Upjohn Institute for Employment Research. There is an absence of focus in this book which is uneven in subject matter as well as writing style. All the authors are totally or partially connected with the Upjohn Institute. It would be a rare group of scholars under one roof who could produce significant papers on demand for a deadline.

Author overlap is minimal with only Garth Mangum presenting papers in both I and II and Herbert Striner presenting a criticism in I and a paper in II. Subject overlap is considerable and shall be discussed under the headings of data needs, experience, and administration. A missing thread in both works serves as a unifying force.

Anyone who has worked in the field of manpower knows that policy and analysis suffer from data deficiency. Ross (I), Striner, Morton, Sheppard and Siegel (II) are strong advocates of increased research efforts. Nearly all the other writers overtly or by implication lament the deficiency in knowledge. Their emphasis is placed on expansion of what we are already doing. They lay great stress on micro-analytical data needs relevant to jobs, demand and other labor market data on a local market bases. The strong structuralist undercurrent in these works seeks to match worker to employer as found in space and time.

There are only vague hints of the great failings, the emptiness of the statistical categories. Thus, the continued accumulation of statistical data in computers by occupations (currently defined) for counties will not really help answer the problem of putting our labor force to more efficient employment.

Morton (II) stresses an electronic manpower data bank and implies that a sophisticated statistical methodology is the proper research approach to end the manpower problems. Unfortunately, the categories into which we shall organize the data are our first gap. The failure to take due cognizance of the

qualitative deficiencies will make us choke on a pile of not too useful "facts."

Our empirical work must be purposive and not comprehensive in some casual way. For example, until we have a theory of occupations integrated with capital theory and the corpus of modern economics, our acquisitive instincts may do us more harm than good.

Absence of an analytical schema means we lose the basis for experience. Joseph Mooney (I), in an interesting essay on the activities of a Neighborhood Youth Corp in Newark, alludes to one form of the real output of the N.Y.C. as a crime combatant. What is lacking and could be done is construction of a test for the programs' long-run effect on a variable such as recidivism based upon different groups propensity to offend. Without care and design in special projects, as well as gross statistical design, we know neither where we have been, or where we should go in a policy sense.

The absence of direction fostered by a casual notion of past events is not compensated for by strong direction in operational procedure or goals and policy. Robson-Mangum (I), Davidson (II), McKeen (II), Belitsky (II) and Cassel (I) are concerned, though not exclusively, with the independent impact of organization on the dependent variable defined as poverty, unemployment, etc. Whether the subject is intragovernmental organization, state, local and private activities as substitutes, or complements for one another in the production of manpower policy, we are left at sea over the absence of an objective function. Davidson (II), a political scientist, does a scholarly job on the New Federalism, but his economics are wanting. The rest playing amateur political scientists and worse organization theorists, lend little insight to an enormously complex field. A stern lesson is that economists should stick to their knitting.

How we are to organize, and the appropriate relation of decision units, is crucial to successful operation of manpower policy. While Harold Taylor (II) asserts "a sound consensus for acceptable policies can truly be developed" there is no apparent consensus. The pure issues of state-federal relationships obscure the more important problem of the most effective type of organization to increase employment, or the optimal program to satisfy a number of categories within boundaries.

One of the frontiers for this apparently bounded subject is the area of the poor; in fact a strong remedial tone permeates most of the essays. Joseph Kershaw (I) makes an interesting argument for the government as an employer of last resort for the poor; and Streiner, in commenting on this, points to the role of transportation in clearing the local labor market. On the other hand, Levitan (II) asks why a war on poverty? He, however, never does answer the question, but contributes to an understanding of the activities of OEO complementing the essay of Mooney (I). The role of the poor in the policy mix is asayed with some sympathy and would be useful reading for most.

Geographic areas of distress are also in the province of manpower. Thus, Belitsky (II) and Holmquist (II) interestingly view specific problems of activating local communities. Burt (II) leaves one with homilies on the desirability of cooperation while the other two have more insight if not analysis.

Neither book will whet the analytical palate, but this is not to imply that

there are no theoretical problems worthy of examination—a fact recognized by the authors. The works being uneven, it would be useful to take parts I, II and IV of “Critical” and the last two sections of “Dimensions” and splice them as a moderately valuable work.

As manpower policy approaches puberty, it is hoped a clear demarcation between scholar and administrator will form. We cannot have manpower be both a discipline and an operational set of activities; thus the answer to the opening question cannot be “Manpower.”

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Soviet Trade Unions and Labor Relations. By EMILY CLARK BROWN. Cambridge: Harvard University Press, 1966. Pp. ix, 394. \$6.95.

Industry and Labour in the U.S.S.R. Edited by G. V. OSIPOV. London: Tavistock Publications, 1966. Pp. viii, 297. \$8.00.

Despite the congruity of the titles, these volumes deal with different ranges of subject matter. Dr. Brown is concerned rather narrowly with trade unionism, while the Osipov book contains a number of essays on what can best be described as industrial sociology. The only point of contact is a common focus on the Soviet labor market.

Soviet Trade Unions is the more interesting of the two books. It contains the best available account of the structure, government, and operation of labor institutions that have undergone many drastic changes in the half century of Soviet power. Starting out as quasisyndicalist bodies, they were reduced by Stalin to relatively unimportant appendages of the state and given the responsibility of spurring workers on to greater productive efforts. Their functions began to shift after the Twentieth Party Congress, and they have gradually taken on more of a “consumptionist” hue, particularly at the local level. However, they continue to operate as “productionist” organizations as well, and to exercise some governmental functions, including administration of the social security system. Dr. Brown has had as much access to Soviet enterprises as any American in recent years, but she was not able to arrange for more than a casual visit to any single plant. For this reason, much of what she has written describes how people say things work, rather than actual practice. She is careful to make this clear to the reader, but there are occasional lapses. Can one really say that “elections appeared to follow the democratic rules and local officers seemed to consider themselves responsible to the members who elected them” in the face of constant Soviet self-criticism in this regard? Or that “if management tries to get rid of a critic on the pretext of reducing the staff, this can be prevented”? Or that “workers do not in general resist technological change. Improvements typically are to the advantage of workers.”? Sweeping statements of this nature, which would be difficult to prove even in the best of circumstances, are impossible to justify in the Soviet context.

I would take issue with Dr. Brown on one important matter of interpretation. She points out quite correctly that strikes have no place in the Soviet

system, and suggests that an occasional strike might have a salutary effect on labor relations. But then she goes on to write of the "withering away" of the strike in the West, and implies that some convergence between East and West is taking place in this respect. Apart from the fact that the strike is still very much alive in the West, one must be clear that there is a fundamental difference between a system of industrial relations which allows strikes, and one which does not, that the line is a sharply drawn one and not just a matter of shading. It may be true that, in the West, "the trend in modern labor relations seems to be toward seeking other channels for the settlement of disputes," but the foundation stone on which Western industrial relations systems rest is the ultimate right to strike. Despite their workers' councils, the Yugoslavs have recently acknowledged that the strike may be a legitimate weapon in a socialist state, but the Russians have given no indication that they are prepared to move in this direction.

In general, Dr. Brown has done an admirable job of presenting a considerable amount of information difficult to come by. She has chapters on national, regional, and local trade union bodies; the settlement of plant grievances; and the specific functions of the unions in other spheres. (One subject that she does not treat, presumably because her field work was completed in 1962, is the attitude of the unions toward Libermanism. I suspect that the Soviet trade unions have been quite conservative on this issue.) *Soviet Trade Unions* is the standard work on the subject, and is likely to retain that status until the Russians permit outsiders to make studies in depth.

Industry and Labour in the U.S.S.R. is quite a different proposition. It is intended as a representative sample of recent work, some of it never before published. Some 60 papers were collected by a Soviet editorial board, and 18 of these were selected by Maurice Hookham of the University of Leicester on the basis of potential interest to Western readers. When reading recent Soviet writing in the social sciences, one constantly alternates between hope and despair: hope because there are many signs that Soviet scholars are at last emerging from the "cult of personality" era, despair because many of them are finding it so difficult to surmount the past. Of the 18 essays in the volume, two impress me as being of possible interest to psychologists, two more may have some value for specialists in vocational education. The rest, with two exceptions, can be of no earthly use to anyone, consisting as they do of commentaries on tables taken from Soviet statistical abstracts, or containing information such as that boiler-assemblyman A. Markin and his team at the Urals Heavy Engineering Plant managed to complete their course work in a technical education program, or passing on the following: "An analysis of the composition of rationalizers working in shops No. 4 and No. 5 of the Pervouralsk Pipe Plant has revealed that rationalization suggestions are made by 50.5 per cent of the setters working there, 42.5 per cent of the electricians, and only 23.2 per cent of the operators." And what is one to make of the numerous passages such as the following, which summarizes what purports to be an essay of a scientific character: "In the transition from socialism to communism, the intelligentsia is playing a greater role. It is helping to create the material and technical basis, to raise the standard of education, culture and technical

knowledge among the people, to promote sciences, technology, the arts, and public health, and to foster communist consciousness and morals."

On the hopeful side, there is an interesting article by V. N. Shubkin of the Novosibirsk Institute of Economics on the occupational preferences expressed by secondary school graduates in that city in 1962. He compares the stated preferences with actual paternal occupations, and finds that by far the greatest continuity prevailed among children of men in technical and physico-mathematical fields; that in the biological sciences, it was mainly the daughters who were maintaining continuity; and that in the humanities (it is not clear from the context whether this includes economics) it was *only* girls who wanted to follow in the footsteps of their fathers. Virtually no one wanted to enter into farm work or the public service (!); a fair number were interested in careers in industry; while "one hundred per cent of the children of intellectuals and of workers in industry and the public services, and eighty-seven per cent of the children of collective farmers want to enter an intellectual profession."

The second essay that is well worth a careful reading is by the late F. Yu. Aleshina of the Institute of Labor in Moscow. It consists of an analysis of budgets for 100 families in three cities for the months of March in 1951, 1956, 1959, and 1961. It is quite objective and almost entirely devoid of the propagandistic overtones that have characterized most Soviet writing on consumption in the past. For example, it is acknowledged that dependents of textile workers have a higher labor force participation rate than those of steel workers simply because the latter are higher paid, a statement that one would not have found a decade ago. We also learn that Engel's law operates in a socialist economy; that caloric intake is positively correlated with family income (indeed, in 1961, Soviet families in the highest income bracket were consuming 3,500 calories each day per person); and that television has adverse effects on theater attendance. The economics of consumption is a very popular area of study in the Soviet Union at the present time, perhaps reflecting Russia's attainment of a new stage of development, and this essay is a good example of the kind of work being done.

The papers were translated initially in Moscow, and then worked over by Mr. Hookham. He has done a magnificent job; they read as though they had been written in the original by good English stylists. Let us hope that the further volumes in the series which are promised, to be concerned with rural and urban studies and with problems of methodology, will be more worthy of his skill.

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Unemployment Insurance in the American Economy. By WILLIAM HABER AND MERRILL G. MURRAY. Homewood, Ill.: Richard D. Irwin, 1966. Pp. xx, 538. \$8.50.

The description on the dust jacket is accurate: "Here is the first comprehensive book on unemployment insurance in the United States, providing in

one volume the principles of unemployment insurance, its history, and a detailed analysis and discussion of the live issues in the field."

Thirty years is a long time to wait for this "first," but the book is worth the wait. Out of what was once a tangled forest of state and federal laws, mountains of documents and reports, swamps of studies and debates—Dean Haber and Dr. Murray have made an ordered suburb with paved roads, street signs, even house numbers. Teachers of labor economics and of social welfare programs will sleep with it under their pillows and smile in their dreams over the added free time they will have, now that so much of their work has been done for them.

The two authors were ideal for the job. Intimately associated with unemployment insurance since its beginning, they brought to the task more than 60 years of combined experience. The book's comprehensive coverage and balanced treatment of all aspects of the subject reflect the authors' long preparation.

After 100 pages of background information on the nature (Part I) and history (Part II) of unemployment insurance, there follow nearly 400 pages of analytical description of unemployment insurance provisions, accompanied by a discussion of the major issues surrounding each provision. The final 100 pages discuss "problems in relationships" between federal and state governments, between unemployment insurance and the employment service, and between unemployment insurance and other related programs.

In discussing issues, the authors frequently indicate their own preferences, which in general are close to, though not identical with, the recommendations of the Johnson administration as reflected in the 1965-1966 bill, H.R. 8282. Despite their frequent expressions of personal preference, the authors manage to maintain an admirable objectivity in presenting both sides of controversial issues. They also supply numerous bibliographical references in which the reader can find further development of the chief debates.

As the only comprehensive treatment of unemployment insurance, this book is likely to do service for a long time and to go through many editions. If some modifications were made in the future editions, a very useful work could be made even more useful. The following critical observations are made with this constructive objective primarily in view.

In a work as comprehensive and detailed as this one, some errors are inevitable. But there are too many printing errors. All numbers, especially dates, need to be rechecked. The table of contents is detailed and clear. It is a good tool. However, it could be improved by a list of tables. The endless and annoying repetition of *op. cit.* makes one wish the book had used the *AER's* superior system of bibliographical reference.

In a program like unemployment insurance, where the largest ten states account for 60 per cent of all covered workers and the smallest ten states account for only 2 per cent, statements that X number of states have a certain provision, or have had a certain kind of experience, would be more meaningful if accompanied by parentheses indicating the percentage of covered workers in the states spoken of. At the very least, the book should include a table show-

ing the percentage of covered workers in each state. The absence of such a table is a major defect in a work that undertakes to describe a system consisting of 52 separate programs (the District of Columbia and Puerto Rico are included).

In a reference work, the index is unusually important and deserves professional handling. If one may judge by the elementary errors made in the construction of this index, it was done by an amateur. For example, the reader is told that "the largest unemployment benefit plan was that of the General Electric Co." (p. 74). One looks in vain for this important fact in the index under "G" or "E." Certain that it must be there somewhere, I finally found it under "V" (voluntary), which was a subdivision under "U" (unemployment insurance), an arrangement which was simply a repetition of the table of contents. This is a typical mistake of the amateur indexer.

In its treatment of some issues, the book repeats unthinkingly—so it seems to me—positions and data that for years have been a part of the official "line." (It would not be surprising if this happened, considering the multitude of problems covered by the book; not all of them could be rethought.) One example is the book's treatment of the problem of "abuse," whose solution is crucial, as the authors explain, to the development of unemployment insurance. In support of its statement that "the amount of willful misrepresentation is small" (p. 137), the book uses the kind of argument employed for years by the Bureau of Employment Security: "In fiscal year 1954 . . . of every \$100 paid out in benefits during the year, only 31 cents was an overpayment resulting from fraud."

These data reflect only *detected, provable* fraud perpetrated by working violators. Much misrepresentation by claimants is not detected; much that is detected cannot be proved to be deliberate; and, most important, the non-working violator is probably the source of more misrepresentation than the working violator. As a measure of the seriousness of misrepresentation in the program, "31 cents out of every \$100," is the kind of statement that produces a credibility gap. The gap is the more unfortunate because it is unnecessary. The full truth is something the program can easily live with.

For another example, in a statement of the adequacy of the maximum benefit the book's Table 11-1 (p. 125) repeats data which include Illinois and Michigan. These states, which use a variable maximum benefit, should never have been included. The unsuspecting reader is left with the impression that in Illinois, for example, only 40 per cent of the claimants receive a benefit that meets the norm of adequacy (a benefit equal to half of wages). But as a matter of fact, in fiscal 1966, 54 per cent of Illinois beneficiaries received a benefit equal to half their gross wages and 67 per cent received a benefit equal to half their take-home pay.

For a third example, in its otherwise excellent treatment of experience rating, the book repeats a common error in its description of the central argument of this kind of tax system; "The allocation of costs argument is based on the assumption that the costs of unemployment compensation will be passed on to the consumer in higher prices." To be correct, the sentence should have continued: . . . or be borne by the owner in the form of lower profits, or be borne

by the worker in the form of lower wages, or be divided among the three. It is an erroneous test of the social-cost argument to inquire into the effect of experience rating on only consumer prices. The source quoted by the authors (p. 343) does little to support their position, for the writer of the quoted Ph. D. dissertation is thoroughly confused on the issue of social cost. I would hope that in any future edition of the Haber-Murray book the social-cost argument would be rethought and rewritten.

A final example is the book's treatment of disqualification provisions. In view of the lack of information on the effects of existing disqualification provisions (this is one of the unexplored areas of the program); in view of the lack of information on nonworking violators, as mentioned above; in view of the industrial differences between states and the consequent differences in a given state's need for a certain type of disqualification; in view of the interrelationships between disqualification provisions and other unemployment insurance provisions, especially eligibility and duration provisions, and also in view of the different mix of these provisions that the various states have chosen; in view, finally, of the ineffectiveness of the job test as administered by the public employment service ("In practice, it has been found that the public employment service cannot adequately apply the 'work test' " [p. 268])—the authors are surprisingly certain about what the disqualification provisions ought to be. Although my own opinions in this area are fairly close to those of the authors, I believe that the reader, especially the student reader, would be better served by a text that made him see why, in the current state of our knowledge, positions on disqualifications provisions must be largely matters of personal opinion.

In conclusion, it is well to repeat that this is a beautiful book: clear, comprehensive, analytical, balanced. The critical observations above do not amount to more than a gloss on this overall evaluation. The authors and the W.E. Upjohn Institute for Employment Research, which financed the writing of the book, are to be praised for a work that is likely to stand for a long time as a unique, and uniquely valuable, contribution to the field of unemployment insurance.

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NOTES

A nominating committee consisting of Edward S. Mason, chairman, Harold J. Barnett, Marvin Frankel, Ruth P. Mack, Clarence E. Philbrook, and Edward S. Shaw has submitted the following slate of nominees for 1968 officers of the American Economic Association.

President Elect:

William Fellner, Yale University

Vice-Presidents:

Edgar M. Hoover, University of Pittsburgh

Walter W. Heller, University of Minnesota

Robert Solow, Massachusetts Institute of Technology

Harry Johnson, University of Chicago and London School of Economics

Executive Committee:

Lyle C. Fitch, Princeton University

Ilse Mintz, Columbia University

A. D. Scott, University of British Vancouver

Otto Eckstein, Harvard University

The annual meeting of the Association will be held in Washington, D.C., December 28-30, 1967, with headquarters at the Sheraton Park Hotel.

AER MANUSCRIPTS

The following manuscripts, exclusive of comments and replies and in addition to those listed in previous issues, have been accepted for publication in subsequent issues of the *American Economic Review*:

H. J. Bruton, Productivity Growth in Latin America.

W. R. B-yan, Bank Adjustments to Monetary Policy: Alternative Estimates of the Lag.

G. C. Chow, Technological Change and the Demand for Computers.

W. J. Fellner, Measures of Technological Progress in the Light of Recent Growth Theories.

R. J. Gordon, The Incidence of the Corporation Income Tax in Manufacturing, 1925-62.

J. P. Gould, Market Value and the Theory of Investment of the Firm.

P. M. Gutmann, External Financing and the Rate of Economic Growth.

M. J. Hamburger, Interest Rates and the Demand for Consumer Durable Goods.

P. Helmberger and A. C. Johnson, Price Elasticity of Demand as an Element of Market Structure.

J. P. Houck, Price Elasticity and Linear Supply Curves.

E. H. Hunt, Railroad Social Saving in Nineteenth Century Central America.

N. M. Kaplan, Growth of Output and Inputs in Soviet Transport and Communications.

M. E. Kreinin, "Price" vs. "Tariff" Elasticities in International Trade—A Suggested Reconciliation.

J. V. Krutilla, Conservation: in Need of a New Economic Rationale?

Tong Hun Lee, Alternative Interest Rates and the Demand for Money: the Empirical Evidence.

- S. R. Lewis, Jr., Effects of Trade Policy on Domestic Relative Prices: Pakistan, 1951-1964, M. Michaely, A Note on Tariffs and Subsidies.
 G. L. Perry, Wages and the Guideposts.
 A. A. Scitovsky, Changes in the Costs of Treatment of Selected Illnesses, 1951 to 1965.
 R. Sherman, A Private Ownership Bias in Transit Choice.
 H. R. Stoll, A Note on Exchange Rate Changes and Capital Flows Under Fixed and Floating Exchange Rate Systems and Some Implications for Empirical Testing.
 E. A. Thompson, Debt Instruments in Both Macroeconomic Theory and Capital Theory.
 R. H. Wessel, A Note on Economic Rent.
 O. E. Williamson, The Welfare Trade-Offs Between Scale Economies and Market Power.
 C. Wright, Some Evidence on the Interest Elasticity of Consumption.

REPORT ON PROPOSED NATIONAL SOCIAL SCIENCE FOUNDATION

On October 11, 1966, Senator Fred R. Harris, chairman of the Subcommittee on Government Research of the Senate Committee on Government Operations, introduced in the Senate S-836, "A Bill to Create a National Foundation for the Social Sciences." The bill was co-sponsored by twenty of Senator Harris' Senate colleagues. In June 1966, a corresponding bill had been introduced in the House by Representative Dante B. Fascell of Florida, chairman of a Foreign Affairs subcommittee, along with a bill to create an Office of Social Sciences in the Executive Office of the President (parallel to the Office of Science and Technology) and another to authorize the holding of a White House conference on the social and the behavioral sciences. An avowed purpose of this proposed legislation is to augment the support of social sciences which are drawing about \$21 million direct support and perhaps as much as \$29 million indirect support from the National Science Foundation, out of its budget of \$480 million (*Science*, 17 Feb. 1967, p. 814). In fiscal 1966 the total federal contribution to research in social science fields amounted to \$188 million (compared with \$35 million in 1960), together with \$87 million for social psychology (*Science*, 28 Oct. 1966, p. 488.)

Two types of dissatisfaction in the social science community and the university community seem to be generating some support for the proposed NSSF. It is felt that support for social science within the framework of the NSF has not grown fast enough. It is also believed that funds have been too unevenly distributed. The view that in principle heavy governmental support of science is questionable seems to have little support. There is complaint, of course, about the disorganizing impact of the governmental and foundation grants system upon the university structure, interuniversity distribution of personnel, and the conduct of education (*Time*, March 17, 1967, pp. 59-60). Interest is increasing in much greater emphasis upon block-grants to universities instead of grants to scientists and persons gifted in "the fine art of grantsmanship." Such funds are administerable within the structure of the recipient university, and, it is believed, in keeping with the sovereignty of the university. Grants to individuals or groups, on the contrary, tend to reduce university sovereignty, it is believed.

The Board of Trustees responsible for the supervision and direction of the proposed Foundation—24 in number—shall be appointed by the President, with the advice and consent of the Senate, from among Americans "widely recognized for their knowledge of, or experience in, the social sciences or related public affairs." A director and a deputy director will be similarly appointed.

Under Section 6 the Foundation is authorized to—

- (1) Develop and encourage the pursuit of a national policy for the promotion of programs of research and scholarship in the social sciences;
- (2) Initiate and support research and programs to strengthen the research potential of the United States in the social sciences, and to promote such research in foreign countries, by making arrangements (including grants, loans, or other forms of assistance) with individuals or groups, including other government or international agencies for such purposes;
- (3) In making grants or other arrangements for scientific research, to utilize appropri-

ations available therefor in such manner as will, in its discretion, best realize the objectives of (a) strengthening the research staff of such organization, particularly institutions of higher learning in the States, Territories, and Possessions and the District of Columbia; (b) aiding institutions of higher learning or non-profit research organization and foundations, which, if aided, will advance basic research; (c) encouraging social science research by individuals, university faculties, interdisciplinary teams of researchers, non-profit and foundation researchers, and (d) to avoid the concentration of research grants in any one state or geographic region; and

(4) foster the interchange of information in the social sciences by disseminating the results of research projects in such sciences not otherwise published, providing at least annually a listing and description of all projects which are receiving assistance from the Foundation, and maintaining a current register of social scientists."

A parenthetical comment. Clause (d) under (3) probably reflects a sentiment embedded also in President Johnson's directive (*Science*, 24 Sept 1965, pp. 1483-85) calling for a broader distribution of research funds. "We are witnessing," D. S. Greenberg reports (*Science*, 24 June 1966, p. 1726), "the slow but certain demolition of the unique enclave that a segment of the scientific enterprise carved out for itself in the period after World War II."

Under Section 7 the Foundation is instructed to correlate its programs with other ongoing programs and authorized to carry out research for other governmental agencies.

"(a) The Foundation shall correlate programs carried out pursuant to this Act, insofar as practicable, with existing Federal programs and with programs being carried out by other public agencies or private groups and shall develop the programs carried out pursuant to this Act, with due regard to the national interest and the contribution to the purposes of this Act, which can be made by other Federal agencies under existing programs.

(b) At the request of any department or agency of the Government, the Foundation may carry out any project within the purposes and authorization of this Act. Such projects shall be carried out on a reimbursable basis, but the total amount expended on such projects in any fiscal year shall not exceed an amount equal to twenty-five percent of the amount expended by the Foundation in such year for projects pursuant to this Act from funds appropriated to the Foundation.

(c) In the administration of this Act, no department, agency, officer, or employee of the United States shall exercise any direction, supervision, or control over the policy determination, personnel, making of loans and grants, or the administration or implementation of any research project funded by the Foundation, except as may be otherwise provided for herein."

Subsection (b) makes the NSSF an operating as well as a disbursing agency. Whether these two functions may economically coexist within an essentially disbursing agency is considered open to question by some.

Section (8) has to do with international cooperation and coordination with foreign policy. It reflects in part dissatisfaction with Camelot and other social science projects supported by the military. There is need, Senator Harris observed in his speech on February 6, 1967, "for the 'civilianizing' of U. S. social science research abroad." Section (8) reads as follows:

"(a) In carrying out projects pursuant to this Act the Foundation may support international activities, in the social sciences, consistent with the purposes of this act, and the national objectives of the United States.

(b) No project pursuant to this Act shall be carried out in any foreign country (1) until forty-five days after the government of such country has been given notice of such project, and (2) at any time if such government objects to such project.

(c) If, in carrying out the provisions of this Act, any negotiation, other than the notice provided for in subsection (b) of this section, with the government of a foreign country becomes necessary, such negotiations shall be carried out by the Secretary of State at the request of the Foundation."

Section 12 authorizes the use of Public Law 480 funds.

"Sec. 12. Section 104 of the Agricultural Trade Development and Assistance Act of 1954 as amended (7U.S.C. 1704), is amended (1) by striking out the period at the end of sub-

section (t) and inserting in lieu thereof a semicolon, and (2) by adding at the end of such section a new subsection as follows:

(u) for supporting projects and providing assistance, in such amounts as may be specified in appropriation Acts, in accordance with the provisions of the National Foundation for the Social Sciences Act of 1966."

Senator Harris's bill does not make provision for education or training, "areas in which NSF, by wide agreement, has filled an enormously valuable role," D. S. Greenberg reports. "The fact that NSF's money has really been secondary to the stimulation and leadership it has provided for educational activities—ranging from reforms in high school curricula to post-doctoral fellowships—seems to have had no impact on the Harris formulation" (*Science*, 17 Feb. 1967, p. 812).

Some social scientists favor the expansion of NSF support over a new NSSF because: (a) the NSF has made quite full use of the scientific community in administering its program; (b) it has strengthened the ties between the natural and the social sciences; and (c) it has in effect put the high prestige of some of the natural sciences back of the less prestigious social sciences.

Senator Harris's bill seems to attach weight to the practical applications of social science. It is thus spiritually in keeping with H.R. 6698 introduced March 7, 1967, by Representative Emilio Q. Daddario as an amendment to the Act creating the NSF in 1950. This bill is intended "to provide a method for identifying, assessing, publicizing and dealing with implications and effects of applied research and technology." Support for applied social science research is envisaged. Representative Henry S. Reuss, chairman of the House Subcommittee on Research and Technical Programs, is undertaking a study of applied social science research and its capability in coping with domestic problems.

PUBLICATIONS

Cambridge University Press announces the publication of a new quarterly journal, *Modern Asian Studies*. The new journal is devoted to the modern history, human geography, politics, economics, sociology, and literature of the countries lying in an area from West Pakistan to Japan, including China, South and Southeast Asia. In addition to original papers and major review articles, *Modern Asian Studies* publishes reviews of books, particularly those available only in Asian languages. The entire journal is published in English. Subscribers to *Modern Asian Studies* before January 1968 will receive a special charter annual rate of \$8.50 instead of the regular rate of \$9.50. For additional information, write to C. A. Fisher, editor, Cambridge University Press, 32 East 57th Street, New York, N.Y. 10022.

The publishers of the *East African Economic Review* call the attention of U.S. economists to its New Series continuing the publication of articles and book reviews related to the problems of East Africa. The journal is published twice a year, summer and winter, by the Oxford University Press, National and Grindlays Bank Building, Government Road, Nairobi, at a subscription price of E.A. Shs. 25/—.

The *Bentley Business and Economic Review* is now in its fourth year of publication. The editors would welcome a larger flow of manuscripts; they should be sent to Dr. Edward George, editor, Bentley College, Boston, Massachusetts 02115.

The book review editors of *Military Affairs* would like to expand their file of academic and professional reviewers of publications in the field of military, naval, and air history and national security. Write to: Mr. Jack Hilliard, American Military Institute, 1307 New Hampshire Ave., N.W., Washington, D.C. 20036.

The editors of *The Antitrust Bulletin* wish to encourage the submission of more original articles of merit on antitrust economics, foreign and domestic. Send manuscripts to Joseph Taubman, editor, Federal Legal Publications, Inc., 95 Morton St., New York, N.Y. 10014.

Announcements

The Office of Economic Research (OER), a staff organization of the Economic Development Administration, U.S. Department of Commerce, is responsible for research programs to increase the understanding of economic development and improve the implementation of federal development policies. It would welcome proposals in any of the following subject areas: (1) understanding area and regional growth; (2) marshaling and using resources for economic development; (3) forging strategies for economic development; (4) training and educating economic development specialists. Research inquiries and proposals in any of the above categories may be submitted at any time. Each proposal is given individual consideration in terms of current research needs, the adequacy of the research design, and the competence to carry it out successfully. Proposals and inquiries should be submitted to the Director, Office of Economic Research, Staff Office of Assistant Secretary of Commerce for Economic Development, U.S. Department of Commerce, Washington, D.C. 20007.

One of the objectives of the Operations Research Society is to stimulate research of high quality and articles suitable for publication. The Cost-Effectiveness Section is sponsoring its second annual essay contest. This year's topic is *Methodology for Measurement of Effectiveness*. Papers should include an illustration of proposed effectiveness (benefit, utility) measurement methods to a given field, for example, (a) research and development, (b) foreign affairs, or (c) education, urban development or transportation. A premium will be placed on the comprehensiveness, explicitness and potential operational feasibility of the techniques suggested. Applications to various governmental levels are welcome. First and second prizes of \$200 and \$100, respectively, will be awarded. Contest results will be announced in November and prizes presented at the national meeting of the Society.

Researchers, analysts, academicians and practitioners are invited to submit papers. Articles should be typed double-spaced and submitted in four copies to Cost-Effectiveness Essay Contest, c/o Frederick S. Pardee, The RAND Corporation, 1700 Main Street, Santa Monica, California 90406, and must arrive not later than 10 September 1967.

An Economics Society of Michigan was formed at the March 1967 meeting of the Michigan Academy of Arts and Sciences. The officers of the society are: honorary president, Lawrence H. Seltzer, Wayne State University; president, Charles C. Killingsworth, Michigan State University; president-elect, Kenneth E. Boulding, University of Michigan; treasurer, Jared Wend, Western Michigan University; and secretary, John W. Simpson, Eastern Michigan University.

The Econometric Society announces its annual North American summer meeting held jointly with The American Mathematics Society and The Mathematics Association of America at the University of Toronto, Toronto, Ontario, Canada, August 28-30, 1967. Accommodations for the meeting will be available at the University of Toronto and nearby hotels. Further information is given in current issues of the *Notices of the American Mathematics Society*.

The National Institute of Social and Behavioral Science will hold its regular sessions for contributed papers at the 134th annual meeting of the American Association for the Advancement of Science, December 26-31, 1967, in New York. Sessions are held with the collaboration of the Section on Social and Economic Sciences of the A.A.A.S.

Economists interested in presenting a paper at these sessions are invited to forward titles and abstracts of 300 words to Donald P. Ray, National Institute of Social and Behavioral Science, 863 Benjamin Franklin Station, Washington, D.C. 20044, not later than September 1st.

Papers should concern recently completed or well-advanced ongoing research in any of the fields of national or international economics. Of perhaps especial interest would be studies on, for example, European-American trade relations, trends in domestic wage-price levels and

the U.S. balance of payments, professional human resources and American foreign assistance programs, legislation and public economic policy, problems in labor economics, regional and foreign area economic development, and interdisciplinary studies.

Selected materials of the sessions will be published, and papers are eligible, upon nomination, for the \$1000 and bronze medal Newcomb Cleveland Prize of the A.A.A.S. The annual meeting invariably receives extensive coverage by science writers from all media. The American Economic Association is an affiliated member of the Section on Social and Economic Sciences of the A.A.A.S.

Edwin G. Nourse has a number of "remainder" copies of his book *Economics in the Public Service* which he will be glad to present to individuals or libraries that have particular interest in early developments under the Employment Act of 1946, the Council of Economic Advisers, and the Joint Economic Committee. Requests should be sent to the author, 1775 Massachusetts Ave., N.W., Washington, D.C.

Deaths

Amotz Morag, The Eliezer Kaplan School of Economic and Social Sciences, Jerusalem.
John G. Shott, economist, Department of Labor, September 1966.

Retirements

Ben W. Lewis, professor of economics, Oberlin College, June 1967.
Shorey Peterson, professor of economics, University of Michigan, retirement furlough 1967-68.

Visiting Foreign Scholars

Christopher Archibald, University of Essex: visiting professor of economics, Northwestern University, fall 1967.

Horace B. Davis, University of Guyana: visiting professor of economics, Hofstra University.

George France, University of York: visiting assistant professor of economics, University of Connecticut.

Robert G. Gregory, The London School of Economics: visiting assistant professor of economics, Northwestern University, fall 1967.

Alexander M. Kerr, University of Western Australia: visiting professor of economics, University of Texas, fall 1967.

G. D. McColl, University of New South Wales: visiting professor of economics, Duke University, spring 1967.

Omer Sarc, University of Istanbul: visiting professor of economics, Columbia University, 1967-68.

Eugene N. Savin, University of Essex: visiting assistant professor of economics, Northwestern University, fall 1967.

Klaus F. Stegemann: visiting assistant professor of economics, Queen's University, fall 1967.

Promotions

Louis P. Beleký: associate professor of economics, C. W. Post College, Long Island University.

Martin R. Blyn: assistant professor of banking, School of Commerce, New York University.

Barry Bressler: instructor in economics, City College of New York.

Henry J. Bruton: professor of economics, Williams College.

- John F. Chant: associate professor of economics, Queen's University.
 David G. Edens: associate professor of economics, University of Connecticut.
 Walter L. Eisenberg: professor of economics, Hunter College, City University of New York.
 Arnold M. Faden: assistant professor of economics, Iowa State University.
 John Firestone: professor of economics, City College of New York.
 Galvin G. Gall: associate professor of economics, University of Connecticut.
 Henry N. Goldstein: associate professor of economics, Washington State University.
 Michael A. Hall: assistant professor of economics, Beloit College.
 Erich Isaac: associate professor of economics, City College of New York.
 Edward B. Jakubauskas: professor of economics, Iowa State University.
 Clinton E. Jencks: associate professor of economics, San Diego State College.
 Richard Leighton: associate professor of economics, State University of New York, Binghamton.
 Albert M. Levenson: associate professor of economics, Queens College, City University of New York.
 Glenn L. Johnson: associate professor of business administration, University of Kansas.
 Hirschel Kasper: associate professor, Oberlin College.
 Allan V. Palmer: professor of business management, Old Dominion College.
 Robert Piron: assistant professor of economics, Oberlin College.
 Fredric Raines: assistant professor of economics, Washington University, St. Louis.
 Werner Sichel: associate professor of economics, Western Michigan University.
 Matthew Simon: associate professor of economics, Queens College, City University of New York.
 Morris Singer: professor of economics, University of Connecticut.
 Gerald Sirkin: associate professor of economics, City College of New York.
 Babette Solon: associate professor of economics, Queens College, City University of New York.
 Thomas M. Stanback: professor of economics, School of Commerce, New York University.
 Edward Sussna: professor of business administration, Graduate School of Business, University of Pittsburgh.
 H. John Thorkelson: professor of economics, University of Connecticut.
 Paul Weiner: associate professor of economics, University of Connecticut.
 Gordon Winston: associate professor of economics, Williams College.

Administrative Appointments

- Abraham C. Burstein: principal analyst, Human Resources Administration, New York City.
 Fred I. Courtney: chairman, department of business management, Guilford College.
 Paul E. Fenlon: head of department and professor of economics, University of Texas.
 William D. Geer: dean, School of Business, Samford University, Birmingham, Alabama.
 Donald Grunewald, Rutgers: professor and dean, College of Business Administration and Graduate School of Administration, Suffolk University.
 John Hein, Federal Reserve Bank of New York: senior economist, International Economics Department, National Industrial Conference Board.
 Peter B. Kenen: chairman, department of economics, Columbia University.
 Kenyon A. Knopf, Grinnell College: dean and professor of economics, Whitman College.
 Richard L. Kozelka, University of Minnesota: professor and director of graduate studies, College of Business Administration, University of South Florida.

Markos J. Mamalakis, Yale Growth Center: associate professor of economics and director of Latin American Center, University of Wisconsin, Milwaukee.

William J. McKinstry: chairman, department of economics, Miami University.

William M. Morgenroth: professor and head of the division of business and economics, California State College, Hayward.

George R. Morrison, State University of New York, Buffalo: senior economist, money markets section, First National City Bank, New York.

Abdul Hameed Naz: senior regional economist, Maryland-National Capital Park and Planning Commission.

Jack W. Nickson, Jr.: acting chairman, department of economics, Old Dominion College.

W. H. Patterson: acting chairman, department of accounting, Old Dominion College.

Donald K. Pemberton: chairman of department of economics, Hiram College.

Janus Poppe, Industrial College of the Armed Forces: dean, United States Merchant Marine Academy, Kings Point, New York.

Edward J. Powers: chairman, department of economics, Saint Anselm's College.

George Rosen: deputy director of economics and technical assistance division, Asia Development Bank, Manila.

Richard H. Rush: professor and director of finance program, School of Business Administration, American University, fall 1967.

Frank I. Stern, New York Central System: director of marketing information systems, Chicago and Northwestern Railway Company.

Paul P. Streeten: acting director, Institute of Development Studies, University of Sussex.

Eugene L. Swearingen, Oklahoma State University: president, Tulsa University.

Roger C. Van Tassel: chairman, department of economics, Clark University.

Joseph S. Wright, Zenith Radio Corporation: chairman, Council on the Graduate School of Business, University of Chicago.

Appointments

Robert F. Adams, University of Maryland: lecturer, University of California, Santa Cruz, 1967-68.

Charles F. Austin, Personnel Research Division, U.S. Army: associate professor of organizational behavior and business administration, School of Business Administration, American University, fall 1967.

Christopher Bach, Western Reserve University: appointment in economics, Lafayette College.

Bela Balassa: professor of political economy, Johns Hopkins University.

Arnold P. Ballantyne: associate professor of economics, University of Colorado, 1967-68.

Bhal J. Bhatt: assistant professor of industrial relations, State University of New York, Buffalo.

Sanjit Bose: assistant professor of economics, Brandeis University.

Anthony Bottomley, chair in economics, University of Bradford, Yorkshire.

Kenneth E. Boulding: visiting professor of economics, University of Colorado, 1967-68.

Dwight S. Brothers, Rice University: Graduate School of Business Administration, Harvard University.

Thomas Burney: assistant professor of economics, Denison University.

Robert C. Burton: associate professor of economics, Old Dominion College, fall 1967.

Marshall L. Casse, III: assistant professor of economics, School of Business Administration, Emory University, September 1967.

V. Karuppan Chetty, University of Wisconsin: associate professor of economics, Columbia University, fall 1967.

Laurits Christensen: assistant professor of economics, University of Wisconsin, fall 1967.

Peter Clark: assistant professor of economics, Duke University, 1967-68.

Norris C. Clement: visiting assistant professor of economics, University of Colorado, 1967-68.

William R. Cline, Yale University: assistant professor of economics, Princeton University, September 1967.

Raymond Coleman: professor of economics and management, College of Business Administration, University of Illinois, Chicago.

Andre Corbeau, Louisiana State University: assistant professor of economics, University of Oklahoma.

Richard R. Cornwall, University of California, Berkeley: assistant professor of economics, Princeton University, September 1967.

Ronald W. Crowley: assistant professor of economics, Queen's University, fall 1967.

Thomas D. Curtis, University of Arizona: associate professor of economics, University of Oklahoma, September 1967.

Herschel Cutler, Association of American Railroads: associate professor of transportation and business administration, School of Business Administration, The American University, fall 1967.

Ralph d'Arge: assistant professor of economics, University of New Mexico, September 1967.

James V. Davis: assistant professor of business administration, Vanderbilt University.

Joseph S. DeSalvo, Virginia Military Institute: research staff, logistics department, RAND Corporation.

Eugene J. Devine, University of California, Los Angeles: instructor in economics and commerce, Simon Fraser University, fall 1967.

Barend A. de Vries, International Bank for Reconstruction and Development: visiting professor of political economy, Johns Hopkins University.

Stephen Dresch, Yale University: instructor in economics, Miami University.

Jan W. Duggar, Florida State University: assistant professor of economics, Louisiana State University, fall 1967.

Robert Eagley, Wayne State University: associate professor of economics, University of Massachusetts, fall 1967.

Robert B. Ekelund, Jr., Louisiana State University: assistant professor of economics, Texas A and M College, fall 1967.

Frank W. Fetter, Northwestern University: visiting professor of economics, Dartmouth College.

Walter Fisher, professor of economics, Northwestern University, fall 1967.

Myron D. Fottler: assistant professor of industrial relations, State University of New York, Buffalo.

Kenneth D. Frederick: assistant professor of economics, California Institute of Technology.

Martin J. Gannon: assistant professor of industrial relations, State University of New York, Buffalo.

Marshall Geer, III: visiting assistant professor of economics, University of Colorado, 1967-68.

Malcolm Gillis: assistant professor of economics, Duke University, 1967-68.

Micha Gisser: associate professor of economics, University of New Mexico, September 1967.

Michael Gort, State University of New York, Buffalo: visiting professor of economics, Northwestern University, fall 1967.

Julian M. Greene: assistant professor of economics, Southern Methodist University, 1967-68.

Benton E. Gup, University of Cincinnati: economist, Federal Reserve Bank of Cleveland.

Jerry W. Gustafson: assistant professor of economics, Beloit College, fall 1967.

David Hahn: assistant professor of marketing, Ohio State University.

James Halstead: assistant professor of economics, Williams College.

Curtis C. Harris, Jr., Economic Development Administration, Department of Commerce: research associate in Bureau of Business and Economic Research and assistant professor of economics, University of Maryland.

Donald J. Harris, University of Illinois: visiting assistant professor of economics, Northwestern University, fall 1967.

John A. Haslem: assistant professor, School of Business, University of Wisconsin.

F. Gregory Hayden, University of Texas: assistant professor of economics, University of Nebraska, fall 1967.

Patric H. Hendershott: assistant professor of economics, Northwestern University, fall 1967.

Robert Hinckley: assistant professor of economics, University of Massachusetts, fall 1967.

Edward P. Holland: economist, Office of Program Coordination, Agency for International Development.

Robinson Hollister: assistant professor of economics, University of Wisconsin, fall 1967.

Sheng-tieh (Frank) Hsiao: assistant professor of economics, University of Colorado, 1967-68.

David S. Huang, Southern Methodist University: visiting professor of business economics, Graduate School of Business, University of Chicago, 1967-68.

Stanley Huff, Princeton University: assistant professor of economics, Denison University.

Joseph Humphrey: instructor in economics, Occidental College.

Thomas Iwand, Pennsylvania State University: associate professor of economics, University of Nebraska, fall 1967.

Elmo L. Jackson: professor of economics, Old Dominion College, fall 1967.

David B. Johnson, University of Virginia: assistant professor of economics, Louisiana State University, fall 1967.

Stanley R. Johnson: associate professor of economics, University of Missouri.

Paul Jonas, Brooklyn College: associate professor of economics, University of New Mexico.

Lamar B. Jones, Virginia Polytechnic Institute: assistant professor of economics, Louisiana State University.

John H. Keith, Jr.: instructor in economics, Dartmouth College.

James Kindahl, University of Chicago: associate professor of economics, University of Massachusetts, fall 1967.

Paul G. King, University of Illinois: assistant professor of economics, Denison University.

Burton H. Klein: professor of economics, California Institute of Technology.

Anthony Y. C. Koo: professor of economics, Michigan State University, September 1967.

George M. Lady, Johns Hopkins University: assistant professor of economics, Rice University.

Robert J. Lawrence, University of Texas: research staff of Board of Governors, Federal Reserve System.

Larry C. Ledebur, Florida State University: assistant professor of economics, Denison University.

Chung H. Lee, Miami University: lecturer in economics, University of Essex, England.

Tong Hun Lee, University of Tennessee: professor of economics, University of Wisconsin, Milwaukee, September 1967.

- An-Yhi Lin: research associate, department of economics, Iowa State University.
- Thomas Love: assistant professor of economics, University of Massachusetts, fall 1967.
- Ruth Mack: visiting professor of economics, City College of New York.
- Thomas E. Manore: assistant professor of business management, Old Dominion College, fall 1967.
- James A. Maxwell: professor of economics, University of Connecticut.
- Donald C. Mead: associate professor of economics, Amherst College, fall 1967.
- Charles R. Miller: associate professor of business management, Old Dominion College, fall 1967.
- George N. Monsma, Jr.: assistant professor of economics, Amherst College, fall 1967.
- Brian Motley: assistant professor of economics, University of Wisconsin, fall 1967.
- Owen B. Murphy: assistant professor of economics, Albany Junior College, Georgia.
- Wallace M. Nelson: associate professor of economics, Rollins College, Florida.
- William G. Nelson, Youngstown University: purchasing agent, University of Pittsburgh.
- Robert E. Olley, University of Saskatchewan: visiting assistant professor of economics, Queen's University, fall 1967.
- Robert M. Olsen, Northwestern University: assistant professor, School of Business, University of Kansas.
- Larry Orr: assistant professor of economics, University of Wisconsin, fall 1967.
- John D. Patrick, Federal Reserve Bank of New York: instructor in economics, Swarthmore College, fall 1967.
- Mark V. Pauly: assistant professor of economics, Northwestern University, fall 1967.
- Allen Perlman: assistant professor, School of Business, University of Kansas.
- Louis R. Pondy: assistant professor of business administration, Duke University, fall 1967.
- Ibrahim I. Foroy, United Nations Conference on Trade and Development, Geneva: assistant professor of economics, San Diego State College, fall 1967.
- Richard L. Porter, Marquette University: visiting professor of economics, Oklahoma State University.
- Martin F. J. Prachowny: assistant professor of economics, Queen's University, fall 1967.
- Cadwell Ray: assistant professor of economics, University of Massachusetts, fall 1967.
- Stanley Reiter: professor of economics, Northwestern University, fall 1967.
- Idrian N. Resnick: lecturer in economics, Princeton University, September 1967.
- Charles Richey: assistant professor of economics, Hiram College.
- H. Raymond Rodosevich: assistant professor, School of Business, University of Kansas.
- Gerald Rosenthal: associate professor of economics, Brandeis University.
- Philip Ross, University of Pittsburgh: professor of industrial relations, State University of New York, Buffalo, September 1967.
- Bernard Saffran, University of California, Berkeley: associate professor of economics, Swarthmore College, fall 1967.
- Lars G. Sandberg: associate professor of economics, Dartmouth College.
- Dean S. Sanders, University of Michigan: assistant professor of economics, Miami University.
- U. Sankar: assistant professor of economics, University of Wisconsin, Milwaukee, September 1967.
- Bikas C. Sanyal: research associate, department of economics, Iowa State University.
- Paul Schatz: professor of insurance, Samford University, Birmingham, Alabama.
- Robert N. Schoepfle: assistant professor of economics, University of Connecticut.
- Nicholas W. Schrock, University of Oregon: assistant professor of economics, San Diego State College, fall 1967.
- R. G. Scott: assistant professor of economics, University of California, Santa Cruz.

- Charles E. Seagrave: assistant professor of economics, Rice University, September 1967.
- Joel Segall, University of Chicago: visiting professor of finance, Graduate School of Business Administration, University of Washington.
- David O. Sewell: assistant professor of economics, Queen's University, fall 1967.
- Sue O. Shaw, visiting lecturer in economics, Vanderbilt University, 1967-68.
- Dennis E. Smallwood, Yale University: assistant professor of economics, Princeton University, September 1967.
- Eugene Smolensky, University of Chicago: associate professor of economics, University of Wisconsin, fall 1967.
- Ronald Soligo, Yale University: associate professor of economics, Rice University, September 1967.
- Gordon R. Sparks, Massachusetts Institute of Technology: associate professor of economics, Queen's University, fall 1967.
- Zane Spindler: instructor in economics and commerce, Simon Fraser University, fall 1967.
- J. Kirker Stephens, University of Illinois: assistant professor of economics, University of Oklahoma, September 1967.
- Dale E. Swan: assistant professor of economics, Southern Methodist University, 1967-68.
- Neil M. Swan: assistant professor of economics, Queen's University, fall 1967.
- Robert L. Swinth, Carnegie Institute of Technology: associate professor, School of Business, University of Kansas.
- Ralph L. Thomas: professor of economics, California State College, California, Pennsylvania.
- Samuel L. Thorndike, Jr., Columbia University: assistant professor of economics, University of Wisconsin, Milwaukee, September 1967.
- Adrian W. Throop: assistant professor of economics, Dartmouth College.
- John O. Tollefson: assistant professor, School of Business, University of Kansas.
- Jack Topiol: appointment in economics, Lafayette College.
- Vladimir Trembl: associate professor of economics, Duke University, 1967-68.
- Paul B. Trescott, The Rockefeller Foundation: professor of economics, Miami University.
- Sheila Tschinkel: assistant professor of economics, University of Connecticut.
- Hiroki Tsurumi: assistant professor of economics, Queen's University, fall 1967.
- Gerald F. Vaughn, Maryland State Planning Department: coordinator, Community and Resource Development, University of Delaware.
- John P. Venieris, University of Oregon: assistant professor of economics, San Diego State College.
- Wayne Vroman: assistant professor of economics, Oberlin College, September 1967.
- Frederick S. Weaver: assistant professor of economics, University of California, Santa Cruz.
- H. Martin Weingartner: professor of business administration, University of Rochester.
- Stephen W. Welch: assistant professor of economics, University of Connecticut.
- Robert Welsh, University of Kentucky: associate professor of marketing, Ohio State University Contract Team in Brazil.
- Jerome D. Wiest, University of California: associate professor of management science, Rice University.
- Frederick D. Whitehurst: assistant professor of accounting, Old Dominion College, fall 1967.
- William E. Whitelaw: assistant professor of economics, University of Oregon.
- Harold O. Wilson: visiting assistant professor of business administration, Vanderbilt University.
- Robert W. Wisner: assistant professor of economics, Iowa State University.
- James E. Zinser: assistant professor of economics, Oberlin College, September 1967.

Leaves for Special Appointments

Werner Baer, Vanderbilt University: Vanderbilt-overseas assignment, University of São Paulo, Brazil, 1967-68.

Jack N. Behrman, University of North Carolina: visiting professor, Harvard Business School.

Harry G. Brainard, Michigan State University: advisor to the Turkish Academy of Economic & Commercial Sciences, Ankara, Turkey, July 1967-July 1969.

Thomas F. Dernburg, Oberlin College: Council of Economic Advisers, September 1967.

Stanley L. Friedlander, City College of New York: President's Council of Economic Advisers, Washington, D.C.

Bela Gold, Case Institute of Technology: visiting professor, The Imperial College of Science and Technology, London.

Douglas H. Graham, Vanderbilt University: Vanderbilt-overseas assignment, Vargas Foundation, Rio de Janeiro, Brazil, fall 1967.

C. Elton Hinshaw, Vanderbilt University: Vanderbilt-overseas assignment, University of São Paulo, Brazil, 1967-68.

John M. Hunter, Michigan State University: Midwest Universities Consortium International Activities Inc.—Brazil Higher Education Project, February 1967-September 1968.

Herbert Kisch, Michigan State University: visiting professor, University of Berlin, 1967-68.

Gian S. Sahota, Vanderbilt University: Vanderbilt-overseas assignment, University of São Paulo, Brazil, 1967-68.

John R. Shea, University of Santa Clara: Ohio State University Human Resource Development Project, USAID, La Paz, Bolivia.

Anthony M. Tang, Vanderbilt University: Vanderbilt-overseas assignment, Chinese University of Hong Kong, fall 1967.

Clems B. Thoman, University of Nebraska: visiting professor of economics, Pahlavi University, Shīvaz, Iran.

Richard S. Thorn, University of Pittsburgh: advisor to the director, International Monetary Fund Bureau of the Budget, Teheran, Iran.

William O. Thweatt, Vanderbilt University: Vanderbilt-overseas assignment, University of São Paulo, Brazil, 1967-68.

Arthur W. Wright, Oberlin College: Economic Growth Center, Yale University.

Resignations

Raul M. Branco, University of Texas, September 1967.

T. M. Brown, Queen's University, 1967.

Brian V. Hindley, Queen's University, 1967.

Wallace L. Nelson, Jr., University of Wyoming.

Ray C. Roberts, Jr., Old Dominion College.

Raymond S. Strangways, Old Dominion College.

Gerald I. Weber, University of Illinois.

Miscellaneous

Edward Ames, professor of economics, Purdue University: named Loeb Distinguished Professor of Economics.

James I. Stewart: appointed a Queen's Counsel on the New Year's Honours List, Toronto, Ontario.

Aaron W. Warner, professor of economics, Columbia University: named to Chair, Joseph L. Bottenwieser Professor of Human Relations.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois, 60201.

Vacancies

Labor economists and/or statisticians: The measurement of the economic damages suffered by those who are wrongly injured or killed is a new area for the application of economic and statistical knowledge and research techniques. This field has been pioneered successfully and Associated Appraisers of Impaired Earning Capacity is now providing this service to attorneys on a nationwide basis. Those in it appraise the losses and are required to testify as an expert in court trials and must withstand hostile cross-examination. It is one of the highest-paid forms of work in the profession and it is possible to engage in it as a part-time activity. Requirements: (1) minimum status of associate professor; (2) experience in research in collective bargaining agreements and fringe benefit programs; (3) location in or near a major metropolitan area. Openings now available in following areas: Boston, Philadelphia, Pittsburgh, Chicago, Minneapolis, St. Louis, New Orleans, Kansas City, Oklahoma City. Waiting list open in other areas. Send résumé and list of publications to: Philip Eden, President, Associated Appraisers of Impaired Earning Capacity, 1303 Walnut Street, Berkeley, California, 94709.

Economist: Ph.D. preferred, doctoral candidates and those with master's degree will be considered. Principles of economics, labor economics, public finance, history of economic thought, macroeconomics. Rank of instructor or assistant professor, depending on advanced degree and teaching experience. Salary range, \$7,500-\$10,000. Liberal fringe benefits. Please send résumé and names and addresses of two references to: Chairman, Department of Economics and Business Administration, Lebanon Valley College, Annville, Pennsylvania, 17003.

Economists, mathematicians, statisticians, operations analysts: Needed by the Research Analysis Corporation to participate in defense-related studies of resource allocation techniques and applications. Areas of interest include systems analysis of defense forces, weapon systems, and manpower and material resources, and its constituent elements of cost-effectiveness analysis and military cost analysis. A strong capability is maintained in computer-assisted model building. Staff members are assisted in acquiring a working knowledge of computer techniques if they do not already have it. Candidates should have at least a master's degree. For additional information, send résumé to: Mr. John G. Burke, Supervisor of Professional Staffing, Research Analysis Corporation, McLean, Virginia, 22101.

Economist: September, 1967. Principles and advanced course to be selected. Twelve-hour load. Assistant or associate. Ph.D. required. Small college of about 2,000 enrollment where emphasis is placed on teaching. Write to: Dick Smith, Head, Department of Social Science, Tarleton State College, Stephenville, Texas, 76402.

Economists and econometricians: We seek several outstanding men or women for the professional staff of a management consulting firm whose principal clients are blue chip industrial companies. Graduate training in economics at a leading university and several years of experience in business, consulting, or teaching required. New York City location. Ten percent travel. Salary open and depends on qualifications and previous achievements. P335

Teaching opportunities abroad: University positions, instructor to professor level, in the developing nations of Asia, Latin America, and primarily Africa. Two-year contracts. Salary dependent on level and location; transportation provided. Write to: Overseas Educational Service, 522 Fifth Avenue, New York, New York, 10036. An Equal Opportunity Employer.

Accountants: With continued growth and expanding academic programs, the Accounting Department, Texas A. and M. University has openings for two additional staff members. One, an outstanding tax and auditing specialist with a capability in computers as related to accounting in general and auditing in particular. Applicants should possess terminal degree and preferably be in mid-career and interested in a balanced program of teaching, research, and publications. Salary to \$16,500 for nine months with opportunity for additional summer term teaching at a comparable rate. Second, an assistant professor interested in principles and theory, cost, and computer applications in accounting. Applicant should possess terminal degree. Salary to \$10,750 for nine months with opportunity for additional summer term teaching. Applicants for both positions may send credentials to: Dr. Jack W. Coleman, Head, Accounting Department, School of Business, Texas A. and M. University, College Station, Texas, 77843.

Finance: With continued growth and expanding programs, the Finance Department has an opening for an additional staff member. This is for an assistant professor with salary to \$10,750 for nine months with opportunity for additional summer term teaching. Requirements call for a terminal degree. Appointee should be interested in specialization in the institutional aspects of finance. Applicants for this position may forward their credentials to: Dr. John E. Pearson, Director, School of Business, Texas A. and M. University, College Station, Texas, 77843.

Transportation economist or transportation specialist: U.S. Army Corps of Engineers, Civil Works Directorate, requires several transportation economics or transportation specialists to fill staff positions in division and district offices in various parts of the country. Positions are from grade level GS-5 (\$5,331) to GS-13 (\$12,873) and are a part of the federal Civil Service program. Basic requirements are a degree in economics or business administration and appropriate experience or training for performing the following duties: supervises or produces economic evaluation studies of proposed waterway or harbor improvements; structures potential area of influence of proposed improvement, determines movements of commodities in area of influence, and assesses economic efficiencies of moving these commodities by various carrier modes—water, rail, road, and pipeline. Should be familiar with cost and rate structures of these carrier modes as well as evolving technologic changes affecting their interrelationships in an overall transportation system for a region and for the entire nation. Applicants should apply to: Mr. N. A. Back, Chief, Economics and Evaluation Branch, Civil Works Directorate, Office, Chief of Engineers, Washington, D.C., 20315.

Economists: Many developing countries turn to the United Nations and its specialized agencies for help and advice in dealing with their problems. Consequently, the United Nations needs the services of highly qualified economists in almost every field of specialization for its technical assistance and pre-investment programs. These overseas assignments in the developing countries vary from a few months to several years, although most of them are for "one year with possibility of extension." Positions with the U.N. Development Program offer economists with graduate degrees and 10 or more years of practical experience an unusual opportunity to participate in the stimulating programs of economic development which characterize a large number of emerging countries. Such assignments often can be fitted into the university professor's sabbatical year; for longer assignments universities have been willing to grant leaves of absence. This program provides a unique opportunity for university economists to fill interesting and worthwhile jobs which will benefit the developing countries and contribute to their academic careers. The United Nations

also seeks economists and econometricians for research and operational posts at its headquarters and Regional Economic Commissions. These posts require candidates for an M.A. or Ph.D. in economics or econometrics and several years of experience which has involved substantial economic research. Most appointments are for a minimum of two years, although a limited number are for shorter periods and can be worked into sabbatical years. U.S. citizens should send résumés to: Director, Office of International Organization Recruitment, Department of State, Washington, D.C., 20520. Non-U.S. citizens should apply directly to: Office of Personnel, United Nations, New York, New York, 10017.

Economists: Office of International Operations, Internal Revenue Service, has unique and intellectually challenging career opportunities for economists in Washington, D.C. Duties involve the application of economic analysis to intercorporate affairs between related corporations to determine whether they reflect market or arm's length standards. Function is to provide analysis and recommendations on the "reasonableness" of intercorporate prices, and whether or not the corporations involved have failed to clearly reflect their true taxable income. These studies cover the entire spectrum of business activity and will have an important bearing on the development of tax law guidelines and precedents involving intercorporate pricing in the expanding field of international commerce. Positions are available at entrance salaries ranging from \$10,000 to \$15,000. Position and starting salary will depend upon educational background and professional experience. For further details, write to: Director of International Operations, Attention of Chief, Economic Advisory Branch, Internal Revenue Service, Washington, D.C., 20225.

Fishery economists: Wide variety of economic research ranging from international agreements, quotas, and tariffs to price analyses, business management of firms, cost-benefit analyses, and whole field of the economics of natural resources. Positions are in the federal Civil Service at Grades GS-9 (\$7,696) to GS-14 (\$15,106). Basic requirements are Ph.D. or master's in economics or agricultural economics; training in international, natural resource, and/or quantitative economics would be helpful. Civil Service Commission Form 57 (application for federal employment) should be sent to: Assistant Director for Economics, Bureau of Commercial Fisheries, U.S. Department of the Interior, 1815 N. Fort Myer Dr., Arlington, Va., 22209.

Economics: Head of department, Ph.D., several years of teaching experience at college or university level. Present eight-man department. Catholic-related, coeducational midwestern university; 2,500 students (500 graduate) in School of Business. T.I.A.A., life insurance, hospitalization, other fringe benefits. Salary commensurate with education and experience. Apply to: Professor William J. Hoben, Dean of Business Administration, University of Dayton, Dayton, Ohio, 45409.

Marketing: Position of lecturer in undergraduate business management program. Combination of marketing and marketing research, with additional field in managerial economics or quantitative methods. Advanced degree in business administration (Ph.D., M.A., or M.B.A.) with some additional experience. Ability to use Chinese language highly desirable but not required. Unusual opportunity in rapidly developing academic institution. Minimum term of appointment two years. Salary and details flexible. For information, write: Registrar, New Asia College, Chinese University of Hong Kong, 6 Farm Road, Kowloon, Hong Kong.

Economics: Head of department; Ph.D. Rank (full, associate, or assistant professor) and salary appropriate to education and experience. Load, 8 to 12 hours. Most courses on alternate-year basis. Teaching assignment flexible, presently includes principles, economic thought, comparative economic systems, public finance, labor economics and collective bargaining, national income analysis, general economic history of Europe and America. Will also consider person approaching or having recently reached emeritus status, in which case would implement rotating headship; emeritus would qualify as visiting professor. Commence September, 1967. Write to: C. L. Kanatzar, Dean of Faculty, MacMurray College, Jacksonville, Illinois, 62650.

Economics, labor: Ph.D. to teach economics and labor courses at accredited undergraduate college on Long Island in the New York City area. Appointment effective August, 1967, in rank of associate professor. Starting salary \$12,959. Additional fringe benefits. Send complete résumé to: Captain Lawrence Jarrett, Head, Department of Maritime Law and Economics, U.S. Merchant Marine Academy, Kings Point, New York, 11024.

Accounting, finance, management: One position available at a four-year, liberal arts college. Rank and salary open. Opportunity to become head of department and/or occupy chair of business administration (doctorate required for chair). Applicants with master's and no experience will be considered at the rank of instructor. Write to: Dr. Dale J. Shaw, Academic Dean, Jamestown College, Jamestown, North Dakota, 58401.

Transportation economist: The national Economics Division of the Office of Business Economics has an opening for a transportation economist to work in the construction of input-output tables. Grade to GS-13 is available, depending on training and experience. Applications (Application for Federal Employment, Standard Form 57, obtainable at all U.S. Post Offices) should be addressed to: Mr. Charles Pierce, Office of Personnel, Room 5230 Main Commerce Building, Washington, D.C., 20230. An Equal Opportunity Employer.

Labor economist: Teaching position in an expanding economics department. Salary, \$9,500 to \$12,000 for academic year. Ph.D. or near Ph.D. required. For further information, write and send résumé to: Phillip A. May, Department of Economics, School of Arts and Sciences, Northern Michigan University, Marquette, Michigan, 49855.

Economic statistician: Under direction of chief, Research and Analysis Division, conducts research and prepares analyses of economic conditions and trends and statistical indicators relevant to the planning and development programs of the state. Requires master's degree in economics or economic statistics and four years of progressively responsible experience in economic research involving collecting, manipulating, and analyzing economic data. Salary range, \$9,840-\$12,552 per year. File applications with: State of Hawaii, Department of Planning and Economic Development, 426 Queen Street, Honolulu, Hawaii, 96813.

Economist I: Under direction of chief, Research and Analysis Division, conducts research and prepares analyses and reports on macro- and microeconomic conditions relevant to the planning and development programs of the state. Requires four years of responsible professional experience involving work in economics and a master's degree in economics or business administration. Specialization in regional economics, econometrics, or statistics helpful. Salary range, \$9,840-\$12,552 per year. File applications with: State of Hawaii, Department of Planning and Economic Development, 426 Queen Street, Honolulu, Hawaii, 96813.

Economics: Assistant dean, College of Business. Ph.D. or D.B.A. in business or economics required. Three to 5 years of full-time teaching experience in college or university. Administrative experience given special consideration. Limited teaching opportunity. Initial annual salary \$17,000-\$18,000. Effective date September 1, 1967. Contact: Deal Earl A. Roth, College of Business, Eastern Michigan University, Ypsilanti, Michigan, 48197.

Economics, business: Rapidly expanding new university, favorably located in the Vancouver metropolitan area, aiming at excellence in undergraduate teaching and strong graduate program, invites applications at all rank levels. Doctorate or near-doctorate required. Additional business administration specialists sought for the current academic year in accounting, finance, production, organization, quantitative methods, and international business. Additional economics specialists sought for 1968 in economic history, educational economics, comparative systems, and regional economics. Write to: Dr. Parzival Copes, Head, Department of Economics and Commerce, Simon Fraser University, Burnaby 2, British Columbia, Canada.

Economist: Ph.D. completed by September, 1967. Appointment at any professional rank, depending upon qualifications. Salary level \$11,000-\$15,000 plus. Immediate teaching assignment in principles and international economics or money and banking. Interests in economics of technology and quantitative analysis appropriate. Willing to consider visiting professor appointment for senior man. Write to: Dascomb R. Forbush, Chairman, Department of Economics, Clarkson College of Technology, Potsdam, New York, 13676.

Economists: The state of Virginia has recently established a Division of Planning with broad research responsibilities. Opportunities are open for original work in state and local finances, regional economics, and operations research. The work is particularly challenging for young men wishing to gain research experience in an organization with few bureaucratic restraints and good facilities and financing. Posi-

tions are open for persons with training in economics at the M.A., near-Ph.D., or Ph.D. level. Salary range fairly close to federal GS-9 through GS-12. For additional information, send résumé to: Chief, Research Section, Division of Planning, 805 State Office Building, Richmond, Virginia, 23219.

Economic statistician: Expanding research and statistics unit of a midwestern state government has an opening for a fiscal analyst, working primarily in forecasting state revenues. He will apply theory to seek relations of flows of state revenue with other variables and use statistical techniques to test and quantify these relations. Ample computer capacity and programming assistance is available. Salary begins at about \$9,600 with annual increases. P337

Economists Available for Positions

(Italics indicate fields of specialization)

International economic development, international trade, finance, marketing: Man, 42, married; lacking thesis for M.A. Bilingual; over 8 years of experience in economics and marketing research and development. Desires international trade development opportunity. E1543

Money and banking, macroeconomics, principles, public finance, land economics, economic history, comparative systems, Soviet economy: Man, 39, married, Canadian; Honors B.A. and M.A. in economics, Ph.D. thesis well under way. Fluent in German. Fifteen years of business experience (accountant) and 3 years of university teaching experience in economics. Seeks research and/or teaching position. Will consider other positions. West Coast of U.S. or Canada. Available in 1967. E1545

Operations research, econometrics, statistics, mathematical economics: Man, 48; B.S. in S.S., Ph.D. course work virtually completed. Desires research position. Willing to relocate. Available immediately. E1548

Economics, business: Man, 56, married; Ph.D. Twenty years of teaching experience, 9 as department head; business experience. Various publications. Desires teaching, research, administration, or some combination. Available immediately. E1550

Marketing, management, communications, data processing, international business, government and business, principles, thought, agricultural economics, development, systems, public finance, consumer economics: Man, 36, married; M.B.A. (marketing), M.A. (economics), U.S.C. Extensive price and cost analysis work, systems and procedures, and procurement exposure in industry. Presently teaching in community college in California. Seeking professional advancement in four-year liberal arts college. Will relocate to any area. Salary and position open. E1561

Business administration, management: Man, 44; Ph.D. Professor; experienced administrator and teacher; publications. Available June or September, 1968. E1579

Principles, theory, American economic history, history of economic thought, money and banking, business economics: Man, 39, married; Ph.D. Twelve years of teaching experience; also experience as department chairman. Desires position in teaching, research, or business. Available in September, 1967. E1594

Micro- and macro theory, growth and development, international economics, public finance, history of economic thought, statistics: Man, 46, married; M.A., Ph.D. Associate professor; modest publications; over 12 years of university and college teaching. Available in fall, 1967. E1600

Statistics, mathematical economics, economic theory, international economics, comparative systems, economic development and planning, economic growth: Man, early 30's; M.A., M.B.A., all Ph.D. requirements except thesis completed. Six years of teaching experience. Desires teaching position in or close to New York City. E1603

Business management, managerial economics, government and business, labor relations, personnel management: Man, 52, married; B.S. (business administration) with highest distinction, M.A. (economics), Phi Beta Kappa, Beta Gamma Sigma. Twenty-five years of experience in all phases of general management in heavy consumer and

defense industries; extensive operating and consulting experience with U.S. government in economic stabilization and defense matters; university staff and visiting lecturer in economics, labor relations, and government-business relationships. Seeks university or other administrative or teaching position. Available on reasonable notice.

E1606

Economic development, industrial organization, economic thought, labor, macroeconomics: Man, 51, married; Ph.D. Now in top level post with leading research foundation. Twelve years of college teaching experience; 10 years of U.S. government and overseas consulting. Nine books; over forty articles and reports. Desires permanent professorship at good university, preferably one with a solid graduate program. Available in fall, 1967.

E1610

International finance, public finance and fiscal policy, national income accounting, economic development, economics of welfare, regional economics: Man, 38, married; M.A., additional work towards Ph.D. Fellowships; languages; 9 years of progressive research experience; publications. Seeks research position. Willing to relocate.

E1625

Micro- and macroeconomics, international economics, money and banking, labor economics, agricultural economics, public finance: Man, 47, married; M.A., Ph.D. dissertation well under way, degree anticipated in fall, 1967. Desires teaching and/or research position. Available in September, 1967.

E1627

Statistics, macro and micro economic theory, business economics and public policy, quantitative methods, marketing research, economic development, regional planning: Man, 45, married; Ph.D. Research and teaching experience. Desires teaching or research position.

E1633

Statistics, microeconomics, macroeconomics: Man, 28, married; B.B.A., M.A. in Economics. Three years of experience in analytical and research work, especially in the application of advanced technology to various areas of business. Also worked extensively with trade associations in the development of statistical and marketing programs. One year of teaching experience. Desires a highly responsible and challenging position as a research analyst or a research specialist with either an industrial concern or a consulting group.

E1634

Economic and business statistics, econometrics, economic theory, economic development, transportation: Man, 43, married; Ph.D. Adequate publications; 14 years of full- and part-time teaching and research experience; 2 years of full-time research abroad and 2 years of administrative experience. Seeks teaching and/or research position. Available in September, 1967.

E1637

History of economic thought, development of industrial civilization, business and society, micro economic theory, principles, interdisciplinary social science: Man, 30; B.A., M.A. in economics, Ph.D. in general education (social science) with emphasis in economics to be completed by August, 1967. Five years of teaching experience. At present assistant professor at midwestern university. Desires teaching position. Available in September, 1967.

E1641

International economics, monetary economics: Man, 46; M.B.A., Ph.D. Professor at large eastern university; author with expertise and knowledge of languages. Seeks interesting teaching position close to research libraries or international-financial assignment. Available in September, 1967.

E1643

Mineral economics, industrial organization, microeconomic theory, economic development: Man, 27, married; B.S.(mng.eng.), M.S.(mng.eng.), working toward a Ph.D. in mineral economics at Penn State, requirements completed except defense of dissertation. About 3 years of industrial experience; more than 3 years of research experience. Seeks teaching-research position. Available in July, 1967.

E1646

Economic principles, economic development, monetary theory: Man, 27; M.A. from an Indian university, A.M. (economics), University of Chicago. Research assistant, Department of Political Science, Community and Family Study Center, Department of Sociology, Department of Psychiatry, University of Chicago. Seeks teaching or research position either in Canada or the United States. Available immediately.

E1647

Money and banking, international finance, economic analysis, economic policy: Man, 44, married; Ph.D. Five years of university teaching experience; 15 years of economic research in business; 2 years as government economist; modest publication record, including two books. Seeks stimulating position in teaching or research. Available at end of 1967. E1649

Industrial organization, economic growth, planning advanced theory: Man, 42; Ph.D. Presently professor and chairman of department of economics in Indian university, language of instruction English; with a maximum of two years leave. Fourteen years of teaching and research; number of publications in pertinent fields, including two books on industrial organization in India. Desires teaching position in United States or Canadian college or university for one or two years; purpose is the acquiring of experience abroad, not the improvement of income. Leave will start with beginning date of position. Family will not accompany applicant. Available at any time after March, 1967. E1652

Public finance, money and banking, principles, history of economic thought: Man, 29, married; B.S.(accounting), M.B.A., Ph.D.(economics) expected in June, 1967. Research fellowship; several teaching fellowships; 3 years of university teaching experience as instructor in economics, undergraduate. Desires position either in teaching, administration, research, or some combination. Prefers New England but would consider other locations. Available in September, 1967. E1654

Principles, labor, consumer comparative economics, history of economic thought: Woman, retired after 20 years of college teaching in one institution; German Dr. rer. pol., M.P.A. (U.S.). Seeks position with college in East on a year-to-year basis. Available in September, 1967. E1657

Economic theory, economic history, labor economics, money and banking: Man, 32, Indian; M.A., working towards Ph.D. at Calcutta University. Trained in labor welfare; 7 years of graduate and undergraduate teaching experience; several publications. Desires teaching or research position. Available in September, 1967. E1658

Managerial economics, corporation finance, economic and financial analysis: Man, 33; B.S., Columbia, M.B.A., Ph.D. candidate. President of textile manufacturing company, former New York banker; pilot; several languages; experience in research for publication; teaching experience as army weapons instructor and platoon leader; tutor in economics. Desires teaching position in New York City area, part time or full time, undergraduate business and economics. Available on reasonable notice. E1659

Theory, international trade, economic development, money and banking, survey methods: Man, 26, married; two M.A.'s from top American institutions, Ph.D. candidate, fellowships. Latin-American, fluent English, 4 other languages. Three years of teaching and research experience; worked for international organizations. Seeks teaching or research position, whether academic or business, anywhere in the world. Available May-June, 1968. E1660

Operation analysis, statistics, market research and quantitative analysis of reinsurance operations of insurance companies and agencies: Man, 36, married; B.A. plus some graduate work. Seven years of experience in above fields. Seeks position in financial-business company or research organization offering greater opportunity and a more intellectually challenging job than now available with the present employer. Prefers Northeast or Far West location. E1661

Economic principles, comparative economic systems, economic development, labor economics, monopoly and competition: Man, 24, married; B.A., M.A. Two years of junior college teaching experience. Desires dynamic teaching situation in junior or small four-year college with opportunity to work toward Ph.D. at nearby university. Available in June or September, 1967. E1662

Labor economics, social security, industrial and labor relations, comparative economic systems (Latin America and Soviet Union): Man, 33, married; Ph.D. Over 30 publications. Considerable full-time and part-time teaching experience in U.S., Europe, and Latin America; 4 years of full-time research experience in an American university. Field research experience in Latin America and Spain. Fluent in Spanish; fair in Russian. Reads French and Italian. Seeks teaching and/or research position. Available in September, 1967. E1663

Micro- and macroeconomics, money and banking, fiscal policy, development, history of thought: Man, 25, married; B.S., M.A. (economics), plus 1 year of course work toward Ph.D. One year of teaching experience. Desires teaching position at college in South or Southwest. Available in fall, 1967. E1664

Principles, history of economic thought, macro- and microeconomics: Man, 29, married; B.A., M.A. expected in August, 1967. Seven years of teaching experience (secondary level). Desires teaching position in junior college or small liberal arts college which emphasizes good teaching, preferably in Midwest. Salary and position open. Available immediately. E1665

Industrial relations and personnel management, economic planning, economic history: Man, 32, married; M.A. Assistant director of compensation service; 7 years of federal civil service; 3 years of teaching experience. Many articles in foreign labor field; fluency in Italian and other Romance languages; wide contacts, including business executives, labor representatives, and federal officials. Desires writing or college teaching position. E1666

Economic policy, agricultural policy, economic development, agricultural economics, administration: Man, 53, married; U.S. citizen by birth; Ph.D. from major U.S. university. Eighteen years of undergraduate and graduate level teaching and research; 17 years as professor; 7 years as head of large department, plus other university administrative experience. Three years as head of a U.S. government agency with overseas posts and programs with relationships with international organizations. Also private consulting and military command, staff, and teaching experience. Able public speaker and writer. Currently employed on successful private project overseas with U.S. university affiliations. Prefers challenging administrative post, professorship, or consulting assignment. Willing and able to pioneer. Available in late 1967 or early 1968. E1667

Macro and micro theory, economic development, comparative economics: Man, 42; Ph.D. Nine years of university teaching experience; 10 years as an administrator in business; foreign experience (Europe and Latin America); trilingual. Substantial number of publications, including two books. Seeks position in teaching-research or in government. Available in September, 1967, or September, 1968. E1668

Land and area development: Man; A.B., M.S., all college and professional honors. Nationally recognized; 35 years of high-level professional experience. Independent consulting economist in Washington, D.C., with own office for 25 years. Over 300 studies made in most economic fields, both in U.S. and abroad. Knowledge of Spanish, Italian; some knowledge of French and German. Desires additional assignments on contract, per diem, or retainer basis. Résumé furnished on request. E1669

Labor economics, international labor, international economics and finance, comparative economic systems, law, international relations: Man, 37, married; B.S.F.S. (Diplomatic Corps), LL.B., M.A. (international economics-development). Over 5 years of experience with major labor union representing it in their political action program; 7 years with nonprofit and profit organizations in economic research and analysis. Desires challenging position in legislative area (D.C.) or overseas in development or labor. E1670

Economic institutions, economic principles, microeconomics, economic history of United States, economic development, comparative systems: Man, 31, Negro, married; B.A., M.A. from U.C.L.A., additional course work towards Ph.D. Good liberal arts background. Desires a teaching position beginning summer or fall, 1967. Will relocate. E1671

Economic principles, international trade and development, business administration: Man, 44, married; European A.B., M.A. Unusual experience overseas; dynamic and resourceful; fluent in several European and oriental languages. Seeks teaching or research position. E1672

International economics, principles of economics, history of economic thought, public finance, money and banking, economics of transport and public utilities, labor economics, Soviet economics, Indian and American economic development, commercial geography: Man, 26; M.A. (economics) Distinction, LL.B. First-Class, both from Nalpur University, plus two semesters of graduate work in economics at Institute of

International Studies, Geneva. Indian, prospective immigrant to the United States; preference petition approved by U.S. Department of Justice, labor certification issued. One year of teaching experience; 3 years in Reserve Bank of India. Seeks suitable post, teaching, research, business or other in U.S. or Canada. Available at short notice. P. L. Rao, C-4, 132 rue de Lausanne, Geneva, Switzerland.

International economics and business: Man, 38, married; M.A. (business administration), Ph.D. (economics). Multilingual; substantial international business and research experience; associate professor of international business and marketing at reputable university, Washington, D.C. Available for business or lecturing engagements during forthcoming summer months (June-September) and part-time assignments during the semester. E1673

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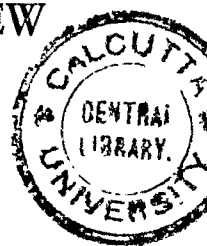
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THE INCIDENCE OF THE CORPORATION INCOME TAX IN U.S. MANUFACTURING, 1925-62

By ROBERT J. GORDON*

The best-known empirical study of the shifting of the corporation income tax is that of Krzyzaniak and Musgrave [18] (K-M for short), who conclude that in the short run in U.S. manufacturing more than 100 per cent of the burden of the corporation income tax is shifted forward to consumers. The K-M method is to measure the effect of alternative independent variables, including tax liabilities, on the pre-tax rate of return in a time series regression covering the years 1935-42 and 1948-59. A positive coefficient on the tax variable would be evidence that, in response to an increase in the corporation income tax rate, corporations raise prices and increase their pre-tax profits, thus shifting to consumers some of the burden of the tax. An insignificant coefficient on the tax variable would indicate that corporations are unable to achieve this discretionary improvement in their pre-tax profits and thus suffer the full burden of the tax.

In the K-M study the coefficient on the tax variable is positive and significantly greater than 100 per cent, implying that corporations respond to an increase in tax liabilities by raising pre-tax profits by *more* than the tax liability. Following the imposition of a tax of 50 per cent, for instance, the pre-tax rate of return would more than double. If K-M are correct, the following is true:

1. The corporation income tax does not reduce the rate of return and thus does not reduce investment (to the extent that the rate of return is an important determinant of the demand for investment).
2. Since the corporation income tax does not reduce dividend payments, there is no *de facto* double taxation of dividends.
3. The corporation income tax is not progressive and has the regressive effect of a general sales tax on the goods and services produced by corporations; the relative tax burden on each product depends on the

* The author is a Ph.D. candidate at the Massachusetts Institute of Technology. He wishes to thank E. C. Brown, F. M. Fisher, R. E. Hall, D. M. Holland, Edwin Kuh, J. G. Gurley, and M. H. David for their helpful suggestions.

profitability, the use of equity capital, and vertical integration among the firms in the industry.

4. The effect of a value added tax on the average level of prices and profits is the same as that of the fully shifted corporation income tax, since both are equivalent to a tax on sales. Thus the substitution of the value added tax for the corporation income tax would not change profits or domestic prices, although it would permit the reduction of export prices.

5. Investigators like J. W. Kendrick and R. Sato [16] cannot use the income share of capital in long-run production functions to compute the elasticity of output to changes in capital input, because shifting raises pre-tax rates of return above the marginal product of capital.

6. An increase in corporation income tax rates is a perverse tool for short-run stabilization policy in inflationary periods, since corporations react immediately by raising prices and thus work against the attempt of policymakers to stabilize prices.¹

7. The major reason for lower corporate rates of return in the 1930s relative to the 1950s was the lower corporation tax rate in the earlier decade, not the much less intensive use of plant and equipment.

Although the K-M model has been extensively criticized, no study has yet presented evidence which modifies the substance of their conclusion.² Most commentators have confined themselves to critiques of the K-M study. Three criticisms are most frequently made. First, K-M's regressions attempt to explain the rise in the before-tax rate of return in manufacturing from the 1930s to the 1950s, but they do not include among their independent variables any measure of the increase since the 1930s in the pressure of demand on capacity. The K-M independent variables other than the tax rate, in addition, appear to have been chosen for their contribution to the fit of the equations and are not derived from a fully articulated model.³ A final problem in the K-M study is the discrepancy between the low measures of shifting obtained when an income share measure of profits is used as the dependent variable and

¹ The higher tax rate, of course, will reduce the pressure of excess demand in the economy. But this could be achieved as well by a tax increase which does not directly result in price increases, i.e., an increase in the personal income tax rate, and which is thus more consistent with the goal of price stability.

² The best critiques to date are the papers by R. E. Slitor [22] and R. Goode [5]. The study by Challis Hall [8] does not estimate a tax-shifting coefficient and the only support of the hypothesis of zero shifting is a slightly higher R^2 than in the full-shifting case. The cross-section regressions by R. Kilpatrick [17] are claimed to support the K-M conclusion of full shifting and are discussed more fully below in Part III.

³ K-M begin with an eight-equation general macro model which is left unexplained ("it cannot be our purpose here to develop a general macro model" [18, p. 35]). No functional forms are specified except for the appearance here and there of a lagged variable. The method of approach is "experimentation" to "find variables which are highly correlated with the rate of return but not with each other" [18, p. 33].

the high coefficients of shifting which result when the rate of return on assets is the dependent variable.

This paper presents a result opposite to that of K-M and finds that on the average the corporation income tax is not shifted in the short run in U.S. manufacturing. The tool of analysis, as in the K-M study, is a set of time-series regression equations in which the corporation profit rate is the dependent variable. Unlike the K-M study, the independent variables here are derived from a model of the probable profit performance of firms that practice mark-up pricing behavior. The coefficient on an independent tax variable is examined for evidence that corporations raise their mark-up margins in response to an increase in the tax rate, thus shifting the burden of the tax forward to consumers.

The period of fit covers the full time span of the Internal Revenue Service data [25], extending from 1924 to 1962, and the regression results are computed by a nonlinear method which is more efficient than the K-M instrumental technique. In the results for manufacturing as a whole the fitted tax shifting coefficients are found to be not significantly different from zero for either an income share or a rate of return concept of profits. The fitted coefficients measure the incidence of the corporation income tax and suggest that corporations suffered the full burden of the tax during the period of study. While tests for each of ten two-digit manufacturing industries reveal no evidence of tax shifting on the average, the industry tax shifting coefficients vary over a considerable range and are significantly correlated with industry concentration ratios. In addition, it is shown that the recent study by Kilpatrick [17] does not support its conclusion of nearly full shifting for the manufacturing aggregate. Finally, the results of this study apply only to short-run shifting and do not rule out the possibility that corporations may be able to shift the burden of the tax in the long run.

I. Tax Shifting and the Theory of the Firm

A firm maximizing profits as in traditional theory would not react to the imposition of a corporation income tax by raising prices, because its price is set by equating marginal revenue and marginal cost, and the tax affects neither MR nor MC.⁴ But since the pioneering empirical work of R. L. Hall and C. J. Hitch [10] and others, it has been recognized that most businessmen do not usually behave as the marginalist theory predicts. They do not know what their marginal curves look like, particularly the marginal revenue curve, and they usually set prices by adding a "mark-up margin" to average total cost at some "normal" level of output. Instead of maximizing profits they attempt to maintain some

⁴ Except to the extent that the tax law does not allow a deduction for imputed interest on working capital, which is a cost of production.

target ratio of profits to sales.⁵ If this desired ratio is computed after taxes, the profits tax becomes an element of average cost to be marked up just like any other kind of cost, and mark-up pricing behavior appears to lead automatically to the conclusion that full tax shifting will result.

But in fact shifting may not automatically occur in the mark-up case. It will not if price is already at the optimum profit-maximizing level before the tax is imposed. The K-M contention that the tax is fully shifted implies that producers always set their prices *below* the profit-maximizing level, since, if prices are already at or above this level when the tax is imposed, a further increase intended to recoup the tax will depress profits. Firms may keep their prices below the optimum level for two reasons:

1. By doing so they may be able to thwart entrants or the antitrust division and thus maximize *long-run* profits. But in this case an increase in tax rates believed to be permanent would not lead to shifting because firms would still want to maintain their price at a sub-optimum level.

2. A margin of unexploited profits may also result as a normal characteristic of the oligopolistic game. A price leader, fearful that its price increase may not be followed, may delay desired price increases until the industry as a whole receives a "signal" which simultaneously induces all firms to desire higher prices. Either expensive wage agreements or increases in the corporation income tax would qualify as appropriate signals for this kind of behavior.

This discussion of mark-up pricing behavior makes the full shifting of the corporation income tax a plausible but not a certain result, and for further information on the degree of shifting we must turn from general theoretical analysis to a specific model suitable for empirical testing. Since the tax liability is not known until the level of profits is fixed, we shall first describe the determination of profits for a representative firm

⁵ The assumption of this paper that businessmen practice markup pricing (sometimes called the "full cost" principle of pricing) is supported by numerous empirical studies. The following are representative conclusions: "It is impossible not to be struck by the devastating completeness of entrepreneurs' uncertainty about matters usually assumed to be known in the textbooks. In this fog of uncertainty the full cost principle may provide a guiding beacon—indeed it often does so" [11, pp. 5-6]. "We concluded that the twelve firms studied did normally fix the price of their products with the guidance of the full cost principle" [2, p. 142]. "Generally speaking, the entrepreneurs in our sample aimed at earning a standard gross margin rather than maximising the current rate of profit" [2, p. 146]. "There can be no doubt that it is an almost universal custom to use as a basis for price fixing a figure . . . which would be described by an economist as a long-period average cost. It is equally indisputable that in many cases there exists within the firm a recognised profit margin (usually expressed as a percentage of average cost) which remains stable over periods long enough to be significant and which is generally accepted as normal" [20, p. 114]. "The businessmen's views on price policy were found to be almost unanimous. . . . To this estimate of the total cost of each article was added a profit margin, based largely on convention" [7, p. 150].

practicing mark-up pricing in a no-tax world. Then a corporation income tax will be introduced, and we shall test the hypothesis that its burden is shifted.⁶

A. *Profits in a Representative Firm*

At any given time our representative firm produces output Q_t in a plant with capacity Q_t^* . Its production-line employees each produce q_t^p units of output, and their average productivity q_t^p increases over time. The firm's demand for production workers can be written

$$(1) \quad L_t^p = Q_t/q_t^p.$$

The work of nonproduction or "fixed" employees (maintenance, research, and clerical employees) is not directly associated with actual output Q_t ; instead each "fixed" worker is needed to maintain q_t^f units of the plant's capacity output Q_t^* .⁷ The demand for fixed labor, then, is

$$(2) \quad L_t^f = Q_t^*/q_t^f.$$

If b units of materials input are required to produce a unit of output, the demand for materials M_t is

$$(3) \quad M_t = bQ_t.$$

Letting w_t^p and w_t^f denote the wage rates of the two different types of labor and p_t^m the price of materials, we can represent the firm's operating costs C_t by its wage bill plus materials costs.

$$(4) \quad C_t = w_t^p L_t^p + w_t^f L_t^f + p_t^m M_t = \left(\frac{w_t^p}{q_t^p} + b p_t^m \right) Q_t + \frac{w_t^f}{q_t^f} Q_t^*.$$

⁶ To test the effect of the corporation income tax on profit rates, we must be able to estimate the level of profit rates in the absence of the tax. Obviously we must assume that without a corporation income tax government revenues are maintained by some other kind of tax. Otherwise a diminution of government revenues would result in a change either in government spending or in the surplus-deficit position and thus in all the economic magnitudes which determine profits.

⁷ The distinction between production and nonproduction workers was suggested by the studies of Kuh [19], Wilson and Eckstein [26], Gallaway [4], and others, which show that the productivity of labor declines during cyclical contractions and rises during the early stages of cyclical expansions, partly because the work force consists of many nonproduction workers whose services are demanded in proportion to capacity rather than output. The distinction between the two types of workers is important whenever the growth of capacity differs from that of output. For instance, the rapid growth of new capacity in 1956 and 1957 was accompanied by a large increase in nonproduction worker employment at a time when real output was relatively stagnant. This has been cited by Schultz [21, pp. 122-25] as an important reason for the slow growth of productivity during those years.

The firm sets its output price p_t by multiplying its average total cost by a markup fraction m . To avoid frequent changes in price resulting from temporary changes in output, m is applied to average cost at capacity output, c_t^* .

$$(5) \quad p_t = mc_t^* = m \left(\frac{w_t^p}{q_t^p} + b p_t^m + \frac{w_t^f}{q_t^f} \right).$$

Total revenue is then

$$(6) \quad R_t = p_t Q_t = m \left(\frac{w_t^p}{q_t^p} + b p_t^m + \frac{w_t^f}{q_t^f} \right) Q_t.$$

We define cash flow Z_t' (the prime represents the absence of taxes) as the difference between revenue R_t and operating cost C_t .

$$(7) \quad Z_t' = R_t - C_t = \left[(m-1) \left(\frac{w_t^p}{q_t^p} + b p_t^m \right) + m \frac{w_t^f}{q_t^f} \right] Q_t - \frac{w_t^f}{q_t^f} Q_t^*.$$

Unfortunately data on the wage bill by industry do not distinguish between w_t^p and w_t^f , and neither wage cost nor materials cost data are available for 18 of the 36 years which we want to include in the regressions, so that we cannot estimate (7) in its present form.⁸ To approximate the wage rates we shall assume that wages are a simple function of the average trend productivity of labor (q_t^p and q_t^f) and of an index of the general level of prices in the economy p_t^g . This assumption is valid if changes in the wage differential between production and nonproduction workers are proportional to changes in the productivity differential between them, and if the profit margin m is constant. This latter condition is, of course, completely consistent with the markup theory used to derive (7). In addition we shall assume that materials prices are proportional to the general price index p_t^g . This assumption is more tenable for manufacturing as a whole than for separate industries. Specifically, these assumptions are written:

$$(8) \quad w_t^p = e p_t^g q_t^p,$$

$$(9) \quad w_t^f = f p_t^g q_t^f,$$

⁸ Data on wage and materials costs by industry are contained in the *Census of Manufactures* and the *Annual Survey of Manufactures*, which are available only for the following dates: 1919-39 biannually, 1947, and 1949 to the present.

$$(10) \quad p_t^m = g p_t^o.$$

Substituting (8), (9), and (10) into (7), and writing p_t/p_t^o as h_t , we obtain

$$(11) \quad Z_t' = [(m-1)(e+bg) + mf] \frac{p_t Q_t}{h_t} - f \frac{p_t Q_t^*}{h_t}.$$

The form to be estimated would be

$$(12) \quad Z_t' = \alpha_1 \frac{R_t}{h_t} + \alpha_2 \frac{R_t^*}{h_t} + u_t$$

where $R_t = p_t Q_t$, $R_t^* = p_t Q_t^*$, and u_t is the error term.

To estimate (12) with industry data we must assume that all manufacturing firms follow pricing practices similar to those of our representative firm, and that (12) represents the behavior of profits in every manufacturing industry. (12) is an inappropriate form for time series estimation, however, because in a growing economy the independent variables will be highly collinear. It is reasonable to assume that the error term grows with the size of the economy, and that we can therefore improve the efficiency of our estimates by rewriting (12) in a standardized form.⁹ Let us define a new error term u_t' which is proportional to a scale variable. Since u_t' might grow proportionately with either assets (K_t) or sales (R_t), there is no a priori way to choose between $\sigma^2 K_t^2$ and $\sigma^2 R_t^2$ as the specification of the error variance. We shall make estimates for each assumption, standardizing first by K_t (in which case the dependent variable is the rate of return on assets) and then by R_t (so that the dependent variable becomes an income share concept).¹⁰

As an additional modification to (12), recall that we were forced to assume (8), (9), and (10) because of data limitations. We should recognize that prices and costs, while highly correlated in the long run, may have different short-run cyclical patterns. If prices increase faster than wages in the first stages of a business expansion and wages increase more rapidly later, profits will vary positively with differing rates of change of output $\Delta Q_t/Q_t$. Another independent determinant of profits may be the effect of price changes $\Delta p_t/p_t$ on inventory valuation profits. Although $\Delta Q_t/Q_t$ and $\Delta p_t/p_t$ are likely to be highly correlated, it is possible that in some periods of stable output and rising prices (e.g., 1946-48 and 1955-57), or of rising output and falling prices (e.g., 1926-29), their separate effects can be distinguished. A given percentage change in out-

⁹ Johnston [13, p. 211].

¹⁰ The numerator of the "rate of return" in this study is cash flow, Z_t , which was first defined in equation (7). It includes profits, depreciation, depletion, and interest paid.

put or prices presumably affects large and small companies in the same way, so that these variables enter our estimating equations in unstandardized form. The final descriptions of the rate of return and income share concepts in the no-tax case are then:

$$(13) \quad \frac{Z'_t}{K_t} = \alpha_1 \frac{R_t}{h_t K_t} + \alpha_2 \frac{R_t^*}{h_t K_t} + \alpha_3 \frac{\Delta p_t}{p_t} + \alpha_4 \frac{\Delta Q_t}{Q_t} + u'_t.$$

$$(14) \quad \frac{Z'_t}{R_t} = \alpha_1 \frac{1}{h_t} + \alpha_2 \frac{R_t^*}{h_t R_t} + \alpha_3 \frac{\Delta p_t}{p_t} + \alpha_4 \frac{\Delta Q_t}{Q_t} + u'_t.$$

We should expect that if our theory is a valid representation of a real world in which firms by markup behavior attempt to keep Z/R relatively constant, then (14) will result in a poorer fit than (13) (i.e., Z/R can be described more adequately than Z/K by a constant term alone).

B. Taxes and Nonlinear Estimation

By shifting the burden of the corporation income tax our representative firm can reduce the impact of any given tax rate on its after-tax profits. Denoting profits net-of-tax by π_t^n , profits in the absence of tax by π_t' , and the proportion of tax liabilities T which the firm shifts by the parameter α_5 , we can write:

$$(15) \quad \pi_t^n = \pi_t' - T_t + \alpha_5 T_t.$$

If no part of the tax burden is shifted, $\alpha_5 = 0$ and $\pi_t^n = \pi_t' - T_t$. If the firm is able to shift the entire burden of the tax, $\alpha_5 = 1$ and $\pi_t^n = \pi_t'$, i.e., net-of-tax profits are just as large as profits would have been in the absence of the tax. Using the two definitions

$$(16) \quad \pi_t^g = \pi_t^n + T_t$$

$$(17) \quad T_t = v_t \pi_t^g$$

(where the superscript g stands for "gross-of-tax" and v_t is the tax rate), and dividing by K_t , (15) becomes

$$(18) \quad \frac{\pi_t^g}{K_t} = \frac{\pi_t'}{(1 - \alpha_5 v_t) K_t}.$$

(18) cannot be estimated by ordinary least squares, because the parameter α_5 appears in the denominator. K-M tried to escape the problem of nonlinear estimation by using the instrumental variables technique to estimate

$$(19) \quad \frac{\pi_t^g}{K_t} = \frac{\pi_t'}{K_t} + \alpha_5 v_t \frac{\pi_t^g}{K_t} + u'_t.$$

The independent variable $v_t \pi_t^q / K_t$ is a function of the dependent variable π_t^q / K_t and is thus correlated with the term u_t' . To avoid this difficulty K-M used the tax rate v_t as an instrument for $v_t \pi_t^q / K_t$. This procedure is an unreliable approximation to (18), as we shall see below in Part II.

We shall estimate (18) directly by an iterative method which is more efficient than the indirect K-M instrumental variable technique.¹¹ Using the definition $Z_t' = \pi_t' + D_t$, where D_t is depreciation, depletion, and interest paid, we can substitute (13) into (18). We obtain the complete equation derived from the preceding theoretical discussion for the estimation of the tax-shifting coefficient α_5 . (We give just the form for standardization by K_t from (13) and do not write separately the similar form derived from (14) for standardization by R_t .)

$$(20) \quad \frac{\pi_t^q}{K_t} = \alpha_1 \frac{R_t}{(1 - \alpha_5 v_t) h_t K_t} + \alpha_2 \frac{R_t^*}{(1 - \alpha_5 v_t) h_t K_t} + \alpha_3 \frac{\Delta p_t}{(1 - \alpha_5 v_t) p_t} \\ + \alpha_4 \frac{\Delta Q_t}{(1 - \alpha_5 v_t) Q_t} - \frac{D_t}{(1 - \alpha_5 v_t) K_t} + u_t'.$$

The first step in the estimation procedure is to linearize (20) by means of a Taylor series expansion around an arbitrary initial set of parameters. The resulting linear form is then used to estimate the difference between the true parameters and the arbitrary initial set. The procedure is completed after several iterations when the estimated difference between the true and fitted parameters is reduced to negligible size.

The technique of linearizing by means of a Taylor series expansion can be seen most clearly if we first perform it for a nonlinear function written in general form:

$$(21) \quad y_t = F(\beta_1, \dots, \beta_n; X_1, \dots, X_n) + u_t$$

where y_t is the dependent variable, the β_i are the coefficients to be estimated and the X_i are the independent variables. Consider an initial trial set of parameters

$$(22) \quad P^0 = (\beta_1^0, \dots, \beta_n^0)$$

where the superscript stands for the iteration number. In practice the initial set of parameters can be guessed from past experience or set equal to some arbitrary number. Each of the true parameters β_i which we are trying to estimate in (21) consists of our initial trial guess β_i^0 and an initial error $\Delta \beta_i^0$:

¹¹ A discussion of the general properties of nonlinear estimators can be found in [3, Ch. 10], and [12, p. 278].

$$(23) \quad \beta_i = \beta_i^0 + \Delta\beta_i^0.$$

Given β_i^0 from an initial trial, by estimating the initial errors $\Delta\beta_i^0$ we will be able to improve our estimates of the true coefficients β_i . Successive iterations should reduce the estimates of the successive errors $\Delta\beta_i^{(n)}$, $\Delta\beta_i^{(n+1)}$, ... to negligible size. We can use ordinary least squares to estimate the errors $\Delta\beta_i^0$ if we expand (21) in a Taylor series around the initial set of parameter estimates β_i^0 , retaining only the first order terms. First we substitute (23) into (21) to obtain an expression for y_i in terms of β_i^0 and $\Delta\beta_i^0$:

$$(24) \quad y_i = F(\beta_1^0 + \Delta\beta_1^0, \dots, \beta_n^0 + \Delta\beta_n^0; X_1, \dots, X_n) + u_i.$$

Next we perform the Taylor series expansion:

$$(25) \quad y_i = F(\beta_1^0, \dots, \beta_n^0; X_1, \dots, X_n) + \sum_{i=1}^n \Delta\beta_i^0 \frac{\partial F}{\partial \beta_i}(\beta_1^0, \dots, \beta_n^0; X_1, \dots, X_n) + u_i.$$

Now we simply substitute (20) into (25) and combine terms to obtain the actual form used in this paper for the estimation of the tax-shifting coefficient α_5 . For instance, the equation fitted for iteration number $(n+1)$ is:

$$(26) \quad \frac{\pi_t^0}{K_t} + \frac{D_t}{X_t^{(n)}K_t} = \alpha_1 \frac{R_t}{X_t^{(n)}h_tK_t} + \alpha_2 \frac{R_t^*}{X_t^{(n)}h_tK_t} + \alpha_3 \frac{\Delta p_t}{X_t^{(n)}p_t} + \alpha_4 \frac{\Delta Q_t}{X_t^{(n)}Q_t} + \Delta\alpha_5^{(n+1)} \frac{v_t}{[X_t^{(n)}]^2} \left[\alpha_1^{(n)} \frac{R_t}{h_tK_t} + \alpha_2^{(n)} \frac{R_t^*}{h_tK_t} + \alpha_3^{(n)} \frac{\Delta p_t}{p_t} + \alpha_4^{(n)} \frac{\Delta Q_t}{Q_t} - \frac{D_t}{K_t} \right] + u_t'.$$

The superscripts refer to the iteration number at which each term is evaluated, and $X_t^{(n)}$ stands for $(1 - \alpha_5^{(n)}\eta_t)$. The standard error of $\Delta\alpha_5$ in the final iteration is the standard error of the shifting parameter α_5 which is presented in the tables of results. (A similar form is derived from (14) for standardization by R_t).¹²

¹² The estimates of (26) were calculated on the IBM 1620 at the M.I.T. Sloan School of Management using a set of programs for nonlinear estimation written by the author. Each iteration was begun by setting all parameters equal to zero, and convergence was considered complete when the parameter $\Delta\alpha_5$ in (26) became less than .005 in absolute value. Three to four iterations were usually necessary to achieve this standard of convergence.

II. The Results

A. Comparison with Krzyzaniak and Musgrave

The K-M conclusions are radically different from our own. Since the two studies cover different time spans and include different variables in regressions estimated by different methods, we shall begin with the published K-M result and then change step-by-step to our results based on Part I above. Equation (I-1A) in Table 1 is the K-M so-called "standard model" [18, pp. 44-45], an estimate of the general model (18) by the instrumental variable technique (19).¹³ The coefficient of the tax variable $v\theta_i^m$ represents the well-known conclusion of the K-M book that the corporation income tax is shifted forward by almost 136 per cent of tax liabilities. Since the data we use have not been collected for 1942, that year is excluded in equation (I-1B), to facilitate comparison of the K-M results with ours. The removal of 1942 raises the tax shifting parameter to 1.455 and raises the Durbin-Watson statistic to 3.32, enough to allow the rejection of the hypothesis of no negative serial correlation.

NOTATION FOR TABLES 1-3

| | |
|------------------|---|
| ΔC_{t-1} | Annual lagged change in the ratio of consumption to GNP [18, p. 75]. |
| $D-W$ | Durbin-Watson statistic adjusted for omission of war years. |
| h_t | Ratio of industry wholesale price index p_t to wholesale price index for manufacturing aggregate (for computation see data appendix). |
| J_t | Ratio of tax accruals (all levels of government) other than corporate tax, minus government transfers, to GNP [18, p. 75]. |
| K_t | Total assets (book value in current dollars), from <i>Statistics of Income</i> . |
| p_t | Wholesale price index (for computation see data appendix). |
| Q_t | Hybrid real output (for computation see data appendix). |
| r_t^m | K-M measure of rate of return [18, p. 74]. |
| R_t | Gross sales from <i>Statistics of Income</i> . |
| R_t^* | Capacity real output times p_t (for computation see data appendix). |
| \bar{R}^2 | R^2 adjusted for degrees of freedom. |
| v_t | Tax rate (see data appendix). |
| $v\theta_i^m$ | Tax liabilities/capital [18, p. 74]. |
| V_{t-1} | Lagged ratio of inventories to sales in manufacturing [18, p. 75]. |
| Z_i^* | This dependent variable is written out in full in equation (26) above. |
| Z_i^q | Cash flow before tax = Profits before tax + Depreciation + Depletion + Interest paid. |

¹³ In all our results the Durbin-Watson statistic is adjusted for the omission of the observations for the war years.

TABLE 1.—COMPARISON WITH THE KEZYANI-K-MUSGRAVE RESULTS
(t coefficients in [])

| Dependent Variables | Independent Variables | | | | | \bar{R}^2 | $D-W^a$ | Shifting Parameter | Years | Estimation Method |
|------------------------------------|-----------------------|-----------------------|-------------------------|--------------------------------|--------------------------|-----------------|---------|--------------------|---------------------|---------------------------|
| K-M Equations: r_t^m | constant | ΔC_{t-1} | V_{t-1} | J_t | $v_t r_t^m$ | v_t | | | | |
| (I-1A) | .281 [5.314] | .411 [2.741] | -.509 [-2.926] | -.834 [-4.759] | 1.358 [12.628] | — | .964 | 2.78* | 1935-42+ 1948-59 | Instrumental ^b |
| (I-1B) | .271 [5.214] | .344 [2.231] | -.444 [-2.485] | -.884 [-5.094] | 1.455 [10.504] | — | .964 | 3.32+ | 1935-41+ 1948-59 | Instrumental ^b |
| (I-1C) | .373 [5.761] | .432 [2.391] | -.800 [-3.589] | -.941 [-4.528] | — | .921 [8.500] | .812 | 2.80* | 1935-41+ 1948-59 | Nonlinear* |
| Present Study: Z_t^* K_t | | $\frac{R_t}{h_t K_t}$ | $\frac{R_t^*}{h_t K_t}$ | $\frac{\Delta p_t}{\hat{p}_t}$ | $\frac{\Delta Q_t}{Q_t}$ | v_t | | | | |

Notation for all tables is summarized on p. 000.

^a Symbols attached to Durbin-Watson statistics:

+ Hypothesis of no serial correlation rejected at 5 per cent level.

* Test inconclusive at 5 per cent level.

** Hypothesis of no serial correlation accepted at 5 per cent level.

^b The K-M statutory rate series was used as an instrument for $v_t r_t^m$ in equations I-1A, I-1B, and I-1C.

• For the exact form used in nonlinear estimation, see equation (26) in the text.

TABLE 1.—(Continued)

| Dependent Variables | Independent Variables | | | | | \bar{R}^2 | D—W ^a | Shifting Parameter | Years | Estimation Method |
|---------------------|-----------------------|-------------------------|--------------------------|--------------------------|--------------------------|------------------------|------------------|--------------------|---------------------|------------------------|
| (I-2K) | $\frac{Z_t^*}{R_t}$ | $\frac{1}{h_t}$ | $\frac{R_t^*}{h_t R_t}$ | $\frac{\Delta p_t}{p_t}$ | $\frac{\Delta Q_t}{Q_t}$ | $\frac{.143}{[.738]}$ | 1.71* | .143 [.738] | 1935-41+ 1948-59 | Nonlinear ^a |
| (I-2R) | $\frac{Z_t^*}{K_t}$ | $\frac{.151}{[3.940]}$ | $\frac{-.045}{[-1.759]}$ | $\frac{.071}{[1.460]}$ | $\frac{.062}{[3.068]}$ | $\frac{.214}{[1.340]}$ | 1.63* | .214 [1.340] | 1935-41+ 1948-59 | Nonlinear ^a |
| (I-3K) | $\frac{Z_t^*}{R_t}$ | $\frac{.176}{[8.942]}$ | $\frac{-.062}{[-4.480]}$ | $\frac{.043}{[1.541]}$ | $\frac{.056}{[3.567]}$ | $\frac{.110}{[1.220]}$ | 1.67* | .110 [1.220] | 1925-41+ 1946-62 | Nonlinear ^a |
| (I-3R) | $\frac{Z_t^*}{R_t}$ | $\frac{.179}{[13.123]}$ | $\frac{-.065}{[-7.175]}$ | $\frac{.051}{[2.224]}$ | $\frac{.060}{[4.506]}$ | $\frac{.057}{[.693]}$ | 2.05** | .057 [.693] | 1925-41+ 1946-62 | Nonlinear ^a |

In equation (I-1C) the K-M instrumental variable method is replaced by the more efficient nonlinear estimation technique described above. The result reveals the inaccuracy of the K-M approximation. The shifting parameter is reduced from 1.455 to .921, while the constant and the parameter on V_{t-1} are raised considerably. The instrumental equation (I-1B) also substantially exaggerates the goodness of fit of the K-M variables; the variance around the regression is over five times larger in (I-1C) than in the instrumental approximation (I-1B).¹⁴

Differences in specification between the K-M study and the present one are much more important than the different methods of estimation. The K-M variables were not derived from a stated set of hypotheses about profit behavior. A quick look at the data indicates that the significant t -coefficients in equation (I-1C) arise from historical coincidences. The change in the ratio of consumption to GNP (ΔC_t) tends to be positive in recessions (as predicted by most theories of the consumption function). Since profits tend to be high in the first post-recession year, the coefficient of the *lagged* K-M variable (ΔC_{t-1}) tends to be positive, as in Table 1. But the positive correlation between the rate of return and ΔC_{t-1} is just a coincidence owing to the fact that postwar recessions have lasted only one year. K-M's coefficient on ΔC_{t-1} would have been different if they had included all of the 1930s in their sample.

K-M could have justified the inventory-sales ratio (V_{t-1}) by considerations similar to ours in Part I. Inventories are a relatively fixed cost, and one would expect profits to increase with a decrease in the ratio of inventories to sales. But it makes no sense to lag the variable one year—this makes the lagged inventory-to-sales ratio reach a cyclical *peak* during the first post-recession year, when in fact one of the main reasons for high profits in the initial post-recession year is the *low* ratio of inventories to sales.¹⁵ The coefficients on V_{t-1} are significantly negative

¹⁴ The inaccuracy of the K-M instrumental technique can be demonstrated by two related experiments. First, it is known that the direct nonlinear procedure finds a local minimum of the residuals, but this does not exclude the possibility that the instrumental estimate in (I-1B) might be another local minimum. To test this possibility the direct nonlinear iteration procedure was initiated at the parameter values of I-1B and immediately converged to the values of I-1C, indicating that the values of I-1B are not a local minimum.

Second, the parameter values of I-1B, fitted by the instrumental variables technique, were substituted directly into the original theoretical equation (18). The resulting \bar{R}^2 between actual and fitted values was only .086, compared to .812 using the values from I-1C. The instrumental estimates are especially inaccurate for the Korean war period. In 1951, for instance, the fitted value of the rate of return is

$$\frac{1951}{(1 - \alpha_{1951})K_{1951}} = \frac{.052}{[1 - (1.455 \times .6775)]} = 365.9 \text{ per cent,}$$

compared with the actual 1951 value of 22.4 per cent.

¹⁵ The following are the annual postwar inventory/sales ratios for U.S. manufacturing for

despite this misspecification because the levels of both profit rates and V_{t-1} changed during World War II.

There is no justification for the third K-M variable, the ratio of tax accruals to GNP (J_t). If it expresses the thought that profits respond to aggregate demand, which usually rises when government spending expands, then government spending would be a better variable, and we would expect the coefficients to be positive. But in Table 1 the coefficients on J_t are all *negative*. The statistical significance of the negative coefficients on J_t is due to another coincidence: the dependent variable (the rate of return) declined somewhat from the early postwar years to the late 1950s, while the tax rate rose (from 38 per cent to 52 per cent). Thus the negative coefficient on J_t pulls down the value of the dependent variable predicted by taxes alone and results in an improved fit.

The second part of Table 1 shows the fitted results for our theory derived above in Part I, with the 19 observations which cover the K-M time span of 1935-41 and 1948-59.¹⁶ Equation (I-2K) is the fitted version of equation (26) above, with profits standardized by total assets (the rate of return concept). Equation (I-2R) is the same with profits standardized by revenue (the income share concept). The variance around the regression in (I-2K) is only about one-quarter of that in the K-M version (I-1C) estimated by the same nonlinear technique, and the Durbin-Watson test is more favorable. All the coefficients have the expected sign in both (I-2K) and (I-2R). As anticipated from our markup hypothesis of firm behavior, the income-share variable has less variance than the rate of return variable. Since the former can be better predicted than the latter by a constant alone, the \bar{R}^2 in (I-2R) is smaller than in (I-2K). Also, the theory above allowed no place for a constant term and in accord with this a priori expectation the fitted constants in (I-2K) and (I-2R) were insignificant (and the equations are shown in Table 1 as recomputed with the constant term suppressed). Most important of all, the parameter of tax shifting is not significantly different from zero.

K-M claim that years before 1935 cannot be included in a study of profits because the "inclusion of the early thirties would have been undesirable, and comparable data for the twenties are not readily available" [18, p. 23]. While the inclusion of the war years may be undesir-

the K-M years, 1948-59, as given in the 1966 *Economic Report of the President*, p. 254:

| | | | | | |
|------|-------|------|-------|------|-------|
| 1948 | 1.57 | 1952 | 1.78 | 1956 | 1.73 |
| 1949 | 1.75 | 1953 | 1.76 | 1957 | 1.80 |
| 1950 | 1.48* | 1954 | 1.81 | 1958 | 1.84 |
| 1951 | 1.66 | 1955 | 1.62* | 1959 | 1.70* |

An asterisk marks the values for the initial post-recession years, 1950, 1955, and 1959.

¹⁶ The data are described in the appendix.

able because of price and wage controls, such tampering with the marketplace was relatively minor during the early 1930s, and thus there is no obvious reason why the early 1930s should be omitted.¹⁷ Data for most of the 1920s are available or can be constructed. Hence, in the third section of Table 1, equations (I-3K) and (I-3R), the time period has been extended from 1935-41+1948-59 to 1925-41+1946-62 (making 34 observations instead of 19). The results for the longer period are strikingly similar to those for the shorter period, indicating that the pre-1935 behavioral relationships are not very different from the post-1935 results. All coefficients have the expected signs, and all are highly significant except for the price change coefficient in (I-3K) and the tax-shifting parameters. As in the shorter period the tax-shifting coefficients are very low and are not significantly different from zero. The values of the coefficients for each variable are roughly the same in both the rate of return (I-3K) and the income share (I-3R) formulation. The Durbin-Watson test is inconclusive in (I-3K) and allows us to accept the hypothesis of no serial correlation at the 5 per cent level in (I-3R).

B. *The Detailed Results by Industry*

The data available in [25] for the 1920s include only eleven two-digit manufacturing industries. Excluding the Miscellaneous category, whose composition frequently changes, we are left with ten two-digit industries for which IRS data are available since 1924. After 1936 the data for twenty two-digit industries are published, but fortunately it was easy to combine these into the original ten for this long-period time series study. The construction of a wholesale price series (for the $\Delta p_i/p_i$ variable) for these ten industries was much less straightforward, as is explained in the data appendix. Table 2 shows two equations for each of the ten industries; as in Table 1 the set of "K" equations is standardized by total assets and the "R" equations by gross sales. As expected from our markup hypothesis of firm behavior, the markup fraction varies less over time than the rate of return, so that the dependent variable in the "R" equations has less variance than in the "K" equations. Since the former can be better predicted than the latter by a constant term alone, it is not surprising to see that in every industry the \bar{R}^2 for the "R" equation is lower than that for the "K" equation.

The significance of the parameters α_1 through α_5 in Table 2 should be judged by a one-tailed *t* test, for which the five per cent value is 1.64 and the one per cent value is 2.32. By this standard all of the 20 esti-

¹⁷ It did not appear worthwhile to adjust for the discontinuation of consolidated returns in 1934. Michael Gort [6, p. 297] comments that the changes in gross income shown in the *Statistics of Income* for 1934 raise doubts about the consistency of the classification principles used in 1933 and 1934.

TABLE 2.—COMPLETE RESULTS BY INDUSTRY, 1925-41+1946-62^b
t coefficients in []

| Independent Variables in "K" equations in "R" equations | t coefficients in [] | | | | | | \bar{R}^2 | $D-W^a$ |
|---|-----------------------|-----------------------|-------------------------|--------------------------|--------------------------|-------------------|-------------|---------|
| | constant | $\frac{R_t}{h_t K_t}$ | $\frac{R_t^*}{h_t K_t}$ | $\frac{\Delta p_t}{p_t}$ | $\frac{\Delta Q_t}{Q_t}$ | v_t | | |
| Food, Beverages, and Tobacco (II-1K) | .030 [2.339] | .087 [4.832] | -.036 [-2.429] | .055 [2.783] | .097 [2.110] | .034 [.228] | .905 | 2.53* |
| (II-1R) | | .071 [4.005] | -.004 [-.240] | .032 [2.718] | .067 [2.443] | -.329 [-2.890] | .479 | 2.26* |
| Textiles and Apparel (II-2K) | -.108 [-3.641] | .208 [4.514] | -.056 [-1.547] | .138 [2.695] | .031 [.759] | -.154 [-.533] | .915 | 1.79* |
| (II-2R) | | .178 [3.601] | -.098 [-2.731] | .164 [4.382] | .025 [.854] | .176 [.637] | .775 | 1.21* |
| Lumber and Furniture (II-3K) | | .175 [5.502] | -.070 [-3.241] | .173 [3.894] | -.005 [-.173] | .280 [1.375] | .917 | 1.91** |

Notation for all tables is summarized on p. 741.

^a Symbols attached to Durbin-Watson statistics:

+ Hypothesis of no serial correlation rejected at 5 per cent level.

* Test inconclusive at 5 per cent level.

** Hypothesis of no serial correlation accepted at 5 per cent level.

^b All equations in Table 2 were estimated by the nonlinear method; the form estimated was equation (26) in the text.

TABLE 2.—(Continued)

| Independent Variables in "K" equations | in "R" equations | | | | | | R^2 | $D-W^a$ |
|---|------------------|-----------------------|-------------------------|--------------------------|--------------------------|----------------------|-------|---------|
| | constant | $\frac{R_t}{h_t K_t}$ | $\frac{R_t^*}{h_t K_t}$ | $\frac{\Delta p_t}{p_t}$ | $\frac{\Delta Q_t}{Q_t}$ | η_t | | |
| | — | $\frac{1}{h_t}$ | $\frac{R_t^*}{h_t R_t}$ | $\frac{\Delta p_t}{p_t}$ | $\frac{\Delta Q_t}{Q_t}$ | η_t | R^2 | $D-W^a$ |
| (II-3R) | | | | | | (Shifting Parameter) | | |
| Paper Products (II-4K) | .057 [2.280] | .186 [6.052] | — .083 [—4.579] | .198 [3.067] | .064 [1.563] | .054 [.173] | .723 | 1.83** |
| (II-4R) | | .174 [10.050] | — .047 [—3.890] | .100 [4.113] | .032 [2.314] | .425 [7.085] | .983 | 1.50* |
| Printing and Publishing (II-5K) | | .188 [12.040] | — .059 [—5.548] | .089 [3.244] | .031 [2.018] | .394 [6.535] | .896 | 1.92** |
| (II-5R) | | .223 [8.316] | — .142 [—5.543] | .146 [2.313] | .084 [2.633] | — .310 [—1.785] | .932 | 1.34* |
| Chemicals and Petroleum (II-6K) | | .216 [9.311] | — .088 [—5.452] | .091 [1.473] | .103 [3.190] | — .674 [—3.885] | .777 | 1.37* |
| (II-6R) | | .180 [8.829] | — .054 [—5.038] | — .013 [— .676] | — .007 [— .135] | .921 [16.870] | .931 | 1.18* |
| Rubber Products (II-7K) | | .182 [7.564] | — .059 [—3.374] | — .024 [— .848] | — .006 [— .202] | .981 [19.701] | .689 | 1.51* |
| | | .081 [6.672] | — .023 [—3.086] | .038 [1.721] | .017 [.867] | .896 [10.286] | .920 | 1.24* |

TABLE 2.—(Continued)

| Independent Variables in "K" equations | constant | $\frac{R_t}{h_t K_t}$ | $\frac{R_t^*}{h_t K_t}$ | $\frac{\Delta p_t}{p_t}$ | $\frac{\Delta Q_t}{Q_t}$ | v_t | \bar{R}^2 | $D-W^a$ |
|---|----------|-----------------------|-------------------------|--------------------------|--------------------------|-----------------------------------|-------------|---------|
| | | | | | | | | |
| in "R" equations | — | $\frac{1}{h_t}$ | $\frac{R_t^*}{h_t R_t}$ | $\frac{\Delta p_t}{p_t}$ | $\frac{\Delta Q_t}{Q_t}$ | v_t (Shifting) Parameter) | \bar{R}^2 | $D-W^a$ |
| | | | | | | | | |
| (II-7R) | | .078 [5.670] | — .023 [-2.570] | .038 [1.933] | .017 [.998] | .908 [8.285] | .678 | 1.30* |
| Leather Products (II-8K) | | .098 [2.698] | — .046 [-1.573] | .151 [4.136] | .126 [3.101] | .178 [.743] | .817 | 1.42* |
| (II-8R) | | .131 [3.328] | — .074 [-2.365] | .110 [4.475] | .089 [3.270] | — .063 [- .219] | .687 | 1.48* |
| Stone, Clay, and Glass (II-9K) | | .158 [9.922] | — .035 [-3.529] | .086 [1.312] | .033 [2.262] | .551 [7.001] | .961 | .98+ |
| (II-9R) | | .179 [12.885] | — .050 [-6.745] | .212 [2.416] | .067 [3.270] | .336 [3.321] | .786 | 1.33* |
| Metal Products (II-10K) | | .179 [7.857] | — .048 [-3.645] | — .055 [- .989] | .059 [4.268] | — .019 [- .136] | .924 | 1.06+ |
| (II-10R) | | .192 [14.730] | — .054 [-9.052] | — .039 [- .877] | .063 [5.324] | — .143 [-1.047] | .882 | 1.33* |

ated α_1 coefficients in Table 2 are significant at the one per cent level. Seventeen of the 20 coefficients α_2 are significant at the one per cent level, while three are insignificant. Fourteen of the α_3 price change coefficients are significant at better than the five per cent level, as are 12 of the α_4 output change coefficients. Only in the Chemical and Petroleum industry is neither price nor output change significant. All insignificant coefficients have the expected sign—positive for α_1 , α_3 , and α_4 , and negative for α_2 .

Of the 20 tax-shifting parameters, eight are significantly positive at the one per cent level, two are significantly negative at the one per cent level and one at the five per cent level. The remaining nine are insignificant. Overall, the industry results in Table 2 give a description of tax-shifting behavior very similar to that of the aggregate equations in Table 1. In the aggregate equations for the full set of 34 observations, (I-3K) and (I-3R) in Table 1, the estimated tax-shifting parameters are .115 and .057 respectively, while in Table 2 the weighted averages of the equivalent tax parameters are .110 and .003.¹⁸ Thus the aggregate and industry equations tell a consistent story—tax shifting in manufacturing is not significantly different from zero, although it is significantly greater than zero in the Paper, Chemicals and Petroleum, Rubber, and Stone industries, and significantly less than zero in the Printing industry and in one equation for the Food industry.

Although the markup theory presented in Part I is supported here to a substantial degree, in several industries it was necessary to make modifications on the *a priori* equations (13) and (14). First, in the Food, Textile, and Printing industries, the fit of the "K" equations was substantially improved by adding a constant term (the markup theory as developed above is supported to the extent that the constant terms were insignificant in the other seven industries, and for these industries the equations, as shown in Table 2, were recomputed without a constant term). The second modification to the original theoretical equations (13) and (14) in the Food, Lumber, Paper, Printing, and Metal industries was to set

$$h_t = 1, \quad t = 1, \dots, 34$$

where h_t , as defined above, is the price index for the industry's output divided by the price index for aggregate manufacturing output. This was made necessary because for these five industries our original assumption about h_t exaggerated the effect of relative prices on profits and thus worsened the \bar{R}^2 and Durbin-Watson statistics. For the remaining five industries h_t was included and the results were satisfactory. Neither

¹⁸ They are weighted by 1935-39 value added, the weights used by the Federal Reserve Board Index of Industrial Production during the middle of our period.

of these modifications had an appreciable effect on the tax-shifting parameters.¹⁹

III. *Tax Shifting and Industry Concentration*

The hypothesis of forward tax shifting requires that firms have sufficient market power successfully to increase pre-tax profits following a tax rate increase. Robert W. Kilpatrick [17] has recently suggested that, if the tax rate is lower in period m than in another period n , a significant cross-section correlation between an industry's concentration ratio C_i and the change in its profit rate over the interval from m to n (r_n/r_m) _{i} is de facto evidence of tax shifting. Kilpatrick tested the model

$$(27) \quad \left(\frac{r_n}{r_m} \right)_i = a + bC_i + u_i$$

for about 100 industries with an initial time period of 1947-49, when the corporate tax rate was 38 per cent, and the terminal time period 1955-57, when the rate was 52 per cent. Experiments were run for explanatory variables in addition to C_i , such as changes in sales and assorted cost factors. Kilpatrick's equations were unable to explain more than a small fraction of the interindustry variance of r_n/r_m (the \bar{R}^2 values were in the .20 to .30 range), but the coefficients on C_i were significantly greater than zero. And on the assumption $a=1$, Kilpatrick was led by these significant coefficients to accept the hypothesis of tax shifting in the 60-80 per cent range in manufacturing.

Two important questions may be raised regarding Kilpatrick's results. Although market power is necessary for tax shifting to exist, it is not a sufficient condition. During any given span of years, the profits of firms in concentrated industries may rise more (or fall less) than those of firms in more competitive industries *independently* of changes in the tax rate. Thus profit rates fell much less between 1929 and 1933 in the concentrated tobacco and aluminum industries than in the unconcentrated textile industry. In short, Kilpatrick might have found the same sig-

¹⁹ The modifications mentioned in this paragraph are the most important weaknesses in the results, but there do not seem to be any other major econometric problems. If profit rates influence the independent variables at all, it is through the indirect effect of profits on the demand for investment goods and on dividend payments, and thus through them on output, utilization, etc. This chain of causation is probably extended over several years, and it is likely that serious simultaneity problems are absent. Multicollinearity among the independent variables has been eliminated by standardizing the equations by K_i and R_i . It is surprising to find that the correlation between $\Delta p_i/p_i$ and $\Delta Q_i/Q_i$ is quite low (.32 for the aggregate).

One minor difficulty results from assumptions (8), (9), and (10), which impart a slight downward bias of approximately 10 per cent to our estimates of α_5 in this study. A correction for this bias would raise the tax-shifting parameter in (I-3K), for example, from .11 to .12. This bias was discovered after the computations were completed and was not considered serious enough to warrant changing the tables.

nificant coefficient on C_i if he had tested his hypothesis for two periods with similar tax rates, or between two periods when tax rates *fell*. The second question relates to Kilpatrick's assumption that $\alpha=1$ in (27) above, so that any positive coefficient on C_i could be interpreted as evidence of positive shifting. The tacit assumption is that shifting would be zero only for a perfectly competitive industry with $C_i=0$. But the results in this paper suggest that this assumption is wrong. We can fit a

TABLE 3.—CONCENTRATION AND TAX SHIFTING, 1925-41+1946-62

| | (1) | (2) | (3) | (4) | (5) |
|----------------------------|-------|------|------|------|------|
| Food, Beverages, & Tobacco | -.148 | .366 | .122 | .064 | .57 |
| Textiles and Apparel | .011 | .188 | .093 | .055 | .72 |
| Lumber and Furniture | .167 | .143 | .112 | .086 | 1.21 |
| Paper Products | .410 | .240 | .132 | .131 | 1.25 |
| Printing and Publishing | -.492 | .183 | .121 | .099 | .95 |
| Chemicals and Petroleum | .951 | .458 | .129 | .155 | 1.21 |
| Rubber Products | .902 | .547 | .117 | .088 | .85 |
| Leather Products | .058 | .258 | .085 | .044 | .61 |
| Stone, Clay, and Glass | .443 | .434 | .143 | .146 | 1.26 |
| Metal Products | -.081 | .439 | .129 | .111 | 1.10 |

Column (1): Average for each industry of the "K" and "R" tax shifting coefficients shown in Table 2.

Column (2): Average for 1947, 1954, and 1958 for each industry of the percentage of sales in each of its constituent four digit industries accounted for by the top four firms (Concentration ratios). Source: [1, p. 335], combined into ratios for our ten industries using as weights industry value added in 1947, 1954, and 1958.

Column (3): Average for all years of ratio of Cash flow before tax to Total assets (Z/K).

Column (4): Average for all years of ratio of Cash flow before tax to Gross sales (Z/R).

Column (5): Average for all years of ratio of Total assets to Gross sales (K/R).

cross-section regression for our ten industries to measure the dependence of the average rate of tax shifting TS_i on the concentration ratio C_i .²⁰

$$(28) \quad TS_i = -.435 + 2.017C_i; \quad \bar{R}^2 = .381 \\ [-1.360] \quad [2.221]$$

The data for this regression are shown in the first two columns of Table 3. The result is that although the coefficient on the concentration ratio is significantly greater than zero, as in Kilpatrick's study, the constant here is negative, admitting the possibility of a positive relationship between tax shifting and concentration but nevertheless an *average* rate of tax shifting for all manufacturing industries which is not significantly different from zero.

The "negative shifting" for relatively unconcentrated industries suggested by (28) is not necessarily related to the increase in the tax

²⁰ As in all our results the numbers in brackets are *t*-statistics, not standard errors.

rate since the 1920s. An increasing ratio of sales to assets over time will lead to a rising pre-tax rate of return on assets if a constant markup policy is followed. In unconcentrated industries the rising rate of return will attract new entrepreneurs who are unable to enter more concentrated high-barrier industries. Thus the before-tax rate of return in unconcentrated industries may not rise with the increasing productivity of assets, and in fact the rate of return may be so depressed (driven down by a surfeit of entrants who are frustrated by barriers in other fields), that a regression shows negative tax shifting. Note that entry can occur even without an initial rise in the *after-tax* rate of return if all alternative investment yields are equally reduced by the tax. This may be true in the United States where corporation income tax and personal income tax rates (the latter applying to income from unincorporated business and financial assets except for capital gains) have increased simultaneously.

Most of this paper has concentrated on the reasons for changes in profit rates over time for a given industry. Here we can briefly inquire into the determinants of the level of the markup margin (Z/R) at any given time. While the markup margin was much more constant than the rate of return on assets (Z/K) over time for each industry, the *average* level of the rate of return shows considerably less interindustry variance than the average markup margin. Except for Textiles and Leather, competition among industries for investment funds, as economic theory would predict, has caused average levels of the rate of return to cluster around a fairly constant level (11-14 per cent). Although the average levels of markup margins vary much more than rates of return, the former can be explained reasonably well by capital-intensiveness and industry concentration:

$$(29) \quad (Z/R)_i = -0.046 + .118 (K/R)_i + .090 C_i; \quad \bar{R}^2 = .869$$

$$\quad \quad \quad [-2.097] \quad [6.106] \quad [2.420]$$

IV. Conclusion

By following a markup pricing policy during a period in which the average productivity of assets grew, manufacturing corporations were able to maintain a fairly constant ratio of pre-tax profits to sales. Thus their average pre-tax rate of return on assets rose with the rising productivity of assets. Corporate rates of return in manufacturing were improved by the growing productivity of capital in much the same way that wages increased with the growing average productivity of labor. But firms on the average were not able to go beyond this and shift the burden of increased corporation income tax rates. This conclusion is more plausible than the contention by Krzyzaniak and Musgrave that corporations succeeded in shifting more than 100 per cent of their tax liability. In a period during which strong labor unions possessed coun-

tervailing power, why would they have *allowed* corporations fully to shift the burden of the corporate tax? The aggressive corporate behavior postulated by Krzyzaniak and Musgrave would have resulted not in successful tax shifting, but in subsequent union wage demands, an inflationary spiral, and an *unchanged* pre-tax corporate rate of return.

The incidence of the corporation income tax is determined by the effect of the tax on the corporate income share. Corporations suffer the full burden of the tax unless they can increase their pre-tax income share in response to a tax rate increase. In their second chapter Krzyzaniak and Musgrave were puzzled by the apparent lack of shifting when measured relative to the corporate income share. They were unable to conclude that the burden of the corporation income tax had been shifted from capital to labor, because their high shifting parameter of more than 100 per cent was a measurement relative to the corporate rate of return, not the corporate income share. The changing productivity of both labor and capital during the period prevents any conclusion from rate-of-return data on the extent to which labor suffered the burden of the corporate tax. The inconsistency between the rate of return and income share measures of tax shifting in the Krzyzaniak and Musgrave study was due to an inadequate specification of the determinants of the rate of return on assets.

The present study contributes to clearing up this mystery which Krzyzaniak and Musgrave were unable to solve. The model of markup pricing behavior specifies productivity as an input, and as a result consistent tax shifting coefficients are obtained relative to *both* the corporate rate of return and the income share. This paper thus offers a general parameter measuring the incidence of a tax burden on factor shares, and concludes that between 1925 and 1962 corporations suffered the full burden of the tax.

This study also points out a weakness in the recent study by Kilpatrick [17]; evidence that changes in profits are related to market power does not imply that on the average in manufacturing the corporate tax is shifted. Markup pricing behavior does not necessarily involve forward tax shifting, because if businessmen are operating at what they think is an optimal price-output combination, the mere introduction of a profits tax will not change this combination. We found that industries with high concentration ratios tend to have higher tax-shifting parameters than less concentrated industries, but this result can be interpreted merely to mean that during the last 40 years, a period of rising tax rates, the rate of return of firms in concentrated industries where barriers to entry are high has risen relative to industries where the rate of return is more subject to the depressing effects of entry.

No conclusions are reached in this study about the degree of *long-run* tax shifting. Even if, as concluded here, corporations do not raise prices

to shift the tax in the short run, they may nevertheless be able to shift the tax in the long run by reducing their investment and substituting other factors for capital. A reduction in the desired ratio of capital to output implies an increase in the average productivity of capital, which is shown in this study to be a major determinant of the pre-tax rate of return on assets. If it could be demonstrated that the increased corporation income tax rate since the 1920s has been the principal cause of the increased average productivity of capital over our period, this would suggest that the tax has been shifted in the long run.²¹ A final conclusion on this issue must await a thorough study of the causes of the increase in the average productivity of capital between the 1920s and 1950s, taking account not only of changes in tax rates, but also of the rate and bias of technological change, the change in the price of capital goods relative to output, changes in the industrial composition of demand, and other factors.²²

²¹ But evidence that the higher tax rate reduced investment would not necessarily change our conclusion about the incidence of the tax—that corporations have suffered the full burden. A reduction in investment would have increased the rate of return, but only at the cost of a smaller capital base relative to sales. For corporations to have escaped some of the burden of the tax, the pre-tax corporate income share would have had to rise, which would have occurred only if the aggregate production function were characterized by an elasticity of substitution smaller than one. If this were true, the boost to the income share induced over our period by the higher tax rate would have resulted in significantly positive coefficients on the tax parameter in the income-share equations of Table 1, but in fact the coefficients are not significantly different from zero.

²² The causes of changes in the capital-output ratio over this period are the subject of the author's current research. There are at least two pieces of evidence which suggest that the corporation income tax rate has not been the main cause of the increase in the average productivity of capital since 1920, and thus that the tax has not been substantially shifted in the long-run. First, most of the increase in R^*/K in this study occurs before 1940, while the main increase in the tax rate occurred after that date. This sequence would have to be reversed for long-run tax shifting to have occurred.

Second, a calculation has been made to determine the effect of changes in the corporation income tax rate on investment for a profit-maximizing firm, using the investment theory developed by Jorgenson [14] [15]. In this theory the desired stock of capital and hence desired investment is influenced by the tax through the effect of the latter on the real price of capital services. The real price of capital services can be shown to be independent of the corporation income tax rate if there is no accelerated depreciation and if the pre-tax interest rate is unchanged by the tax.

Consider the following version of equation (6) in the recent paper by Hall and Jorgenson [9], adapted here to allow for tax shifting, to exclude the tax-credit term, and to state explicitly that r is the pre-tax interest rate.

$$(30) \quad \frac{c}{p} = \frac{(1 - wvz)q}{(1 - wv)p} [r(1 - wv) + \delta]$$

c/p is the real price of capital services, q/p is the real price of investment goods, v is the tax rate, z is the present value of the depreciation deduction, r is the pre-tax interest rate, δ is the depreciation rate, and w is a tax-shifting parameter ($w=0$ for full shifting and $w=1$ when shifting is completely absent, thus $w=1-\alpha_s$). A calculation of c/p for (30) with data similar to that used in [9] shows that c/p is the same for all values of w between 0 and 1.3. Thus this calculation casts doubt on the usual assumption that a corporation income tax which is not shifted in the short run necessarily reduces investment.

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DATA APPENDIX

A. Financial Series from *Statistics of Income* [25].

1. "Total assets," K_t . Includes fixed assets, inventories, and liquid assets.
2. "Gross sales," R_t . This figure should not be confused with Gross Receipts, which includes receipts from sources other than sales.
3. "Depreciation, depletion, and interest paid," D_t .
4. "Net profits before taxes," π_t^e . Includes all corporations, including those reporting no net income.

B. Constructed Series.

1. "Total apparent tax rate," v_t . Sometimes, particularly in 1941 and 1950, tax rates were raised in the latter part of the year, but corporations were liable for payment at the increased rate for their profits of the entire year. Thus the *ex post* ratio of tax liabilities to taxable profits is an overestimate of the tax rate which management *thought* was in effect at a given time. To correct this, our "apparent" tax series is an average of the legal tax rates for each year, weighted by the proportion of the year over which they were in effect. Also, we can't consider the legal excess profits tax rate (used by Krzyzaniak and Musgrave) to reflect the actual burden of the tax on all corporations in an industry, since not all corporations earned excess profits. The excess profits tax rate is multiplied by an estimate of the ratio of corporate profits subject to the tax to all corporate profits.

2. "Wholesale price index," p_t . This was a difficult series to construct, because the reported Industry subgroups in the BLS wholesale price index have changed repeatedly, and because the weights necessary to combine them to correspond to our industry classification have changed over time. A detailed documentation of the methods and weights used for each industry is available on request.

3. "Hybrid real output in constant dollars," Q_t . Since the Index of industrial production (I.I.P.) and the Wholesale price index (W.P.I.) were computed in completely different ways with different industry definitions and differing assumptions about quality and productivity change, it is not surprising to find that the I.I.P. and Sales deflated by W.P.I. show different trends in the growth of real output for some industries. I selected deflated Gross sales as the better measure of the trend of real output, but I

lean to I.I.P. as the better indicator of cyclical variation, particularly in some of the postwar recessions. To achieve the best compromise a hybrid measure was constructed, which equals deflated Gross sales in selected base years, but the movements of which in nonbase years are proportional to I.I.P.

4. Capacity series, R_t^* . Any attempt to construct capacity and utilization series by industry is bound to be based to a considerable extent on guesswork. In previous studies the general procedure in computing capacity has usually been to take a given year, to assume that utilization was 100 per cent during that year, and that capacity output grew at a fixed trend rate between one capacity year and another. We cannot use existing utilization indices for this study since they extend back only to 1947, and in addition the usual procedure needs to be refined somewhat to take account of the fact that utilization is not the same in all "full capacity" years, and of the probability that the growth of capacity between base years is uneven.

A very brief description of the procedure is as follows (a more complete documentation is available from the author on request). My first step was to select years during which utilization probably reached a cyclical peak. The years chosen for each industry were different, but they had the common characteristic that output was rising rapidly to a plateau substantially higher than previous output peaks, while real capital was rising relatively slowly. Next, for each industry I guessed a number between .90 and .98 as the average utilization rate during each peak year. Outside evidence, e.g., the monthly pattern of output within the year and comment in contemporary business periodicals, was used as a guide in the selection process. This procedure results in a capacity figure for each industry for each year of peak utilization.

Finally, the change in capacity between years of peak utilization is not assumed to be linear as in many previous studies of postwar utilization. The annual rate of growth of capacity equals the rate of growth of real fixed capital in each year plus the rate of growth of the ratio of capacity to fixed capital from the previous year of peak utilization to the next.

The data for real fixed capital are book value figures from [25] for each industry, adjusted for changes in the definition of capital and deflated by an unpublished Department of Commerce implicit price index for the stock of fixed capital in manufacturing.

Capacity in current dollars, R_t^* , equals real capacity as calculated above times p_t .

THE ROLE OF A STOCK MARKET IN A GENERAL EQUILIBRIUM MODEL WITH TECHNO- LOGICAL UNCERTAINTY

By PETER A. DIAMOND*

Moving from the certainty model of general equilibrium to one with uncertainty, the constructors of the model¹ with contingent commodity markets have given considerable insight into the problems of uncertainty for an economy. However, the construction of perfect insurance markets, where all risks are insurable, does not lend itself well to understanding the economy's imperfect solution of these problems. The model presented here is an example of an imperfect solution where a stock market permits the sharing of risks, but only incompletely. This will permit examination of the handling of risks by such an economy, as well as the shortcomings of this handling. An important element in any model involving uncertainty is the source of probabilities used in decision making and the ultimate location of risk-bearing. In the contingent commodity model, firms bear no risks in the sense that no probability distributions are needed to calculate firm decision rules or profits accruing to firm owners, and all risks are borne by consumers. With a stock market, however, where shareholders receive residual profits after the completion of production under technological uncertainty, firm owners do bear risks and need to use their subjective probabilities in determining their portfolio structures. Paralleling the contingent commodity model, however, firms planning production to maximize stock market value employ no probabilities in their calculations, relying instead on determinate, concurrent market phenomena, the prices inherent in the stock market.

By means of a one-period one-commodity model it is shown that firm behavior based on this rule, combined with trading in the stock market to maximize expected utility, results in a competitive economy's achieving a constrained Pareto optimum. The constraint does not arise from an inability of individual firms to use the pooling advantages implied by the law of large numbers, for with stock market value maximization by firms and portfolio diversification by wealth owners, all of the economy's

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¹ The contingent commodity model of an exchange economy is presented in Arrow [1]. The model with production is in Debreu [4].

averaging possibilities are brought to bear on each production decision. Rather, the constraint, that consumption patterns across the states of nature be linear combinations of firm output patterns, arises from an inability to distinguish finely among the states of nature in the economy's trading.

The highly restrictive one-period one-good model is not employed simply for heuristic purposes, for an increase to two periods or goods in a seemingly natural way (the stock market reopening at the start of the second period or commodities traded after the completion of production) introduces a different element from technological uncertainty-price uncertainty. In the presence of only technological uncertainty, current trading on the stock market does not affect future probabilities of the states of nature. Nor does it convey information about those probabilities. Thus, in this model, individuals have subjective probabilities at the start of trading which are not altered. However, with price uncertainty current trading does affect future prices and does convey information about the tastes of other consumers which is relevant for subjective probabilities of future prices. It then becomes necessary to consider the extraction of this information, the alteration of subjective probabilities, and the change of market demands in the process of trading. These problems do not arise in the simple model considered. Thus this model is static in the important sense that all trading occurs at one time. (Both the certainty and contingent commodity uncertainty general equilibrium models are static in this sense, as is any perfect foresight model.) The model is static also in the sense that no one learns anything which affects his decisions. (Prices are reacted to, but this is not taken to convey new information which, for example, might lead to a change in subjective probabilities.)

Following a description of the technology is an examination of a centrally planned economy with restricted planning abilities, which therefore achieves a constrained Pareto optimum. In this way the conditions for the constrained optimum are obtained. It is then shown that a competitive equilibrium satisfies these conditions. The validity, in this model, of the Modigliani-Miller propositions is then examined, as well as the treatment of uncertainty for government investment. The appendix contains a brief description of the contingent commodity model in order to present the analogy between production decisions in the two models.

I. *States of Nature*

To introduce technological uncertainty, it is assumed that production functions of the individual firms can be written as functions of the input and a random variable reflecting the state of nature.² Thus the

² In addition to the assumption of a single input and a single output, it is assumed that the

output of the j th firm, if state θ occurs, is a function of the input level to the j th firm³ (which is independent of θ) and the state of nature. Algebraically,⁴

$$(1) \quad y_j(\theta) = f^j(\theta, k_j).$$

Thus, for example, the output of corn if θ inches of rain fall depends on the number of inches of rain and the input of seed. The pattern of output of the j th firm across the states of nature, $y_j(\theta)$, can be thought of as a joint product resulting from the input level k_j . In the special case where the production function is decomposable,

$$(2) \quad y_j(\theta) = f_j(\theta)g_j(k_j),$$

the output pattern (suitably normalized) can be considered a composite commodity since its individual elements are produced in fixed proportions. In this case, a change in input level alters the scale of output but not the pattern of output across the states of nature. It is assumed that the set of firms is fixed for the analysis. This eliminates not only searching for new investment opportunities but also the creation of insurance firms (although existing insurance firms can be fitted into the model [see below, section 5]).

Each consumer is assumed to have his own subjective probability density over the states of nature, $h_i(\theta)$, and a utility function of consumption,⁵ $U_i(c_i)$, and to be interested in maximizing expected utility,⁶

$$(3) \quad V_i = \int U_i(c_i(\theta))h_i(\theta)d\theta.$$

It is assumed that after its occurrence the state θ which has occurred is known by all. In an insurance setting, this rules out some of the prob-

states of nature can be described by a single, continuous random variable. While the assumption of knowledge of the relationship of the random variable to output is restrictive, the ability to parametrize the states of nature on a single variable is not important.

³ Additional inputs contracted for before the state of nature is known add no complication. However, inputs hired after θ is known (e.g., labor to harvest the crop) involve trading more than once, implying price uncertainty, and thus do not fit within this model. Inputs employed after the state of nature is known but hired on a contingent basis before it is known would simply represent the addition of particular contingent commodity markets to the existing stock market. This creates no complication.

⁴ It is assumed that f^j is twice continuously differentiable for all j , with $f_{k^j}^j > 0$, $f_{k^j k^j}^j < 0$.

⁵ It is assumed that U_i is twice continuously differentiable for all i , with $U_i' > 0$, $U_i'' < 0$.

⁶ All integrals are assumed to exist. Writing expected utility in this form (additivity vis-à-vis consumption in the different states of nature with the same utility function, except for a scalar) is highly restrictive, ignoring, for example, the impact of rain on the utility derived from an umbrella. Dropping this assumption alters no results in the text, but does alter the descriptive elements in the appendix.

lems arising from moral hazards. (For a discussion of the informational aspects of insurance, which are lacking in the model, see Arrow [2].)

II. Centrally Planned Economy

Before examining a centrally planned economy, it is necessary to consider the expression of its objectives. A natural extension of the certainty concept is to define Pareto optimality in terms of the expected utilities, V_i . This implies that the planner uses different probabilities in calculating the expected utilities of different individuals, since, in general, the h_i vary among individuals.^{7,8}

If a central planner with control over all economic variables, k_i and $c_i(\theta)$, seeks a Pareto optimum, he will produce an allocation which could be achieved by a competitive economy with a complete set of contingent commodity markets (discussed in the Appendix). The cost elements which limit the set of markets in a competitive economy might also limit the degree of control exercised by a planner. So we shall consider a planner with somewhat limited powers, chosen in a manner so that a competitive economy with a stock market can achieve the same allocation. Thus, by examining the restrictions on a planner which result in the same allocation, we can gain some insight into the limitations inherent in a stock market.

We shall continue to assume that the planner has full control over allocation of the input, but we shall lessen his control over the distribution of output at the completion of production. We shall assume that all distribution instructions are given before production occurs and that they take one of the following two simple forms. Firms are instructed to deliver to given individuals given fractions of their output independent of the state of nature. Let α_{ij} be the fraction of its output the j th firm delivers to the i th individual.⁹ Individuals are instructed to

⁷ This is the concept employed by Arrow [1] and Debreu [4].

⁸ The social welfare function which fits with this definition of Pareto optimum, in the sense that a Pareto optimum is a necessary condition for maximizing the welfare function, is a function of the levels of expected utility. Algebraically we have $W(V_1, V_2, \dots, V_m)$.

An alternative approach to extending the certainty concept of social welfare is to calculate expected social welfare (using a probability distribution chosen in some fashion) where welfare is a function of achieved utilities. This would be written as $\int W(U_1(c_1(\theta)), U_2(c_2(\theta)), \dots, U_m(c_m(\theta)))h(\theta)d\theta$. In general this approach is not consistent with the definition of Pareto optimality given above. (While this approach does not require knowledge of h_i it still requires knowledge of U_i).

Each of these simplifications is well constructed for some of the aspects of social welfare. The latter can take account of the costs, e.g., through envy, in the spread of achieved utilities. The former, however, allows for a concept of justice in terms of expected utilities relative to θ . For this reason the former concept is preferred, and the definition of Pareto optimality given above, which yields optimality results similar to those under certainty, will be employed in calculating efficiency conditions.

⁹ The constraint $\alpha_{ij} \geq 0$ are assumed not to be binding in the maximization below (i.e., everyone receives something from each firm). It is assumed that there are no delivery costs. Further, the constraint $c_i(\theta) \geq 0$, which implies a constraint on α_i is also assumed not to be binding.

deliver to other individuals fixed quantities of output independent of the state of nature. Let α_i be the net quantity the i th individual receives in this fashion. The planner then controls k_j , α_{ij} , and α_i .

These restrictions imply that consumption of the i th individual in terms of the control parameters can be written

$$(4) \quad c_i(\theta) = \alpha_i + \sum_{j=1}^n \alpha_{ij} f^j(\theta, k_j).$$

This, then, is the linearity constraint, that consumption patterns be a linear combination of output patterns, i.e., that consumption be the same linear combination of firm outputs in all states of nature. Using equation (4) for all individuals but the first, the constraint that total consumption equal total output can be expressed by writing the consumption of the first consumer as

$$(5) \quad c_1(\theta) = - \sum_{i=2}^m \alpha_i + \sum_{j=1}^n \left(1 - \sum_{i=2}^m \alpha_{ij} \right) f^j(\theta, k_j).$$

Thus the net flows between individuals add to zero and the fractions of each firm's output received add to one.

With these constraints on consumption patterns and the constraint on available input supply,

$$(6) \quad \sum_{j=1}^n k_j = k,$$

we can express the conditions for a Pareto optimum by maximizing expected utility of the first individual

$$\int U_1(c_1(\theta)) h_1(\theta) d\theta$$

subject to constraints on the expected utility levels of the other $m-1$ consumers

$$(7) \quad \int U_i(c_i(\theta)) h_i(\theta) d\theta = \bar{V}_i.$$

Substituting for $c_i(\theta)$ from equations (4) and (5) and forming the Lagrangian expression,

$$(8) \quad \int U_1 \left(- \sum_{i=2}^m \alpha_i + \sum_{j=1}^n \left(1 - \sum_{i=2}^m \alpha_{ij} \right) f^j(\theta, k_j) \right) h_1(\theta) d\theta \\ + \sum_{i=2}^m \lambda_i \left(\int U_i \left(\alpha_i + \sum_{j=1}^n \alpha_{ij} f^j(\theta, k_j) \right) h_i(\theta) d\theta - \bar{V}_i \right) + \mu \left(k - \sum_{j=2}^n k_j \right),$$

we can calculate the first order conditions. Differentiation with respect to α_i and α_{ij} gives

$$(9) \quad \begin{aligned} - \int U'_1 h_1 d\theta + \lambda_i \int U'_i h_i d\theta &= 0, & i = 2, 3, \dots, m, \\ - \int U'_1 f^j(\theta, k_j) h_1 d\theta + \lambda_1 \int U'_1 f^j(\theta, k_j) h_1 d\theta &= 0 & \begin{matrix} i = 2, 3, \dots, m, \\ j = 1, 2, \dots, n. \end{matrix} \end{aligned}$$

Eliminating λ_i from the equations we have $(m-1)n$ equations

$$(10) \quad \frac{\int U'_i f^j(\theta, k_j) h_i d\theta}{\int U'_i h_i d\theta} = \frac{\int U'_1 f^j(\theta, k_j) h_1 d\theta}{\int U'_1 h_1 d\theta}, \quad \begin{matrix} i = 2, 3, \dots, m, \\ j = 1, 2, \dots, n. \end{matrix}$$

Recalling the interpretation of the output pattern of a firm over states of nature as a joint product, equation (10) expresses the equality among consumers of the marginal rates of substitution for these joint products, where the marginal rate of substitution is expressed in terms of expected marginal utility.

Differentiation of (8) with respect to k_j gives

$$(11) \quad \int U'_1 \left(1 - \sum_{i=2}^m \alpha_{ij} \right) f_k^j h_1 d\theta + \sum_{i=2}^m \lambda_i \alpha_{ij} \int U'_i f_k^j h_i d\theta = \mu, \\ j = 1, 2, \dots, n,$$

where f_k^j is the marginal product of the input (and varies with θ). Eliminating λ_i from the equations (11) we obtain

$$(12) \quad \frac{\int U'_1 f_k^j h_1 d\theta}{\int U'_1 h_1 d\theta} + \sum_{i=2}^m \alpha_{ij} \left[\frac{\int U'_i f_k^j h_i d\theta}{\int U'_i h_i d\theta} - \frac{\int U'_1 f_k^j h_1 d\theta}{\int U'_1 h_1 d\theta} \right] = \frac{\mu}{\int U'_1 h_1 d\theta},$$

where the right-hand side is independent of θ , since θ appears solely as a variable of integration.

In the special case of a decomposable production function, equation (2), we have

$$f_k^j(\theta, k_j) = f_j(\theta) g_j'(k_j).$$

Thus the equality of marginal rates of substitution, equation (10), implies that all the terms on the left-hand side, except the first, vanish. Eliminating μ we have the allocation rule

$$(13) \quad \frac{g'_j}{g'_i} = \frac{\int U'_1 f_1 h_1 d\theta}{\int U'_1 f_j h_1 d\theta}, \quad j = 2, 3, \dots, n.$$

With the interpretation of output patterns as composite commodities, this expresses an equality between the marginal rate of transformation and the marginal rate of substitution.

III. *Stock Market Equilibrium*

Let us now consider a competitive economy with the same technology as above. The initial conditions are the ownership of the supplies of input and the shares of stock of the firms. Let w_i be the supply of input and w_{ij} the fraction of the j th firm owned by the i th consumer. Simultaneously trading occurs to reach an equilibrium in the factor market, the stock market, and a bond market, not yet introduced. At the completion of trading, each firm produces with the factors hired under whatever state of nature occurs. Factors are then paid their wages (the wage contract, although fulfilled after production, is set in real terms independent of the state of nature). Let r be the payment per unit for the factor. The remaining profits, which if state θ has occurred, are

$$(14) \quad \pi_j(\theta) = f^j(\theta, k_j) - rk_j,$$

are then distributed to the owners in proportion to their ownership of stock.

Denoting the value of the i th individual's equilibrium holdings of the stock of the j th firm by s_{ij} ,¹⁰ the total stock market value of the j th firm, M_j , can be written

$$(15) \quad M_j = \sum_{i=1}^m s_{ij}.$$

The fraction of the firm owned by the i th consumer is then s_{ij}/M_j and the consumption goods he receives from the j th firm are

$$(16) \quad s_{ij} M_j^{-1} \pi_j(\theta).$$

Thus, s_{ij}/M_j is the equilibrium fraction of the firm owned, while w_{ij} was the initial fraction. With this framework, stockholders need to know

¹⁰ It is assumed that the constraint $s_{ij} \geq 0$ is not binding (i.e., everyone owns stock in each firm). This constraint, and the corresponding one for the planned economy can be dropped without altering the basic optimality results, although the analysis becomes more complicated. However, the Modigliani-Miller results may be affected by binding constraints on financial behavior.

the relationship of θ to profits although only entrepreneurs need to know the relationship between inputs and output which determines this relationship. In a certainty model firm owners know the level of profits, again without needing to know the combination of inputs and outputs resulting in the profit. This increase in information required for the result, while a natural extension is a significant increase.

In addition to the stock market let us assume there is a bond market where individuals trade promises to pay fixed amounts of consumption, independent of the state of nature, at the completion of the production process. It is assumed that all bond commitments and commitments to the factors of production, are met (bankruptcy never occurs). For convenience let us define one unit of bond as a promise to pay r units of consumption where r is the factor wage. Then, an individual receives fixed payments for both the input he owns and the bonds he holds. Let b_i be the sum of these two elements. We can now write the expression for consumption of the i th individual as

$$(17) \quad c_i(\theta) = rb_i + \sum_{j=1}^n s_{ij}M_j^{-1}\pi_j(\theta).$$

By trading on the stock and bond markets, the consumer seeks to select b_i and s_{ij} to maximize expected utility,

$$(18) \quad \int U_i \left(rb_i + \sum_{j=1}^n s_{ij}M_j^{-1}\pi_j(\theta) \right) h_i(\theta) d\theta$$

within his budget constraint,

$$(19) \quad b_i + \sum_{j=1}^n s_{ij} = w_i + \sum_{j=1}^n w_{ij}M_j,$$

that the value of bonds purchased, $b_i - w_i$, plus stocks purchased equal the value of his initial stock holdings (the bond is employed as numeraire, making the number of bonds equal their value). The consumer acts as a competitive trader in that he views r , M_j , and $\pi_j(\theta)$ as parameters.

Substituting from the budget constraint for b_i in the expression for expected utility, we have an unconstrained maximization with respect to s_{ij} which has the first order conditions

$$(20) \quad \int U'_i(-r + M_j^{-1}\pi_j(\theta))h_i(\theta)d\theta = 0, \quad j = 1, 2, \dots, n.$$

Rearranging terms, we can express this as an equality between relative prices (recalling that the bond is numeraire, with its price equal to one)

and the marginal rate of substitution of the joint product of consumption across the states of nature obtained by buying the stock of the j th firm for the joint product obtained by buying a bond.

$$(21) \quad M_j = \frac{\int U'_i \pi_j(\theta) h_i(\theta) d\theta}{\int U'_i r h_i(\theta) d\theta} .$$

Substituting the definition of profits, equation (14), and rearranging terms we obtain a similar expression for the output pattern of a firm to that we had for its profit pattern.

$$(22) \quad M_j + k_j = \frac{\int U'_i y_j(\theta) h_i(\theta) d\theta}{\int U'_i r h_i(\theta) d\theta} .$$

The step between equations (21) and (22) is analogous to the Modigliani-Miller theorem (see [6]) that the total market value of a firm is independent of its debt-equity structure.¹¹ The interpretation of this step is that the market value of output, $M_j + k_j$, equals the market value of profits, M_j , plus the value of factor payments, k_j . Recalling the normalization of bonds in terms of factor payments we can view the factor payment obligation as an ordinary element of debt. If we consider two firms, of varying efficiency, so that they have the same output stream $y_{j_1}(\theta) = y_{j_2}(\theta)$ but different input levels, $k_{j_1} \neq k_{j_2}$, we can see from equation (22) that their output value must be the same, although stock market profit value will differ by the difference in present value of their future factor payments. Equivalently we could consider two firms with the same inputs and outputs, differing in that one was in debt. Then the value of debt plus market value for one would equal the market value of the other. Further we can see that a single firm could sell equities to purchase debt (e.g., to pay factors in the future) without altering the real equilibrium position since equilibrium is preserved if each stockholder preserves his fraction of the firm owned and goes into debt to finance his increased shareholdings. This result is a consequence of competitive equilibrium with price takers facing the same prices. The fact that the firm and all its stockholders can lend and borrow on exactly the same terms preserves this condition.

¹¹ Note that this result is independent of the fact that subjective probabilities differ among individuals. This is in contrast to Lintner [5].

IV. *Production Decisions*

Let us consider, to begin, a firm that has a decomposable production function (as in equation (2)). The fact that the market value of profits equals the market value of output less that of input payments simplifies the calculation for the price-taking firm of the market value corresponding to any level of production. If, for example, the firm is considering doubling its input, this would be calculated as doubling the value of input payments (since the firm is a price taker). The output pattern remains unchanged by the assumption of a decomposable production function, the firm's output becoming $f_j(\theta)g_j(2k_j)$ instead of $f_j(\theta)g_j(k_j)$. Acting as a price taker the firm calculates that its output value will increase in proportion to output, to

$$\frac{g_j(2k_j)}{g_j(k_j)} (M_j + k_j)$$

and thus that its stock market value would become

$$\frac{g_j(2k_j)}{g_j(k_j)} (M_j + k_j) - 2k_j.$$

In general, at an input level of \hat{k}_j with a market value of \hat{M}_j , the firm would calculate market value at an alternative input level as

$$(23) \quad M_j = \frac{g_j(k_j)}{g_j(\hat{k}_j)} (\hat{M}_j + \hat{k}_j) - k_j,$$

and would plan to carry production to the point where the derivative of M_j with respect to k_j equalled zero. In equilibrium, this derivative is calculated at the equilibrium input level ($\hat{k}_j = k_j$). Thus the equilibrium condition for production is

$$(24) \quad \frac{g'_j(k_j)}{g_j(k_j)} (M_j + k_j) = 1.$$

Substituting from equation (22) for $(M_j + k_j)$ in this equation we have

$$(25) \quad \frac{g'_j \int U_j f_j(\theta) h_i(\theta) d\theta}{\int U'_i r h_i(\theta) d\theta} = 1.$$

Comparing these equations, (22) and (25), with the allocation rules in the planned economy, equations (10) and (13), we see that they are

equivalent and subject to the same interpretations given above. Thus in the case of decomposable production functions the competitive equilibrium is a constrained Pareto optimum.

In this special case of decomposable production functions, we can consider the output pattern of each firm, suitably normalized, as a commodity. Both production and utility functions can be expressed in terms of these commodities, making this model equivalent to a general equilibrium model under certainty. It is, then, not surprising that we can obtain the standard optimality implications of a competitive equilibrium, with optimality holding relative to these composite commodities. The restriction to these commodities is, of course, the linearity constraint resulting in a constrained rather than full optimum.

Dropping the decomposability assumption, it remains true that the price-taking firm wants to carry production to the point where the derivative of output value (in the stock market) with respect to input level equals one. In the decomposable case, the marginal output pattern and the total output pattern are identical (i.e., for some α , which is a function of k ,

$$f_k^j(\theta, k) = \alpha f^j(\theta, k)$$

for all θ) and the firm can obtain the appropriate prices for its marginal calculations from its own stock value. In the absence of decomposability, this price might be found in the value of some other firm (or combination of firms) if the marginal pattern of the first firm coincided with the total pattern of the second at the equilibrium input levels. Thus if at the equilibrium values k_{j_1} and k_{j_2}

$$(26) \quad f_k^{j_1}(\theta, k_{j_1}) = \alpha f^{j_2}(\theta, k_{j_2})$$

for all θ , the equilibrium condition for firm j_1 is

$$(27) \quad \alpha(M_{j_2} + k_{j_2}) = 1.$$

Note that exactly the same patterns of output are required for this calculation. Two firms with the same expected return and the same variance (using someone's subjective probability) will not, in general, carry production to the same point.

It would be surprising if firms were capable of carrying out this calculation exactly. However, we might expect them to approximate it reasonably well if the major risks are decomposable into particular types which are fairly common. For example, a firm may find its marginal risks simply related to the weather and a random variable independent of anything else in the economy (and, in some extension to price uncer-

tainty, also to the business cycle, which is not, of course, a technological uncertainty). I have not found a rule for competitive production in the general case when the firm is unable to price its marginal output stream from the average profit streams valued in the market.¹²

V. *Implications*

It is interesting to contrast the allocation in the planned economy with that arising from the simple policy of dividing up output equally at the completion of production. By dividing up claims equally before production, and permitting trade in these claims, consumers are able to raise their expected utility levels, as they view them, by means of trade among persons of differing risk aversion or different subjective probabilities. Secondly, we note that even if the planner uses his own subjective probabilities, the presence of technological uncertainty which varies among investment opportunities requires knowledge of consumer risk aversion for production decisions.¹³ This is analogous to the certainty model where decisions about possibilities involving different goods require information about tastes for these goods. Levels of risk aversion can be learned from market information only if consumers are offered risky alternatives. A stock market is one method of acquiring this information. In the absence of contingent commodity markets, perhaps socialist economies should have stock markets (possibly with stock on a leasehold not freehold basis, consumers owning claims to output with ownership of the firm reverting to the state at set time intervals).

¹² J. A. Mirrlees has raised the issue of exact achievement of the constrained optimum by nonmarket mechanisms. One such mechanism is a stockholders meeting, run in a particular way. Let the firm ask stockholders how much they are just willing to see the firm pay for the marginal unit of input. Each stockholder will answer by calculating the change in expected utility from a change in input as a function of the wage paid the input, ρ , and then equate this with zero. The expression is

$$\int U_i s_{ij} M_j^{-1} (f'_k(\theta, k_j) - \rho) h_i(\theta) d\theta = 0$$

or, solving for ρ , the wage equals

$$\frac{\int U'_i f'_k h_i(\theta) d\theta}{\int U'_i h_i(\theta) d\theta}.$$

If production is carried to the point where the wage equals the average of these answers, with the fractions of the firm owned serving as weights in the averaging, the optimal allocation rule, equation (12), is achieved.

¹³ Note that a stock market affects the allocation of all inputs into a risky production process (not just capital) since varying output patterns arising from varying input combinations have different stock market valuations. For example, substituting labor for fertilizer will, in general, alter the pattern of output across the states of nature.

Now let us turn to the Modigliani-Miller propositions (see [6]) that firms with the same output patterns have the same expected return on total capital (i.e., the value of debt plus equity, or in this case the present value of contracted factor payments plus stock market value). In turn this rate is the appropriate expected return for the margin of investment. The first proposition holds in general (following from expected utility maximization) while the second requires the decomposability assumption (and follows, then, from market value maximization by the firm). Both conclusions hold for any probability function. The condition of identical output patterns is that as functions of θ

$$y_1(\theta) = \alpha y_2(\theta)$$

for some constant α . From the conditions for expected utility maximization, equation (22), we have

$$(28) \quad (M_1 + k_1)^{-1} \int U'_i y_1(\theta) h_i(\theta) d\theta = (M_2 + k_2)^{-1} \int U'_i y_2(\theta) h_i(\theta) d\theta$$

or, upon substitution

$$(29) \quad (M_1 + k_1)^{-1} \alpha = (M_2 + k_2)^{-1}.$$

The expected return on total capital is

$$(30) \quad (M_j + k_j)^{-1} \int y_j(\theta) h(\theta) d\theta.$$

Substituting for both returns and output, we have the equality of expected returns. Note that the same output pattern, and not just the same variance, is required for the conclusion of equal expected returns.

Similarly, the market value maximization equation, (24), implied that the expected marginal return equals the expected average return. However, this result depends on the identical pattern of total and marginal returns and does not hold without the decomposability assumption.

By means of portfolio diversification on the part of consumers, all of the economy's averaging possibilities are brought to bear on each production decision. Thus, in the model, large firms have no pooling advantages over small firms, nor does the government have an advantage over private business.¹⁴ Provided that the government does not go

¹⁴ This conclusion does not depend on the assumption that fixed interest debt obligations (or equivalently wages) are met in every state of nature. Bankruptcy for limited liability corporations still leaves well defined payment patterns for both stocks and bonds. Provided both of these are traded competitively (preserving the assumption that an internal equilibrium exists or alternatively that individuals can sell short) the basic conclusions of the model are preserved. It is true that large firms, with diversified investments, will tend to go bankrupt in fewer states of nature and so will pay a lower nominal rate for fixed interest debt. But this does not alter

outside the linearity constraints of the model, government investors should imitate private business, evaluating individual projects separately, not pooling, and looking to the stock market for the appropriate return criteria. Given market value maximization, average stock market returns reflect marginal returns to investment. Neither, in general, reflects average returns to a firm's real assets.

While some examples of government investment may fall neatly into the pattern of the model (for example, pure public goods provided equally to everyone), many of them will not. Presumably some government activity may be devoted precisely to circumventing the linearity constraint where the constraint imposes a large cost on the economy. (An analysis explaining the particular set of trade opportunities available may well relate a limit on the cost of this constraint to the costs of establishing further markets.) Also, parallel to government activity in a certainty model, some actions may be taken to correct divergences from the competitive model which interfere with the optimality properties of competitive equilibrium. Where government investment is not necessarily linked to either of these aspects it seems reasonable to expect optimal behavior to be similar to that for investment falling within the pattern of the model. For example, optimal investment with nonlinear distribution rules, unless these rules are chosen to correct limitations from linearity in the private economy, may on average involve expected returns approximating those on investments with linear distribution rules.¹⁵

The model assumed a given set of firms, chosen historically to reflect investment opportunities. Relaxing this assumption to permit the addition of new firms allows the introduction of a competitive insurance market.¹⁶ This, however, raises even more seriously the problem of the informational aspect of the model and the empirical distinction among states of nature. Nevertheless, in the context of the model, by defining a pattern of output across the states of nature, trading can occur in stocks without the necessity of production behind them. This trading is neces-

the conclusion that large firms and small firms should pursue identical investment opportunities to the same margin. Various market imperfections would naturally alter this conclusion. Taxes, transaction costs in debt issuance, and difficulty for financial investors in obtaining information about the nature of real investment opportunities, $y_i(\theta)$, are obvious candidates for this disruption.

¹⁵ The fact that benefits from government investment may not be tradable may decrease their social value.

¹⁶ Another opportunity for an increase in efficiency is by the creation of two firms dividing the profits of an existing firm nonlinearly between them. This increases the number of possible patterns available to consumers. Mutual funds, however, offer no new opportunities in a perfect knowledge, no transaction cost model.

sarily buying and short selling, where selling is a promise to pay the appropriate dividend level. For example, a pattern involving payment in case of a flood in a given area, but not otherwise, is a stock which will be bought by residents of the area and sold by nonresidents. Thus, competitive insurance markets could be introduced. In the absence of all transaction costs, this could be carried to the point where it would become possible to purchase, by some combination of stocks, a unit of output in any state of nature, i.e., the set of output patterns would span the states of nature. At this point the stock market model becomes equivalent to the contingent commodity model discussed in the Appendix and the economy achieves a full Pareto optimum.

APPENDIX: CONTINGENT COMMODITY MARKETS

As mentioned earlier, the general equilibrium model with a stock market developed above is, in many ways, extremely analogous to the general equilibrium model with contingent commodity markets. Apart from the differences induced by the different market structures, individual behavior is essentially the same in the two models. In both models all trading occurs at one time. Again, consumers maximize expected utility, in one case by purchasing the joint products of patterns of consumption across the states of nature, in the other by purchasing consumption in each state of nature separately. In both cases factors of production are paid fixed amounts and stock market value is maximized in determining the extent of employment. (As in the certainty model, in the contingent commodity model the firms maximize profits, which are determinate, not random, but the level of determinate profits is precisely the value of the firm to its owners.) The similarities of production are such that, in terms of a consumer's utility function, rules for allocation of resources among firms and evaluation of firms are the same in both models (in the case of decomposable production functions, which is the only well-defined case with the stock market economy). The differences in consumption patterns arising from the limitation on purchases in a stock market model do, however, imply different allocations (as well as the difference that the stock market economy achieves only a constrained optimum while the contingent commodity economy achieves a full optimum). We shall return to these parallels below after describing the contingent commodity model.

With a system of contingent commodity markets, goods traded are denoted not only by type and time and place of delivery but also by a state of nature, for they are delivered if and only if that state of nature occurs. But, like insurance policies, they are paid for at the time of trading independent of whether that state occurs. Since the commodities it would produce in each state of nature are all sold in this way, firm profits are determinate.

Let $p(\theta)$ be the price of a unit of consumption delivered if state θ occurs. Then the profits of the j th firm are (the factor is paid in advance)

$$(31) \quad \pi_j = \int p(\theta) f^j(\theta, k_j) d\theta - rk_j$$

where r is the rate of factor remuneration. Probabilities do not enter this expression since all possible outputs are sold with payment at the time of trading. (Probabilities will, of course, be reflected in prices.)

Similarly, the consumer purchases consumption in each state of nature separately so as to maximize his expected utility. The consumer's problem can be written

$$(32) \quad \begin{aligned} & \underset{c_i(\theta)}{\text{Maximize}} \int U_i(c_i(\theta)) h_i(\theta) d\theta \\ & \text{subject to} \int p(\theta) c_i(\theta) d\theta = rw_i + \sum_j w_{ij} \pi_j \end{aligned}$$

where w_i is his stock of input and w_{ij} the fraction of the j th firm he owns. (With just a stock market, these separate commodities, $c(\theta)$, could only be purchased in bundles, reflecting profit patterns as a function of θ .) The first order conditions for this maximization are

$$(33) \quad U'_i(c_i(\theta)) h_i(\theta) = \lambda_i p(\theta)$$

where λ_i is a Lagrangian multiplier. Differentiating this equation logarithmically with respect to θ gives a description of the pattern of consumption in terms of subjective probabilities, risk aversion, and prices.

$$(34) \quad \frac{U'_i''}{U'_i} \frac{dc_i}{d\theta} + \frac{1}{h_i} \frac{dh_i}{d\theta} = \frac{1}{p} \frac{dp}{d\theta}.$$

Thus for two individuals with the same subjective probabilities (and facing the same prices) consumption variation depends on risk aversion,¹⁷

$$(35) \quad \frac{dc_1/d\theta}{dc_2/d\theta} = \frac{U_2''/U_2'}{U_1''/U_1'}.$$

Plotting these consumption patterns, we obtain a figure which shows that the individuals have the same iso-consumption loci, i.e., $c_1(\theta_1) = c_1(\theta_2)$ implies $c_2(\theta_1) = c_2(\theta_2)$. This follows from the special form of the utility function in terms of the contingent commodities, with $U_i(c_i(\theta)) h_i(\theta) d\theta$ being additive in $c_i(\theta_1)$ and $c_i(\theta_2)$ with different individuals treating the two goods symmetrically. Thus

$$\frac{h(\theta_1) U'_1(c_1)}{h(\theta_2) U'_1(c_1)} = \frac{p(\theta_1)}{p(\theta_2)} \quad \text{implies} \quad \frac{h(\theta_1) U'_2(c_2)}{h(\theta_2) U'_2(c_2)} = \frac{p(\theta_1)}{p(\theta_2)}.$$

¹⁷ This is the index of risk aversion which appears in Arrow [3] and Pratt [7].

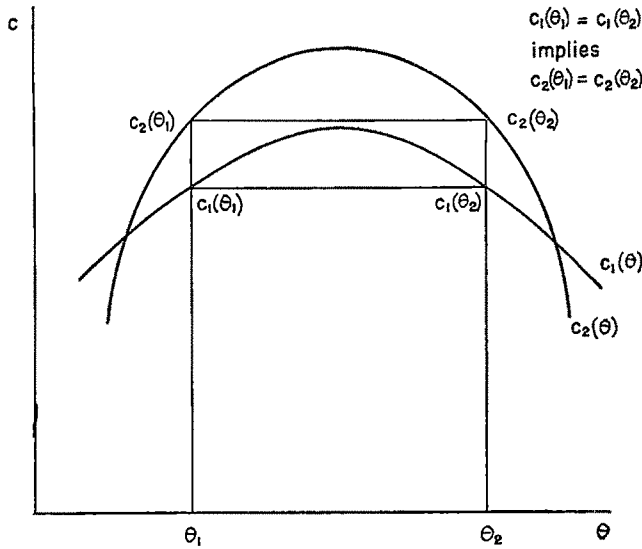


FIGURE 1

If everyone held the same subjective probabilities, the states of nature could be redefined so they were equally probable (i.e., so that $h_i(\theta)$ became the uniform density). Then prices would vary inversely with aggregate output, showing greater variation the more risk averse the consumers. To see this, we take the market clearance condition that total consumption equal total output in all states of nature and differentiate with respect to θ ,

$$(36) \quad \sum_i \frac{dc_i}{d\theta} = \sum_j \frac{dy_j}{d\theta}.$$

Substituting for the change in consumption from equation (34) (recalling that $dh_i/d\theta = 0$) we have

$$(37) \quad \frac{1}{p} \frac{dp}{d\theta} \sum_i \frac{U'_i}{U'_i} = \sum_j \frac{dy_j}{d\theta}.$$

With greater risk aversion, more price variance is needed to induce consumers to purchase the varying output supply.

Let us now examine the production conditions in this model to show the parallel with the allocation with a stock market. Profit maximization can be written

$$(38) \quad \begin{aligned} &\text{Maximize}_{k_j} \int p(\theta) y_j(\theta) d\theta - rk_j \\ &\text{subject to } y_j(\theta) = f^j(\theta, k). \end{aligned}$$

The first order condition for maximization is then

$$(39) \quad \int p(\theta) f_k^j(\theta, k_j) d\theta = r.$$

Substituting for the price from the first order condition for utility maximization, equation (33), we have

$$(40) \quad \lambda_i^{-1} \int U_i^j f_k^j(\theta, k_j) h_i(\theta) d\theta = r.$$

Assuming a decomposable production function this can be written

$$(41) \quad g_j^j(k_j) \int U_i^j f_j(\theta) h_i(\theta) d\theta = r \lambda_i.$$

This has the same form as equation (25).

Similarly the value of output can be written

$$(42) \quad \pi_j + r k_j = \int p(\theta) y_j(\theta) d\theta = \lambda_i^{-1} \int U_i^j y_j(\theta) h_i(\theta) d\theta.$$

This has the same form as equation (22). Thus the production aspects of the two models are the same, with the differences in market structure affecting the actual input allocation by affecting the consumption possibilities which can be purchased.

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CONSERVATION RECONSIDERED

By JOHN V. KRUTILLA*

"It is the clear duty of Government, which is the trustee for unborn generations as well as for its present citizens, to watch over, and if need be, by legislative enactment, to defend, the exhaustible natural resources of the country from rash and reckless spoliation. How far it should itself, either out of taxes, or out of State loans, or by the device of guaranteed interest, press resources into undertakings from which the business community, if left to itself, would hold aloof, is a more difficult problem. Plainly, if we assume adequate competence on the part of governments, there is a valid case for *some* artificial encouragement to investment, particularly to investments the return from which will only begin to appear after the lapse of many years."

A. C. PIGOU

Conservation of natural resources has meant different things to different people. But to the economist from the time of Pigou, who first took notice of the economics of conservation [10, p. 27ff], until quite recently, the central concerns have been associated with the question of the optimal intertemporal utilization of the fixed natural resource stocks. The gnawing anxiety provoked by the Malthusian thesis of natural resource scarcity was in no way allayed by the rates of consumption of natural resource stocks during two world wars occurring between the first and fourth editions of Pigou's famous work. In the United States, a presidential commission, reviewing the materials situation following World War II, concluded that an end had come to the historic decline in the cost of natural resource commodities [12, pp. 13-14]. This conclusion reinforced the concern of many that the resource base ultimately would be depleted.

More recently, on the other hand, a systematic analysis of the trends in prices of natural resource commodities did not reveal any permanent interruption in the decline relative to commodities and services in general [11]. Moreover, a rather ambitious attempt to test rigorously the thesis of natural resource scarcity suggested instead that technological progress had compensated quite adequately for the depletion of the higher quality natural resource stocks [1]. Further, given the present state of the arts, future advances need not be fortuitous occurrences;

*The author is indebted to all of his colleagues at Resources for the Future and to Harold Barnett, Paul Davidson, Otto Davis, Chandler Morse, Peter Pearse, and Ralph Turvey for many helpful suggestions on an earlier draft of this paper.

rather the rate of advance can be influenced by investment in research and development. Indeed, those who take an optimistic view would hold that the modern industrial economy is winning its independence from the traditional natural resources sector to a remarkable degree. Ultimately, the raw material inputs to industrial production may be only mass and energy [1, p. 238].¹

While such optimistic conclusions were being reached, they were nevertheless accompanied by a caveat that, while we may expect production of goods and services to increase without interruption, the level of living may not necessarily be improved. More specifically, Barnett and Morse concluded that the quality of the physical environment—the landscape, water, and atmospheric quality—was deteriorating.

These conclusions suggest that on the one hand the traditional concerns of conservation economics—the husbanding of natural resource stocks for the use of future generations—may now be outmoded by advances in technology. On the other hand, the central issue seems to be the problem of providing for the present and future the amenities associated with unspoiled natural environments, for which the market fails to make adequate provision. While this appears to be the implication of recent research,² and is certainly consistent with recent public policy in regard to preserving natural environments, the traditional economic rationale for conservation does not address itself to this issue directly.³ The use of Pigou's social time preference may serve only to hasten the conversion of natural environments into low-yield capital investments.⁴ On what basis, then, can we make decisions when we confront a choice entailing action which will have an irreversible adverse consequence for rare phenomena of nature? I investigate this question below.

Let us consider an area with some unique attribute of nature—a geomorphologic feature such as the Grand Canyon, a threatened species, or an entire ecosystem or biotic community essential to the survival of the threatened species.⁵ Let us assume further that the area can be used

¹ The conclusions were based on data relevant to the U.S. economy. While they may be pertinent to Western Europe also, all of my subsequent observations are restricted to the United States.

² For example, see [7].

³ It must be acknowledged that with sufficient patience and perception nearly all of the argument for preserving unique phenomena of nature can be found in the classic on conservation economics by Ciriacy-Wantrup [3].

⁴ An example of this was the recent threat to the Grand Canyon by the proposed Bridge and Marble Canyon dams. Scott makes a similar point with reference to natural resource commodities [13].

⁵ Uniqueness need not be absolute for the following arguments to hold. It may be, like Dupuit's bridge, a good with no adequate substitutes in the "natural" market area of its

for certain recreation and/or scientific research activities which would be compatible with the preservation of the natural environment, or for extractive activities such as logging or hydraulic mining, which would have adverse consequences for scenic landscapes and wildlife habitat.

A private resource owner would consider the discounted net income stream from the alternative uses and select the use which would hold prospects for the highest present net value. If the use which promises the highest present net value is incompatible with preserving the environment in its natural state, does it necessarily follow that the market will allocate the resources efficiently? There are several reasons why private and social returns in this case are likely to diverge significantly.

Consider the problem first in its static aspects. By assumption, the resources used in a manner compatible with preserving the natural environment have no close substitutes; on the other hand, alternative sources of supply of natural resource commodities are available.⁶ Under the circumstances and given the practical obstacles to perfectly discriminating pricing, the private resource owner would not be able to appropriate in gate receipts the entire social value of the resources when used in a manner compatible with preserving the natural state. Thus the present values of his expected net revenues are not comparable as between the competing uses in evaluating the efficiency of the resource allocation.

Aside from the practical problem of implementing a perfectly discriminating pricing policy, it is not clear even on theoretic grounds that a comparison of the total area under the demand curve on the one hand and market receipts on the other will yield an unambiguous answer to the allocative question. When the existence of a grand scenic wonder or a unique and fragile ecosystem is involved, its preservation and continued availability are a significant part of the real income of many individuals.⁷ Under the conditions postulated, the area under the demand curve, which represents a maximum willingness to pay, may be significantly less than the minimum which would be required to compensate such individuals were they to be deprived in perpetuity of the opportunity

principal clientele, while possibly being replicated in other market areas to which the clientele in question has no access for all practical purposes.

⁶The asymmetry in the relation posited is realistic. The historic decline in cost of natural resource commodities relative to commodities in general suggests that the production and exchange of the former occur under fairly competitive conditions. On the other hand, increasing congestion at parks, such as Yellowstone, Yosemite, and Grand Canyon, suggests there are no adequate substitutes for these rare natural environments.

⁷These would be the spiritual descendants of John Muir, the present members of the Sierra Club, the Wilderness Society, National Wildlife Federation, Audubon Society and others to whom the loss of a species or the disfigurement of a scenic area causes acute distress and a sense of genuine relative impoverishment.

to continue enjoying the natural phenomenon in question. Accordingly, it is conceivable that the potential losers cannot influence the decision in their favor by their aggregate willingness to pay, yet the resource owner may not be able to compensate the losers out of the receipts from the alternative use of the resource. In such cases—and they are more likely encountered in this area—it is impossible to determine whether the market allocation is efficient or inefficient.

Another reason for questioning the allocative efficiency of the market for the case in hand has been recognized only more recently. This involves the notion of *option demand* [14]. This demand is characterized as a willingness to pay for retaining an option to use an area or facility that would be difficult or impossible to replace and for which no close substitute is available. Moreover, such a demand may exist even though there is no current intention to use the area or facility in question and the option may never be exercised. If an option value exists for rare or unique occurrences of nature, but there is no means by which a private resource owner can appropriate this value, the resulting resource allocation may be questioned.

Because options are traded on the market in connection with other economic values, one may ask why no market has developed where option value exists for the preservation of natural environments.⁸ We need to consider briefly the nature of the value in question and the marketability of the option.

From a purely scientific viewpoint, much is yet to be learned in the earth and life sciences; preservation of the objects of study may be defended on these grounds, given the serendipity value of basic research. We know also that the natural biota represents our reservoir of germ plasm, which has economic value. For example, modern agriculture in advanced countries represents cultivation figuratively in a hot-house environment in which crops are protected against disease, pests, and drought by a variety of agricultural practices. The energy released from some of the genetic characteristics no longer required for survival under cultivated conditions is redirected toward greater productivity. Yet because of the instability introduced with progressive reduction of biological diversity, a need occasionally arises for the reintroduction of some genetic characteristics lost in the past from domestic strains. It is from the natural biota that these can be obtained.

The value of botanical specimens for medicinal purposes also has been long, if not widely, recognized. Approximately half of the new drugs currently being developed are obtained from botanical specimens.⁹ There is a traffic in medicinal plants which approximates a third

⁸ For a somewhat differently developed argument, see [6].

⁹ For an interesting account of the use of plants for medicinal purposes, see [8].

of a billion dollars annually. Cortisone, digitalis, and heparin are among the better known of the myriad drugs which are derived from natural vegetation or zoological sources. Since only a relatively small part of the potential medicinal value of biological specimens has yet been realized, preserving the opportunity to examine all species among the natural biota for this purpose is a matter of considerable importance.

The option value may have only a sentimental basis in some instances. Consider the rallying to preserve the historical relic, "Old Ironsides."¹⁰ There are many persons who obtain satisfaction from mere knowledge that part of wilderness North America remains even though they would be appalled by the prospect of being exposed to it. Subscriptions to World Wildlife Fund are of the same character. The funds are employed predominantly in an effort to save exotic species in remote areas of the world which few subscribers to the Fund ever hope to see. An option demand may exist therefore not only among persons currently and prospectively active in the market for the object of the demand, but among others who place a value on the mere existence of biological and/or geomorphological variety and its widespread distribution.¹¹

If a genuine value for retaining an option in these respects exists, why has not a market developed? To some extent, and for certain purposes, it has. Where a small natural area in some locality in the United States is threatened, the property is often purchased by Nature Conservancy,¹² a private organization which raises funds through voluntary subscriptions.¹³ But this market is grossly imperfect. First, the risk for private investors associated with absence of knowledge as to whether a particular ecosystem has special characteristics not widely shared by others is enormous.¹⁴ Moreover, to the extent that the natural environment will support basic scientific research which often has unanticipated practical results, the serendipity value may not be appropriable by those paying to preserve the options. But perhaps of greatest significance is that the preservation of the grand scenic wonders, threatened species, and the like involves comparatively large land tracts which are not of merely

¹⁰ The presumption in favor of option value is applicable also to historic and cultural features; rare works of art, perhaps, being the most prominent of this class.

¹¹ The phenomenon discussed may have an exclusive sentimental basis, but if we consider the "bequest motivation" in economic behavior, discussed below, it may be explained by an interest in preserving an option for one's heirs to view or use the object in question.

¹² Not to be confused with a public agency of the same name in the United Kingdom.

¹³ Subscriptions to World Wildlife Fund, the Wilderness Society, National Parks Association, etc. may be similar, but, of course, much of the effect these organizations have on the preservation of natural areas stems not from purchasing options, but from influencing public programs.

¹⁴ The problem here is in part like a national lottery in which there exists a very small chance for a very large gain. Unlike a lottery, rather large sums at very large risk typically would be required.

local interest. Thus, all of the problems of organizing a market for public goods arise. Potential purchasers of options may be expected to bide time in the expectation that others will meet the necessary cost, thus eliminating cost to themselves. Since the mere existence or preservation of the natural environment in question satisfies the demand, those who do not subscribe cannot be excluded except by the failure to enroll sufficient subscribers for its preservation.

Perhaps of equal significance to the presumption of market failure are some dynamic characteristics of the problem suggested by recent research. First, consider the consumption aspects of the problem. Davidson, Adams, and Seneca have recently advanced some interesting notions regarding the formation of demand that may be particularly relevant to our problem [5, p. 186].

When facilities are not readily available, skills will not be developed and, consequently, there may be little desire to participate in these activities. If facilities are made available, opportunities to acquire skill increase, and user demand tends to rise rapidly over time as individuals learn to enjoy these activities. Thus, participation in and enjoyment of water recreational activities by the present generation will stimulate future demand without diminishing the supply presently available. Learning-by-doing, to the extent it increases future demand, suggests an interaction between present and future demand functions, which will result in a public good externality, as present demand enters into the utility function of future users.

While this quotation refers to water-based recreation, it is likely to be more persuasive in connection with some other resource-based recreation activity. Its relevance for wilderness preservation is obvious. When we consider the remote backcountry landscape, or the wilderness scene as the object of experience and enjoyment, we recognize that utility from the experience depends predominantly upon the prior acquisition of technical skill and specialized knowledge. This, of course, must come from experience initially with less arduous or demanding activities. The more the present population is initiated into activities requiring similar but less advanced skills (e.g., car camping), the better prepared the future population will be to participate in the more exacting activities. Given the phenomenal rise of car camping, if this activity will spawn a disproportionate number of future back-packers, canoe cruisers, cross-country skiers, etc., the greater will be the induced demand for wild, primitive, and wilderness-related opportunities for indulging such interest. Admittedly, we know little about the demand for outdoor experiences which depend on unique phenomena of nature—its formation, stability, and probable course of development. These are important questions for research, results of which will have significant policy implications.

In regard to the production aspects of the "new conservation," we need to examine the implications of technological progress a little further. Earlier I suggested that the advances of technology have compensated for the depletion of the richer mineral deposits and, in a sense, for the superior stands of timber and tracts of arable land. On the other hand, there is likely to be an asymmetry in the implications of technological progress for the production of goods and services from the natural resource base, and the production of natural phenomena which give rise to utility without undergoing fabrication or other processing.¹⁵ In fact, it is improbable that technology will advance to the point at which the grand geomorphologic wonders could be replicated, or extinct species resurrected. Nor is it obvious that fabricated replicas, were they even possible, would have a value equivalent to that of the originals. To a lesser extent, the landscape can be manufactured in a pleasing way with artistry and the larger earth-moving equipment of today's construction technology. Open pit mines may be refilled and the surroundings rehabilitated in a way to approximate the original conditions. But even here the undertaking cannot be accomplished without the cooperation of nature over a substantial period of time depending on the growth rate of the vegetal cover and the requirements of the native habitat.¹⁶ Accordingly, while the supply of fabricated goods and commercial services may be capable of continuous expansion from a given resource base by reason of scientific discovery and mastery of technique, the supply of natural phenomena is virtually inelastic. That is, we may preserve the natural environment which remains to provide amenities of this sort for the future, but there are significant limitations on reproducing it in the future should we fail to preserve it.

If we consider the asymmetric implications of technology, we can conceive of a transformation function having along its vertical axis amenities derived directly from association with the natural environment and fabricated goods along the horizontal axis. Advances in technology would stretch the transformation function's terminus along the horizontal axis but not appreciably along the vertical. Accordingly, if we simply take the effect of technological progress over time, considering tastes as constant, the marginal trade-off between manufactured and natural amenities will progressively favor the latter. Natural environments will represent irreplaceable assets of appreciating value with the passage of time.

If we consider technology as constant, but consider a change in tastes progressively favoring amenities of the natural environment due to the learn-by-doing phenomenon, natural environments will similarly for this

¹⁵ I owe this point to a related observation, to my knowledge first made by Ciriacy-Wantrup [3, p. 47].

¹⁶ That is, giving rise to option value for members of the present population.

reason represent assets of appreciating value. If both influences are operative (changes in technology with asymmetric implications, and tastes), the appreciating value of natural environments will be compounded.

This leads to a final point which, while a static consideration, tends to have its real significance in conjunction with the effects of parametric shifts in tastes and technology. We are coming to realize that consumption-saving behavior is motivated by a desire to leave one's heirs an estate as well as by the utility to be obtained from consumption.¹⁷ A bequest of maximum value would require an appropriate mix of public and private assets, and, equally, the appropriate mix of opportunities to enjoy amenities experienced directly from association with the natural environment along with readily producible goods. But the option to enjoy the grand scenic wonders for the bulk of the population depends upon their provision as public goods.

Several observations have been made which may now be summarized. The first is that, unlike resource allocation questions dealt with in conventional economic problems, there is a family of problems associated with the natural environment which involves the irreproducibility of unique phenomena of nature—or the irreversibility of some consequence inimical to human welfare. Second, it appears that the utility to individuals of direct association with natural environments may be increasing while the supply is not readily subject to enlargement by man. Third, the real cost of refraining from converting our remaining rare natural environments may not be very great. Moreover, with the continued advance in technology, more substitutes for conventional natural resources will be found for the industrial and agricultural sectors, liberating production from dependence on conventional sources of raw materials. Finally, if consumption-saving behavior is motivated also by the desire to leave an estate, some portion of the estate would need to be in assets which yield collective consumption goods of appreciating future value. For all of these reasons we are confronted with a problem not conventionally met in resource economics. The problem is of the following nature.

At any point in time characterized by a level of technology which is less advanced than at some future date, the conversion of the natural environment into industrially produced private goods has proceeded further than it would have with the more advanced future technology. Moreover, with the apparent increasing appreciation of direct contact with natural environments, the conversion will have proceeded further, for this reason as well, than it would have were the future composition of tastes to have prevailed. Given the irreversibility of converted natural

¹⁷ See [2]; also [9].

environments, however, it will not be possible to achieve a level of well-being in the future that would have been possible had the conversion of natural environments been retarded. That this should be of concern to members of the present generation may be attributable to the bequest motivation in private economic behavior as much as to a sense of public responsibility.¹⁸

Accordingly, our problem is akin to the dynamic programming problem which requires a present action (which may violate conventional benefit-cost criteria) to be compatible with the attainment of future states of affairs. But we know little about the value that the instrumental variables may take. We have virtually no knowledge about the possible magnitude of the option demand. And we still have much to learn about the determinants of the growth in demand for outdoor recreation and about the quantitative significance of the asymmetry in the implications of technological advances for producing industrial goods on the one hand and natural environments on the other. Obviously, a great deal of research in these areas is necessary before we can hope to apply formal decision criteria comparable to current benefit-cost criteria. Fully useful results may be very long in coming; what then is a sensible way to proceed in the interim?

First, we need to consider what we need as a minimum reserve to avoid potentially grossly adverse consequences for human welfare. We may regard this as our scientific preserve of research materials required for advances in the life and earth sciences. While no careful evaluation of the size of this reserve has been undertaken by scientists, an educated guess has put the need in connection with terrestrial communities at about ten million acres for North America [4, p. 128]. Reservation of this amount of land—but a small fraction of one per cent of the total relevant area—is not likely to affect appreciably the supply or costs of material inputs to the manufacturing or agricultural sectors.

The size of the scientific preserve required for aquatic environments is still unknown. Only after there is developed an adequate system of classification of aquatic communities will it be possible to identify distinct environments, recognize the needed reservations, and, then, estimate the opportunity costs. Classification and identification of aquatic environments demand early research attention by natural scientists.

Finally, one might hope that the reservations for scientific purposes would also support the bulk of the outdoor recreation demands, or that substantial additional reservations for recreational purposes could be

¹⁸ The rationale above differs from that of Stephen Marglin which is perhaps the most rigorous one relying on a sense of public responsibility and externalities to justify explicit provision for future generations. In this case also, my concern is with providing *collective consumption goods for the present and future*, whereas the traditional concern in conservation economics has been with provision of *private intermediate goods for the future*.

justified by the demand and implicit opportunity costs. Reservations for recreation, as well as for biotic communities, should include special or rare environments which can support esoteric tastes as well as the more common ones. This is a matter of some importance because outdoor recreation opportunities will be provided in large part by public bodies, and within the public sector there is a tendency to provide a homogenized recreation commodity oriented toward a common denominator. There is need to recognize, and make provision for, the widest range of outdoor recreation tastes, just as a well-functioning market would do. We need a policy and a mechanism to ensure that all natural areas peculiarly suited for specialized recreation uses receive consideration for such uses. A policy of this kind would be consistent both with maintaining the greatest biological diversity for scientific research and educational purposes and with providing the widest choice for consumers of outdoor recreation.

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A NOTION OF EQUILIBRIUM AND ITS POSSIBILITY UNDER MAJORITY RULE

By CHARLES R. PLOTT*

Consider a committee that is faced with the task of deciding on how to change the magnitude of several variables. It is assumed that agreement is defined by majority rule. The decision-making body could be a board of directors attempting to decide on the magnitude of several investment projects, or a group of individuals deciding upon the allocation of a budget among several public goods. The setting makes no difference as long as the variables could conceivably be changed by any amount. If a change in the variables is proposed and the change does not receive a majority vote, then the "existing state" of the variables remains. If no possible change in the variables could receive a majority vote, then the "existing state" of the variables is an "equilibrium."¹

The purpose of this paper is to make clear such a notion of equilibrium and to investigate the possibility of its existence. Section I sets forth the general setting, definitions, and assumptions. Section II pertains to situations where there is no constraint on the possible magnitude of the variables. Section III pertains to situations where there is one constraint (such as a fixed amount that the committee may spend). Section IV contains some general observations, possible applications, and questions brought forth by the procedure. An appendix contains an outline to the proofs of the propositions in the text.

Before continuing, it may be best to indicate some of the things *not* considered except by way of observation in the final section, if at all. Only "local" equilibriums are considered. Of course, "global" equilibriums must be special cases of these. There is no real theory of the path to equilibrium or even, for that matter, a convincing assurance that an equilibrium, if it exists, will be attained. Strategic considerations are ignored as are all second order conditions. These omissions are especially important in light of the results.

I. *The Basic Model*

Assume there are m individuals $(1, 2, \dots, i, \dots, m)$ who are attempting to decide on the magnitude of n variables $(x_1, x_2, \dots, x_j,$

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¹ This notion of equilibrium was first examined by Duncan Black and R. A. Newing. Special cases of some of the theorems are implied by the diagrams in [1, pp. 19-28] and [2, pp. 137-39].

$\dots, x_n)$. Each individual is assumed to have a differentiable utility function $U^i = U^i(x_1, x_2, \dots, x_n)$ defined on the n variables.²

Consider now a specific, small change in the variables (dx_1^* , dx_2^* , \dots , dx_n^*) from some "existing situation" (say \bar{X} in Euclidean n -space, E_n). This change in the variables shall be called a "motion."³ Individual i would "vote for" the motion over the existing situation \bar{X} if

$$\frac{\partial U^i}{\partial x_1} dx_1^* + \frac{\partial U^i}{\partial x_2} dx_2^* + \dots + \frac{\partial U^i}{\partial x_n} dx_n^* > 0.$$

That is, he favors the motion if it would increase his utility. Adopting the notation to be used, we can say that he "votes for" the motion b_k if

$$\nabla U^i b_k > 0$$

where

$$\nabla U^i = \left(\frac{\partial U^i}{\partial x_1}, \frac{\partial U^i}{\partial x_2}, \dots, \frac{\partial U^i}{\partial x_n} \right)$$

is the gradient vector of individual i 's utility function, b_k is some particular "motion" (dx_1^* , dx_2^* , \dots , dx_n^*) from the infinite set of "small moves away from \bar{X} ."⁴ Further, if $\nabla U^i b_k < 0$, we say he is "indifferent." It shall be assumed that indifferent individuals behave in the same specified manner, i.e., always "vote for," always "vote against," or "never vote" as the case may be.

Graphically the situation is represented by Figure 1. The curve I is an indifference curve or level surface of the utility function. Suppose \bar{X} represents the "existing state." This individual would "vote for" a proposed move from \bar{X} such as b_f , vote "against" a motion such as b_a , and be indifferent toward a proposal such as b_i .

Consider now a group of individuals $(1, 2, \dots, m)$ and the associated gradient vectors $\nabla U^i = (a_{i1}, a_{i2}, \dots, a_{in})$. Ignoring problems of strategic behavior, all individuals would "vote for" any proposal, b , which satisfies⁵

$$(1) \quad Ab > 0$$

² The variables are such that they behave as "collective goods." See [5].

³ We shall assume the "motion" vector is normalized, i.e., where $(dx_1^*, \dots, dx_n^*) = b^*$, then $|b^*| = 1$. For notational purposes, some particular motion, say j , will be denoted as b_j and the components will be denoted as $(b_{j1}, b_{j2}, \dots, b_{jn})$.

⁴ He votes for any motion for which the directional derivative, in that direction, is positive. ab , where a and b are vectors, denotes an inner product.

⁵ The problem of finding such "acceptable proposals" is simply a linear programming problem. See [4].

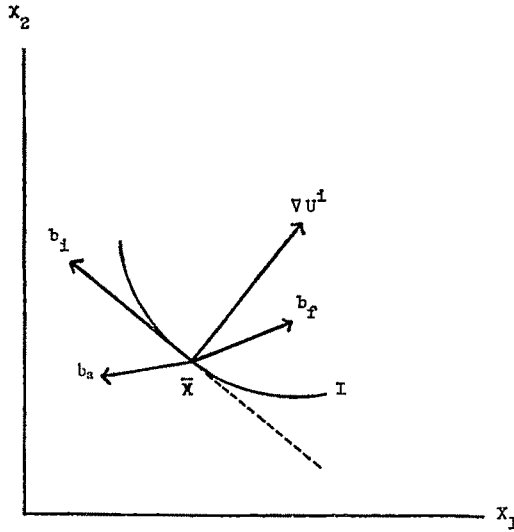


FIGURE 1

where

$$A = \begin{bmatrix} a_{11} & a_{12} & \cdots & a_{1n} \\ \vdots & & & \\ a_{m1} & a_{m2} & \cdots & a_{mn} \end{bmatrix}.$$

It is well known that this system of inequalities has a solution, b , if and only if the system

$$(2) \quad yA = 0 \quad y = (y_1, \dots, y_m) \text{ and } y_i \geq 0 \text{ not all } = 0$$

has no semipositive solution y (see appendix).

Suppose the committee was operating under unanimity. Suppose further that an individual will vote "against" a motion that does not increase his utility (indifferent people vote "no"). Then any point in the space where (2) is satisfied would be an "equilibrium point." There would not exist a "motion" that could receive the required vote because, for any of the infinite possible motions, at least one person would vote "no."⁶

Suppose the committee was operating under majority rule. The committee would be at an equilibrium if and only if there does not exist a "motion" that could receive a favorable vote from a majority. This notion of equilibrium seems to accord well with the usual meanings of

⁶ If it is supposed that individuals would "permit" a motion to which they are indifferent, an equilibrium point must be one where (2) has a strictly positive solution y . Every "motion" would either decrease the utility of at least one individual or leave everyone indifferent.

equilibriums. Certainly if the magnitude of the variables was such that some change in them could receive the required vote there is no a priori reason to suppose that the variables would not be so changed.

II. *Equilibrium: No Constraint*

The conditions for majority rule equilibrium when there exists no constraint on permissible motions will be given. Such a constraint is added in the following section. It will be assumed that there are m (an odd number) decision makers. Simple modifications can be made to account for situations where the number of people is even.

A. Existence of an unconstrained equilibrium requires that indifferent individuals do not vote "yes."

If individuals who are indifferent are assumed to abstain from voting, such activities cannot be considered as "yes" votes. Otherwise, an equilibrium cannot exist. To prove this point, let M be an $(m+1)/2 \times n$ matrix, the rows of which come from the rows of A . Since indifferent people vote "yes," equilibrium must be a situation where $Mb < 0^7$ for all possible M that can be formed from the rows of A and all possible b . Pick an M and b that satisfy this equilibrium condition (say M_i and b_i). Then, where $b_k = -b$, $M_i b_k > 0$. The motion b_k would receive a majority contrary to the assumption of equilibrium. Indifferent people cannot vote "yes."

The remaining equilibrium conditions must be satisfied by the gradient vectors. The proofs are in the appendix.

B. Any equilibrium must be a point of maximum utility for at least one individual.

If the point is a maximum for one and only one individual,⁸ the gradients of the remaining individuals must satisfy C.

C. The remaining (even number of) rows of A can be divided into pairs for which there exists a strictly positive solution to

$$(3) \quad \begin{bmatrix} y_i \\ y_j \end{bmatrix} \begin{bmatrix} a_{i1} & a_{i2} & \cdots & a_{in} \\ a_{j1} & a_{j2} & \cdots & a_{jn} \end{bmatrix} = 0 \quad y_i, y_j > 0.$$

The last condition means that all individuals for which the point is not a maximum can be divided into pairs whose interests are diametrically opposed. The situation is shown diagrammatically by Figure 2. Points 1, 2, 3, 4, and 5 are the points of maximum utility for individuals 1, 2, 3, 4, and 5 respectively. The lines connecting the points are those

⁷ The notation $Mb > 0$ means that the inner product of b with each row of M is strictly positive.

⁸ The conditions can be modified to account for the point being a maximum for more than one individual.

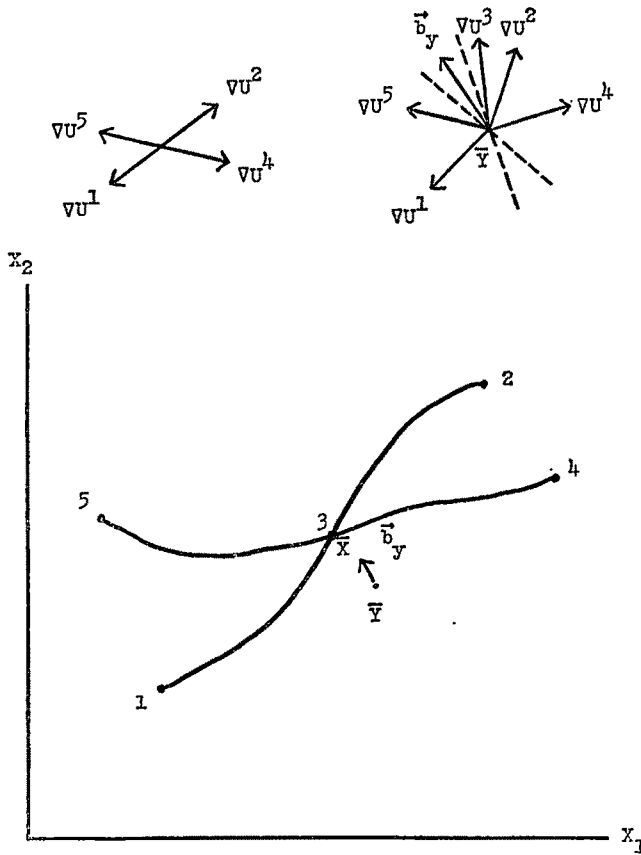


FIGURE 2

points for which (3) is satisfied—the “contract curves” as traditionally termed in economics. The only point which is an equilibrium point is \bar{X} , the point corresponding to the maximum of individual 3. At any other point there exists a motion which would receive a majority vote. For example, consider \bar{Y} . The motion b_y would receive the “yes” votes of individuals 2, 3, and 5.

The above conditions, as qualified, are both necessary and sufficient for a point to be an equilibrium.

Notice that any majority rule equilibrium is obviously Pareto Optimal (almost by definition). But, certainly not all Pareto Optimals are majority rule equilibriums. The condition for Pareto Optimality is simply condition (2), or the condition described in footnote number 6, depending upon how you choose to define Pareto Optimality.

The most important point is that there is certainly nothing inherent

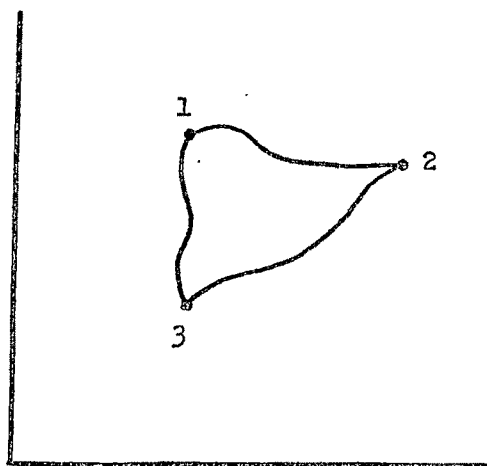


FIGURE 3

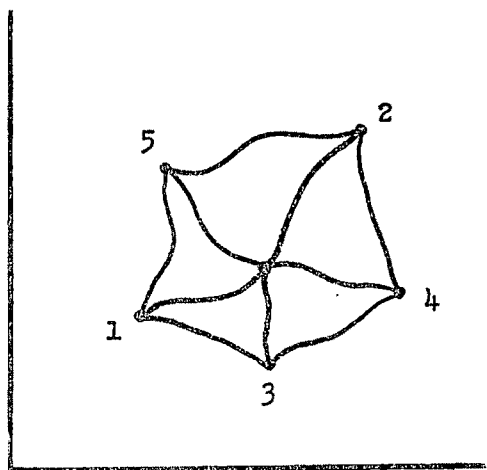


FIGURE 4

in utility theory which would assure the existence of an equilibrium. In fact, it would only be an accident (and a highly improbable one) if an equilibrium exists at all. For example, in Figures 3 and 4 there exists no equilibrium. And, it will be shown below that the addition of a constraint does little to relieve the situation if there are more than two variables to be determined.

III. *Equilibrium: One Constraint*

Suppose the committee was operating under a single constraint such as a budget constraint. Each of the variables has an associated price

and the committee has only a fixed amount (I) to spend. The constraint is of the form

$$(4) \quad \sum_{i=1}^n P_i x_i \leq I.$$

If the committee is at a point such that

$$\sum_{i=1}^n P_i x_i < I$$

the constraint does not alter the range of possible motions and an equilibrium must satisfy the conditions A, B, and C above. Therefore, only points such that

$$\sum_{i=1}^n P_i x_i = I$$

are of interest.

The constraint can be treated as an individual who has veto power. Where the price vector, or the gradient of the constraint is denoted as P , the only admissible (or feasible) motions are those in a set β where

$$\beta = \{b \in E_n \mid Pb \leq 0\}.$$

The problem of finding majority rule equilibrium conditions is simply one of finding conditions on the gradients of the individuals such that

$$Mb \leq 0$$

for all M and all $b \in \beta$ where M again ranges over all $(m+1)/2 \times n$ matrices that can be formed from the rows of A , the matrix of all gradient vectors.

Again, just as in the unconstrained case, it is assumed that all indifferent individuals behave in the same manner. This assumption gives rise to the following behavioral condition.

D. If a constrained equilibrium exists, people cannot "vote for" a motion to which they are indifferent.⁹

Again the remaining conditions are on the gradient vectors of the individuals.

E. For a point to be an equilibrium, the gradient of at least one individual must satisfy

$$(5) \quad \begin{bmatrix} y_1 \\ y_p \end{bmatrix} \begin{bmatrix} a_{i1} & a_{i2} & \dots & a_{in} \\ -P_1 & -P_2 & \dots & -P_n \end{bmatrix} = 0 \quad y_i > 0 \quad y_p \geq 0.$$

⁹ The proof is analogous to the proof of A above. The only difference is that rather than *any* b , one must be chosen from the set β' where $\beta' = \{b \in E_n \mid Pb = 0\}$.

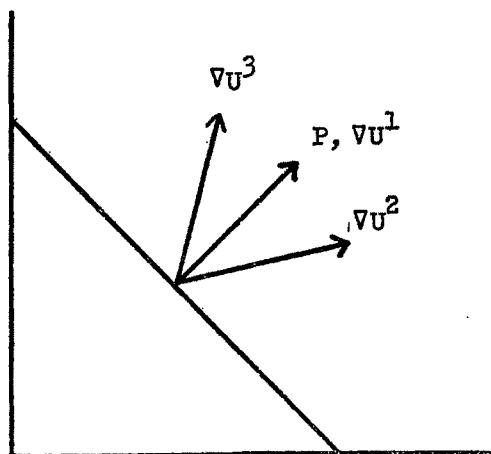


FIGURE 5

This says that the point is either a constrained maximum or a maximum for at least one individual. We shall assume for the following conditions that this is true for one and only one individual. Again, the conditions can easily be modified to account for other cases.

E. For a point to be an equilibrium point, those gradients (even in number) for which D does not hold can be divided into pairs such that there exists a solution to

$$(6) \quad \begin{bmatrix} y_i \\ y_j \\ y_p \end{bmatrix} \begin{bmatrix} a_{i1} & a_{i2} & \cdots & a_{in} \\ a_{j1} & a_{j2} & \cdots & a_{jn} \\ -P_1 & -P_2 & \cdots & -P_n \end{bmatrix} = 0 \quad \begin{matrix} y_i, y_j > 0 \\ y_p \geq 0. \end{matrix}$$

In words this means that the individuals can be divided into pairs such that they are either diametrically opposed or at least opposed on every motion in the set β' defined in footnote 9. On Figure 5, the vectors P and ∇U^1 are parallel and lie in the same direction thus satisfying (5). P can be found as a positive combination of ∇U^2 and ∇U^3 thus satisfying (6).

It is certainly no trick to find a situation where there is no equilibrium at all. For example, there is no equilibrium on Figure 6, where points 1, 2, and 3 are points of constrained maximum for individuals 1, 2, and 3 respectively. Again there is nothing in utility theory that would guarantee the existence of an equilibrium. And again, if an equilibrium exists, it would be purely accidental. This result leads, at this stage of analysis, to rather pessimistic conclusions about the allocation of public goods. Samuelson [5] has demonstrated that the equilibrium attained by a market mechanism for public goods will, in general, fail to be an optimum. The analysis here implies that a majority rule political process

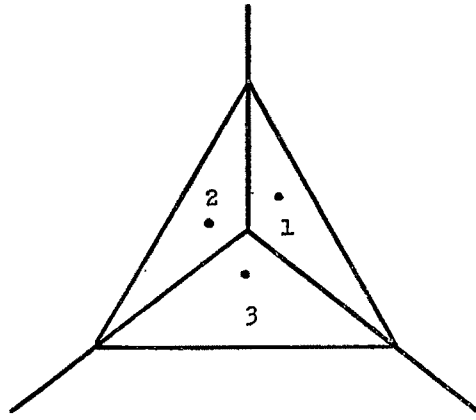


FIGURE 6

will fail to reach an equilibrium at all. Thus, in the case of public goods, society can count upon neither the market nor a majority rule political process to be a desirable allocative device.

IV. *Observations and Conjectures*

The results thus far seem rather negative in that equilibrium under majority rule would seem to be an almost nonexistent phenomenon. In view of this, a review of the concept of equilibrium used, and an inquiry into whether or not any important ingredients were left out should be useful.

Of great importance is the process by which motions are proposed. The decision process *itself* may dictate that some motions cannot be proposed. Often, before a motion can be voted on by a decision body, it must pass through a subcommittee. Unless proposals and amendments can be made from the floor, the subcommittee determines what motions can be voted on. In such cases, the equilibrium conditions would apply only to members of this committee. When the members of such a committee are in equilibrium, the complete voting body is in equilibrium regardless of the preferences of those not on the subcommittee.

This brings up an additional observation on coalitions. Where there exists a majority coalition which, itself, operates under a rule of unanimity, condition (2), when satisfied for the members of this coalition, defines the equilibrium points. If there exists a constraint, the traditional requirements on utility functions are sufficient to assure the existence of an equilibrium in such cases.

A third observation pertains to the role of time in the analysis. Certainly there may be great costs of indecision—if nothing else, simply sitting. As time passes, the utility functions as reflected in the com-

modity space may tend to "flatten." That is, with time, the marginal utility associated with an additional unit of a variable may tend to diminish. The points of maximum may extend to a neighborhood around the original maximum and the "contract curves" may tend to become "broad" as the time taken to reach a decision increases. This type of phenomenon would certainly tend to increase the possibility of the existence of an equilibrium.

Associated with the possibility of a change in evaluations through time is the possibility of a change in preference due to the decision process itself. The exchange of information associated with any decision process may serve actually to change the utility functions. A "persuasive" individual may be successful in changing the utility functions of others so that all of the orderings, in the end, resemble his. Or, the process may tend to cause all utility functions to change to a "similar" ordering but one which is not "similar" to any of the original functions.

The model outlined here lends itself to the possible testing of all of these speculations. If the original utility functions are known, the contract curves can be described as a system of equations and solved (at least theoretically) for an equilibrium. In the absence of such detailed information one might assume that the contract curves are linear. The only information then needed would be the points of maximum for the various individuals (or constrained maximums as the case may be). The contract curves would then be the curves L of the form

$$L = tC + A \quad 0 \leq t \leq 1$$

where

$$B - A = C$$

assuming B is the maximum for individual B and A is the maximum for individual A .

Having obtained this information the experimenter can estimate the equilibrium. The decision process can then be observed. The resulting equilibrium can be compared with the "theoretical" equilibrium and the final utility functions compared with the originals. The path to equilibrium can be observed along with associated strategic behavior, etc. It may be the case that strategic considerations influence individuals to vote against some motion which would increase their utility. Such observations could, in principle, lead to the identification of variables which systematically contribute to the final outcome of group decisions.

MATHEMATICAL APPENDIX

The proofs for the various statements and conditions given in the text are presented below. The propositions given below follow in the same order as the corresponding descriptions in the text.

Definitions

A is an $m \times n$ (m, n finite and m an odd number) matrix.

(m) is the set of vectors which serve as rows of A . The elements of (m) are $(a_1, \dots, a_i, \dots, a_m)$.

M is an $(m+1)/2 \times n$ submatrix of A . (M) is the set of all M that can be formed from A .

A^* is a $r \times n$ submatrix of A ($r = m - 1$).

(r) is the set of vectors which serve as rows of A^* .

In general, except for specifically defined submatrices such as those above, if (ξ) is a set of row vectors of some matrix with ξ elements, then $[\xi]$ is the submatrix with the ξ members of (ξ) as rows.

For any matrix $[\xi]$ and vector b the notation $[\xi] b > 0$ means that the inner product of b with every row of $[\xi]$ is strictly greater than zero. The complement of a set (ξ) is denoted as $(\bar{\xi})$. E_n denotes Euclidean n -space. The notation ab represents an inner product. Define A to be an \hat{A} if and only if for any $b \in E_n \exists M \in (M)$ such that $Mb \leq 0$.

Lemma 1. A is an \hat{A} if and only if for no $M \in (M)$ there is a $b \in E_n$ such that $Mb > 0$.

Proof. Examine first the "if" part. Assume that for $M_i \exists b_i$ such that $M_i b_i > 0$. Since M_i has $(m+1)/2$ rows, there are at most $m - (m+1)/2 = (m-1)/2$ remaining rows of A for which $ab_i \leq 0$ so A cannot be \hat{A} .

Assume that for $b_i \nexists$ an $M \in (M)$ such that $Mb_i \leq 0$. At most, there could be $(m-1)/2$ rows of A such that $ab_i \leq 0$ is the case. For the remaining $(m+1)/2$ rows—call them M_i —it must be the case that $ab_i > 0$. Thus, for M_i there exists a solution to $M_i b > 0$.

Lemma 2. If A is an \hat{A} , then for every $b \in E_n \exists$ an $a \in (m)$ such that $ab = 0$.

PROOF. Assume A is an \hat{A} . Assume that for b_i there does not exist an $a \in (m)$ such that $ab_i = 0$. By the definition of $\hat{A} \exists$ some $M_j \in (M)$ such that $M_j b_i < 0$. But, by letting $b_k = -b_i$, we find $M_j b_k > 0$, thus contradicting the assumption that A is an \hat{A} .

Theorem 1. If A is an \hat{A} , then there exists at least one $a_i \in (m)$ such that $a_i = 0$.

PROOF. For any $b \in E_n$ there must exist an orthogonal row of A by Lemma 2. To prove the theorem, it is sufficient to show that if at least one row of A is not the zero vector, then there exists a solution, say b^* , to $Ab \neq 0$. This will be shown by induction.

Assume A has only one column, no element of which is zero. Then any non-zero scalar satisfies the requirements of b^* .

Assume A has n columns. Assume further that there exists a solution, say b , to

$$\sum_{j=1}^{n-1} a_{ij} b_j = \alpha_i \neq 0 \quad i = 1, \dots, m.$$

Now, if there exists b_n such that $a_{in} b_n \rightarrow -\alpha_i$ for $i = 1, \dots, m$, we are finished. Certainly such a number exists. Just choose b_n not equal to any of the numbers

$$\left(-\frac{\alpha_1}{a_{1n}}, \dots, -\frac{\alpha_n}{a_{nn}} \right).$$

By the theorem, at least one row of A must be the zero vector. The reduced matrix A^* is formed from A by the elimination of a row which is the zero vector. The remaining discussion pertains to A^* which has $r=m-1$ rows called the set (r) .

Lemma 3. The equation

$$[\xi]x > 0$$

has a solution x if and only if the equation

$$y[\xi] = 0$$

has no semipositive solution y .¹⁰

Corollary. The equation

$$[\xi]x < 0$$

has a solution, x , if and only if the equation

$$y[\xi] < 0$$

has no semipositive solution y .

Proof. It is sufficient to show that $[\xi]x < 0$ has a solution if and only if $[\xi]x > 0$ has a solution. Suppose x is a solution to $[\xi]x > 0$. Then $x' = -x$ is a solution to $[\xi]x' < 0$. If x is a solution to $[\xi]x < 0$, then $x' = -x$ is a solution to $[\xi]x' > 0$.

Lemma 4. If A is an \hat{A} then for any $b \in E_n$ such that $a_i b_i = 0$ for some non-zero $a_i \in (r)$, then there exists at least one $a_j \in (r)$, where $i \neq j$, such that $a_j b_i = 0$.

PROOF. Let A be an \hat{A} and assume that for b_i , $ab_i = 0$ for one and only one non-zero $a \in (r)$. Since A is an \hat{A} , we know the following:

$ab_i = 0$ for one and only one non-zero $a \in (r)$, by assumption. Call it a_i .

$ab_i > 0$ for $\frac{r}{2}$ rows of A^* . Call them the set (P) .

$ab_i < 0$ for $\frac{r}{2} - 1$ rows of A^* . Call them the set (D) .

The last two statements are valid because otherwise either b_i or $-b_i$ would be a solution to $Mb > 0$ contrary to the assumption that A is an \hat{A} . It makes no difference whether (P) or (D) contains $r/2$ elements since (D) becomes (P) for $-b_i$. By Lemma 3, there exists no semipositive solution to

$$y[P] = 0.$$

¹⁰ For the proof, see [3, p. 48]. "Semipositive" is taken to mean that $y_i \geq 0$ and not all $y_i = 0$.

This is forced by the definition of (P) . There also exists no semipositive solution to

$$(1) \quad y \begin{bmatrix} P \\ a_i \end{bmatrix} = 0.$$

Assume that there does exist a semipositive solution to (1). This implies that $-a_i$ can be expressed as a positive combination of some of the elements of (P) . Assume them to be the first k elements. That is

$$-a_i = \sum_{j=1}^k y_j a_j \quad \text{where } y_j > 0.$$

Multiplying by b_i , we find

$$0 = -a_i b_i = y_1 a_1 b_i + \cdots + y_k a_k b_i \quad y_i > 0.$$

But, this contradicts the fact that $a_i b_i > 0$ for all $a \in (P)$. Because (1) has no solution, by Lemma 3 there must exist a solution to

$$(2) \quad \begin{bmatrix} P \\ a_i \end{bmatrix} \quad b > 0$$

and, since there are $(m+1)/2$ rows of the matrix in (2), A cannot be an \hat{A} . The lemma is proved.

Lemma 5. If A is an \hat{A} , then for each row vector a_i of A^* there exists at least one row vector a_j , $j \neq i$, of A^* and scalars α_i and α_j (not both zero) such that $\alpha_j a_j + \alpha_i a_i = 0$.

PROOF. If some $a \in (r)$ is the zero vector, the condition of the lemma is satisfied. If no $a \in (r)$ is the zero, then any b such that $a_i b = 0$ must be orthogonal to at least one other row of A^* . The finite condition on the rows forces there to be at least one row, say a_j , which is orthogonal to every vector b which is orthogonal to a_i . This means a_i and a_j are parallel and the lemma is proved. The formal proof follows closely the proof of Theorem 1.

Theorem 2. If one and only one row of A is the zero vector, then A is an \hat{A} if and only if the rows of A^* can be partitioned into two element sets such that there exists a solution to

$$y_i a_i + y_j a_j = 0 \quad y_i, y_j > 0.$$

PROOF. By Lemma 5, if A is an \hat{A} , then the rows of A^* can be divided into sets which are parallel. It will be shown that each such set contains an even number of vectors and that half lie in one direction and the other half lie in the opposite direction. This is simply another way of stating the theorem.

Choose any row of A^* , say a_i , and divide all rows of A^* into the sets with respect to a_i as defined below.

s = the number of vectors in a set (s) where $(s) = \{a_j \in (r) \mid \alpha_i a_i + \alpha_j a_j = 0$

has a solution α_i and α_j . Note that (s) contains a_i . Recall also that by assumption, (r) contains no zero.

q = the number of vectors in a set (q) where $(q) = \{a_j \in (s) \mid a_j = \lambda a_i \text{ has a solution } \lambda > 0\}$. Note that (q) contains a_i .

l = the number of vectors in a set (l) where $(l) = \{a \in (s) \mid a \notin (q)\}$. Notice that $s - q = l$.

b_i = some vector in E_n such that $ab_i = 0$ for all $a \in (s)$ and $ab_i \neq 0$ for all $a \in (r) \cap (\bar{s})$.

p = the number of vectors in a set (p) where $(p) = \{a \in (r) \cap (\bar{s}) \mid ab_i > 0\}$.

d = the number of vectors in a set (d) where $(d) = \{a \in (r) \cap (\bar{s}) \mid ab_i < 0\}$.

Observe that

$$r = p + d + l + q.$$

By Lemma 3 and its corollary there exists no semipositive solution, y , to

$$y[p] = 0 \quad \text{or} \quad y[q] = 0.$$

Also, there exists no semipositive solution, y , to

$$(3) \quad y \begin{bmatrix} p \\ q \end{bmatrix} = 0.$$

A solution to (3) would imply that the negative of some member of (q) can be expressed as a positive combination of a subset of (p) or vice versa. Assume $a_q \in (q)$ can be expressed as a positive combination of the first k elements of (p) , i.e.,

$$-a_q = y_1 a_1 + \cdots + y_k a_k \quad y_i > 0.$$

Multiply by b_i and obtain

$$0 = -a_q b_i = y_1 a_1 b_i + \cdots + y_k a_k b_i \quad y_i > 0.$$

But, by the definition of (p) , all members on the right of the equation are positive thus establishing a contradiction. Since there is no semi-positive solution to (3), there must exist a solution to

$$(4) \quad \begin{bmatrix} p \\ q \end{bmatrix} b > 0.$$

If A is to be an \hat{A} , (4) dictates that

$$(5) \quad p + q \leq \frac{r}{2}.$$

Otherwise, $\begin{bmatrix} p \\ q \end{bmatrix}$ would be an M for which there is a solution to $Mb > 0$

thus violating Lemma 1. By the same reasoning, we know that

$$(6) \quad p + l \leq \frac{r}{2}$$

and, since (d) becomes (p) for $-b_i$, the same argument gives

$$(7) \quad d + l \leq \frac{r}{2}$$

$$(8) \quad d + q \leq \frac{r}{2}.$$

Recalling that $r = p + d + l + q$ and solving equations 5 through 8, one can derive

$$(9) \quad q \leq l \quad \text{and}$$

$$(10) \quad l \leq q.$$

Therefore, $l = q$ and the "only if" part of the theorem is proved.

Now the "if" part. Assume the conditions of the theorem are satisfied. Choose any $b_i \in E_n$ such that $ab_i = 0$ for the zero vector. For every $a \in A$ such that $ab_i > 0$, there is an $a \in A$ such that $ab_i \leq 0$. Therefore $ab_i \leq 0$ for $(m+1)/2$ elements of (m) and A is an \hat{A} .

The theorems below are related to the discussion contained in Section Three of the text. Recall that the discussion there is about majority decisions which must satisfy a single constraint such as

$$\sum_{i=1}^n p_i x_i \leq I.$$

It is assumed that the "existing position" is one which satisfies

$$\sum_{i=1}^k p_i x_i = I.^{11}$$

The following definitions are needed.

Let C be a specific, non-zero vector (the negative of the gradient of the constraint).

$$G = \{b \in E_n \mid b \in J \cup K\}$$

$$J = \{b \in E_n \mid Cb > 0\}$$

$$K = \{b \in E_n \mid Cb = 0\}.$$

Let A be an \hat{A} if and only if for every $b \in G$ there exists an $M \in (M)$ such that $Mb \leq 0$.

¹¹ See the discussion in the text.

Lemma 6. A is an \hat{A} if and only if for no $M \in (M)$ does there exist a $b \in G$ which is a solution to $Mb > 0$. (proof omitted)

Lemma 7. If A is an \hat{A} , then there must exist some $a \in (m)$ and a set of scalars (α_i, a_o) with at least one $\alpha \neq 0$ such that

$$\alpha_i a + \alpha_o C = 0.$$

PROOF. Assume no $a \in (m)$ satisfies the conditions of Lemma 7. Then there exists $b_i \in K$ such that $ab_i = 0$ for no $a \in (m)$. The elements of (m) can be partitioned into two sets according to whether $ab_i > 0$ or $ab_i < 0$. One of these sets must contain at least $(m+1)/2$ elements. Since these sets reverse positions for $-b_i$, A cannot be an \hat{A} by Lemma 6.

Theorem 3. If A is an \hat{A} , then there exists some $a \in (m)$ such that a solution exists to

$$y_i a + y_o C = 0 \quad y_i > 0, y_o \geq 0.$$

PROOF. The following definitions are needed.

H = the number of elements in the set (H) where $(H) = \{a \in (m) \mid \alpha_i a + \alpha_o C = 0 \text{ not all } \alpha = 0\}$.

By Lemma 7, the set (H) is not empty. If a member of (H) is the zero vector, it also satisfies the conditions of the theorem. So only the case where (H) contains no zero vector need be examined.

Assume A to be an \hat{A} and assume that no member of (H) satisfies the condition of the theorem.

b_i = some $b \in K$ such that $ab_i \neq 0$ for all $a \in (\bar{H})$.

P = the number of elements in (P) where $(P) = \{a \in (\bar{H}) \mid ab_i > 0\}$.

D = the number of elements in (D) where $(D) = \{a \in (\bar{H}) \mid ab_i < 0\}$. Note that $P + D + H = m$.

If A is an \hat{A} , then

$$P < \frac{m+1}{2} \quad \text{and} \quad D < \frac{m+1}{2}.$$

Otherwise either $[P]b$ or $[D](-b)$ would satisfy $Mb > 0$ contrary to Lemma 6.

Now, there can exist no semipositive solution to

$$y \begin{bmatrix} P \\ H \\ C \end{bmatrix} = 0.$$

A solution to this implies that any $a_k \in (H) \cup C$ with $y_k \neq 0$ can be expressed as

$$\sum y_j a_j = -a_k \quad y_j \geq 0.$$

Multiply this equation by b_i and observe

$$\sum_{j=1}^P y_j a_j b_i = -a_i b_i = 0 \quad y_j \geq 0$$

which contradicts the fact that $a_j b_i > 0$ for $j=1, \dots, P$.

So, by Lemma 3, there does exist a solution to

$$\begin{bmatrix} P \\ H \\ C \end{bmatrix} \quad b > 0.$$

The same argument shows there is a solution to

$$\begin{bmatrix} D \\ H \\ C \end{bmatrix} \quad b > 0.$$

But, since $P+D+H=m$, either $P+H \geq (m+1)/2$ or $D+H \geq (m+1)/2$ so either one or the other satisfies $Mb > 0$. By Lemma 6, A cannot be an \hat{A} .

If A is an \hat{A} at least one element of (m) must satisfy the condition of Theorem 3. Elimination of this element yields a set called (r) with $r=m-1$ elements.

Lemma 8. If A is an \hat{A} and if the condition of Theorem 3 is satisfied by one and only one row of A , then for every $a_i \in (r)$ there exists at least one $a_j \in (r)$, $i \neq j$, such that a solution exists to

$$\alpha_i a_i + \alpha_j a_j + \alpha_c C = 0 \quad \alpha_i, \alpha_j \neq 0.$$

PROOF. It can be shown that if the condition of this Lemma is *not* satisfied for some row, say a_i , then there exists a $b \in K$ such that $a_i b = 0$ and $a_j b \neq 0$ for all $j \in (r)$, $j \neq i$.¹²

Assume A is an \hat{A} , no $a \in (r)$ satisfies the condition of Theorem 3, and that for a_i there exists a $b \in K$, say b_i , such that $a_i b_i = 0$ and $a_j b_i \neq 0$ for all $a_j \in (r)$, $i \neq j$. We let

P = the number of elements in a set (P) where $(P) = \{a \in (r) \mid a b_i > 0\}$

D = the number of elements in a set (D) where $(D) = \{a \in (r) \mid a b_i < 0\}$.

If A is an \hat{A} then one set contains $r/2$ elements and the other contains $r/2-1$. Otherwise, either b_i or $-b_i$ would be a solution to $Mb > 0$. Assume $P=r/2$. By the same argument used previously there exists no semipositive solution to

$$y \begin{bmatrix} P \\ a_i \\ C \end{bmatrix} = 0.$$

¹² Use corollary to Theorem 2.3 in [3, p. 37].

Thus, by Lemma 3, there exists a solution to

$$\begin{bmatrix} P \\ a_i \\ C \end{bmatrix} \quad b > 0.$$

But the set $(P) \cup a_i$ contains $(m+1)/2$ elements and is therefore an M . So A cannot be an \hat{A} . Recalling that (D) becomes (P) for $-b_i$ proves the Lemma.

Lemma 9. If A is an \hat{A} and if the condition of Theorem 3 is satisfied by one and only one row of A , then the elements of (r) can be partitioned into mutually exclusive sets each containing two elements such that there exists a solution to

$$\alpha_i a_i + \alpha_j a_j + \alpha_c C = 0 \quad \alpha_i, \alpha_j \neq 0.$$

PROOF. Assume A is an \hat{A} and that the condition of Theorem 3 is satisfied by one and only one row of A . Choose any member of (r) , say a_i , and partition the members of (r) with respect to a_i as follows.

H = the number of elements in a set (H) where $(H) = \{a_j \in (r) \mid \text{a solution exists to } \alpha_i a_i + \alpha_j a_j + \alpha_c C = 0, \alpha_i, \alpha_j \neq 0\}$. Notice that $a_i \in (H)$ and by Lemma 8, (H) contains more than one element.

$b_i = \{\text{some } b \in K \mid \text{for all } a \in (H), ab_i = 0 \text{ and for all } a \in (r) \cap (\overline{H}), ab_i \neq 0\}$.

$b_j = \{\text{some } b \in K \mid \text{for all } a \in (H), ab_j \neq 0\}$.¹³

P = the number of elements in a set (P) where $(P) = \{a \in (r) \cap (\overline{H}) \mid ab_i > 0\}$.

D = the number of elements in a set (D) where $(D) = \{a \in (r) \cap (\overline{H}) \mid ab_i < 0\}$.

Q = the number of elements in a set (Q) where $(Q) = \{a \in (H) \mid ab_j > 0\}$.

L = the number of elements in a set (L) where $(L) = \{a \in (H) \mid ab_j < 0\}$.

Observe that

$$r = Q + L + P + D.$$

By arguments used previously, there exists no semipositive solution to

$$y \begin{bmatrix} P \\ Q \\ C \end{bmatrix} = 0.$$

So, if A is to be an \hat{A}

$$(11) \quad P + Q \leq \frac{r}{2}.$$

¹³ The existence of such a b_j involves an assumption that $\alpha_i a_i + \alpha_c C = 0$ has no solution for any $a \in (H)$. No confusion should result since a_i could be so chosen.

The same argument gives

$$(12) \quad P + L \leq \frac{r}{2}.$$

Since (D) becomes (P) for $-b_i$, we have

$$(13) \quad D + L \leq \frac{r}{2}$$

$$(14) \quad D + Q \leq \frac{r}{2}.$$

Recalling that $r = Q + L + P + D$, it can be shown that

$$(15) \quad L = Q.$$

Since $L + Q = H$, the lemma is proved.

Theorem 4. If the condition of Theorem 3 is satisfied by one and only one row of A , A is an \hat{A} if and only if the elements of (r) can be partitioned into pairs such that there exists a solution to

$$y_i a_i + y_j a_j + y_c C = 0 \quad \begin{array}{l} y_i, y_j > 0 \\ y_c \geq 0. \end{array}$$

PROOF. Choose some element of (r) , say a_k . The elements of (r) can be partitioned with respect to a_k as follows.

H = the number of elements in a set $(H) = \{a_i \in (r) \mid \text{a solution exists to } \alpha_i a_i + \alpha_k a_k + \alpha_c C = 0, \alpha_i, \alpha_k \neq 0\}$.

By Lemma 8 and Lemma 9, the number of elements in (H) is greater than zero and even. Notice that $a_k \in (H)$.

Now partition (H) into pairs such that for the maximum possible number of pairs a solution exists to

$$y_i a_i + y_j a_j + y_c C = 0 \quad y_i, y_j > 0, y_c \geq 0.$$

Call the set of pairs for which this condition is satisfied $(H_p) \cdot (H_p)$ is a subset of (H) and contains H_p (an even number) elements.

H_n = the (even) number of elements in (H_n) where $(H_n) = (H) \cap (\overline{H_p})$.

b_i = $\{ \text{some } b \in K \mid ab_i = 0 \text{ for all } a \in H \text{ and } ab_i \neq 0 \text{ for all } a \in (r) \cap (\overline{H}) \}$.

P = the number of elements in a set (P) where $(P) = \{a \in (r) \cap (\overline{H}) \mid ab_i > 0\}$.

D = the number of elements in a set (D) where $(D) = \{a \in (r) \cap (\overline{H}) \mid ab_i < 0\}$.

There exists no semipositive solution to $y \begin{bmatrix} P \\ H_n \\ C \end{bmatrix} = 0$. This can be shown

by the method used previously. By eliminating the proper elements ($H_p/2$ in number) from (H_p) a set (H'_p) containing $H_p/2$ elements can be formed such that there exists no semipositive solution to

$$y \begin{bmatrix} P \\ H_n \\ H'_p \\ C \end{bmatrix} = 0.$$

Therefore, if A is an \hat{A}

$$P + \frac{H_p}{2} + H_n \leq \frac{r}{2}.$$

The same argument gives

$$D + \frac{H_p}{2} + H_n \leq \frac{r}{2}.$$

By noting that $r = P + D + H_p + H_n$, it can be shown that

$$H_n \leq 0.$$

The "only if" part is proved. Proof of the "if" is omitted.

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ON LOGNORMALITY AS A SUMMARY MEASURE OF CONCENTRATION

By IRWIN H. SILBERMAN*

Discussions in the recent literature [3] [5] [6] [8] [12] [13] [14] of industrial organization have focused on the problem of determining an appropriate theoretical distribution to describe the actual array of business firms and establishments, in the economy as a whole, and in particular industries. Pragmatically, the advantage of using a simple and well-known probability distribution is immense, because we can summarize the entire array of business units by the parameters of that distribution. More specifically, a particular interest has centered around the use of the lognormal distribution. If the distribution in question is lognormal, it is possible to subject its parameters to standard classical tests of significance. Thus, for the first time, we will be able to make direct statistical comparisons of the size distributions of business units, both over time and among markets.

Studies of the usefulness of particular probability distributions as summary measures of concentration have generally had two objectives: (a) development of statistical techniques to determine the goodness of fit of the distribution in question, and (b) examination of how well the growth patterns of firms conform to the stochastic processes known to generate these distributions.

Unfortunately, the results reported thus far, both as they apply to the techniques of fitting and the applicability of the stochastic processes to particular industries, are of limited assistance to the specialist in industrial organization. If a particular distribution is to be useful in the study of concentration, the statistical test of goodness of fit employed must "make do" with the sparsity of data generally available for most industries. Further, the data being fitted must be economically relevant. Finally, it must be shown that the distribution in question is appropriate over the broad array of industries to be described, or at least, that the distribution is appropriate for industries with certain readily identifiable attributes. The latter finding might allow us to gain some insights regarding the stochastic process or processes which generate the distribution in particular cases.

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As an illustration of the problems involved in satisfying these criteria, we briefly refer to a series of recent methodological papers by R. E. Quandt [11] [12], who proposes a new method of fitting, and applies the technique to several firm size distributions. Application of Quandt's technique requires data on the size of individual firms and plants operating in an industry. But across a large number of industries, such data are generally not available. In fact, Quandt draws his sample of firms in particular industries from the Dun and Bradstreet *Million Dollar Directory*, and obtains asset figures from *Moody's Manuals*. The resulting sample is biased in two respects. First, small firms will systematically be excluded because of the arbitrary cutoff. And an undetermined number of large firms will be omitted because *Moody's* does not necessarily include all such firms in an industry. Second, the general quality of the financial data supplied, together with the inclusion of all the operations of multi-product firms and the lack of uniformity in the treatment of corporate interrelationships, distort the measure of size employed.

To avoid the problems of data collection for particular industries, P. E. Hart and S. J. Prais [3, p. 151] chose as their sample "those firms which have shares quoted on the London Exchange." However, the economic meaning of the size distribution of firms in the economy as a whole is open to serious question, even if the sample were representative of the firms in the entire economy (which it was not). That is to say, pricing and output decisions are determined within particular market contexts, and it is the size distributions of sellers within these markets which should be analyzed for a meaningful study of concentration.

This paper reports the results of an attempt to comprehensively describe the seller size distributions of firms and establishments in 90 four-digit S.I.C. industries by the two-parameter lognormal function. In doing so we try to avoid the major data pitfalls encountered by other investigators. The study is based upon data collected by the Census of Manufactures, which provides the best available enumeration of companies and establishments in manufacturing. Further, we employ a statistical technique for fitting which does not require knowledge of the operations of individual business units. The only data necessary are concentration ratios generally available from published sources, and the mean and the variance of the logarithms of company and establishment size for each industry, which have been computed for us by the Census Bureau¹ without disclosing individual operations.

Section I describes the statistical test to determine the goodness of fit of the lognormal distribution, essentially by comparing observed and theoretical values along the Lorenz curve. Though the test places pri-

¹ The Census Bureau has a running computer program written by Nelson [9] to compute the required statistics.

mary emphasis upon the extreme upper tail of the distribution, where the difference between the lognormal and other highly skewed distributions is most pronounced, it can also successfully discriminate at lower-size classes of firms and establishments.

Section II discusses the data which we use to test the lognormal hypothesis. In Section III, we analyze our results and attempt to ascertain whether lognormality is systematically related to particular characteristics of the distributions. We conclude by arguing that though the lognormal hypothesis cannot be rejected for specific industries, it is inappropriate to consider the function as a generalized statistical summary measure of the industry size distributions in manufacturing.

I. *The Test for Lognormality*

We propose to determine the goodness of fit of the two-parameter lognormal distribution, and in particular, of the extreme upper tail of that distribution, by testing the difference between *actual* concentration ratios, e.g., the market shares of the top 4, 8, 20, and 50 business units, and *expected* concentration ratios, based on the assumption that the observed distribution of the size of business units can be described by the lognormal. In other words, we compute the mean and the variance of the logarithms of size of the actual distribution, and assume that they are the parameters of a lognormal distribution. We then compute the hypothetical concentration ratios implied by this assumed lognormal distribution, and compare them with the actual concentration ratios. If they are not significantly different, we infer that the upper tail of the actual distribution approximates the lognormal.²

² The essential elements of the mathematical derivation of the hypothetical concentration ratio for the lognormal distribution were presented in a 1954 paper by J. Aitchison and J. A. C. Brown [1]. In essence, the aim of that paper was to show the relationship which exists between the variance of the logarithms of the variates in a lognormal distribution and the Lorenz and Gini coefficients of concentration. Derivation of the Lorenz and Gini coefficients, however, requires knowledge of all the points on the Lorenz curve, and each point is given by what Aitchison and Brown call the first moment distribution, $F_1(x)$. The first moment distribution represents the proportion of total income accruing to persons with incomes not greater than x . But each point on the Lorenz curve is nothing more than what is in the literature referred to as a "relative" concentration ratio, i.e., the proportion of total income, assets, shipments, or employment accounted for by a given *proportion* of persons or business units.

Though our method requires the calculation of "absolute" concentration ratios, i.e., the share of total income or shipments accounted for by a given *number* of persons or business units, these can be directly obtained from the relative concentration ratios, so long as the number of firms is known. Consequently, our technique for deriving the hypothetical concentration ratios is essentially no different from the technique proposed by Aitchison and Brown, and as a result, $F_1(x)$ is equivalent to our $[m'(x_0)/m']$ (hypothetical concentration ratio based on the assumption that the distribution is lognormal).

In addition, there is presented in Aitchison and Brown [2, App. Table A1], for various values of σ^2 (variance of logarithms) the corresponding Lorenz coefficients, given that the distribution is lognormal. From these coefficients the points on the Lorenz curve (and hence the

The proposed method is flexible in that it can be used to test the share of the total market accounted for by the top 2, 3, or 4 business units, or the share of the top 20, 50, or 100 units, or of any other number of business units. There is nothing sacred about our use in this paper of the top 4, 8, 20, and 50 business units, but we employ them here because such information is generally available in government published statistics.

Glossary of Terms

The following notation appears in our technical description of the test.

$p(x_0)$ is equal to q/n , where $q=4, 8, 20$, and 50 , and n is equal to the total number of firms in the industry. Thus, in an industry having 100 firms, $p(x_0)$ will take on the values .04, .08, .20, and .50. Further, x_0 is the size associated with the q^{th} firm. Thus $p(x_0)$ may also be viewed as the proportion of total firms in the industry having a size equal to and greater than x_0 .

$m(x_0)$ is technically known as the incomplete first moment. It is equal to the sum of the firm sizes of the top q firms, divided by n , or the sum of firm sizes equal to and greater than x_0 , divided by n . Hence:

$$(1) \quad m(x_0) = \frac{\sum_{i=n-(q-1)}^n x_i}{n}.$$

m is the mean size of firm in the industry and is defined as:

$$(2) \quad m = \frac{\sum_{i=1}^n x_i}{n}.$$

$\frac{m(x_0)}{m}$ is then very simply:

hypothetical concentration ratios) may be determined.

Further, in a paper published in 1957, Hart [4] calculated, based on the relations presented in [1], the hypothetical top 3 and top 10 concentration ratios for the British brewing industry for a few selected years, and compared them with the actual ratios. The calculations were performed using the Prais slide rule. (This technique is mentioned in [3, p. 160] but its exact nature is not clear.) Hart, however, presents no statistical technique for determining whether the observed differences between the actual and hypothetical ratios are significant.

The method as utilized in this paper, although based on essentially the same theoretical considerations, has the advantage that once the number of business units, and the mean and the variance of the logarithms of size are given, the hypothetical concentration ratios can be computed directly. Consequently, it was possible to develop a computer program which could perform this process and facilitate the handling of large quantities of data.

$$(3) \quad \frac{\sum_{i=n-(q-1)}^n x_i}{\sum_{i=1}^n x_i} .$$

It will be noticed that the quantity $[m(x_0)/m]$ is the top q concentration ratio, or alternatively, it is the market share of firms having size greater than and equal to x_0 .

$m'(x_0)$ is the incomplete first moment of the lognormal distribution.

m' is the mean of a lognormal distribution, equal to $e^{(M+1/2V^2)}$ where M and V^2 are respectively, the mean and the variance of the logarithms of firm size.

$\frac{m'(x_0)}{m'}$ is then the top q concentration ratio, based on the assumption that the x_i 's are lognormally distributed.

The Test for the Lognormal Distribution

Let x equal the size variable under consideration. The size distribution of business units can then be summarized by a relative frequency function $f(x)$, with the mean of x denoted by m , and the variance of x denoted by v^2 .

The proportion, p , of business units in the top bracket, i.e., above size x_0 is given by:

$$(4) \quad p(x_0) = \int_{x_0}^{\infty} f(x) dx.$$

Define the incomplete first moment, $m(x_0)$, by

$$(5) \quad m(x_0) = \int_{x_0}^{\infty} xf(x) dx.$$

Then, the proportion of total industry size accounted for by the business units in the bracket p is given by $[m(x_0)/m]$, i.e.,

$$(6) \quad \frac{m(x_0)}{m} = \frac{\int_{x_0}^{\infty} xf(x) dx}{\int_0^{\infty} xf(x) dx} .$$

Though the development up to this point suggests that we are computing a "relative" concentration ratio, i.e., the share of total industry size accounted for by $p(x_0)$, the *proportion of firms* or establishments having

size greater than x_0 , we are in fact also computing an "absolute" concentration ratio, i.e., the market share of the 4, 8, 20, or 50 *largest firms* or establishments. This equivalence results from our fixing the fraction $p(x_0)$ to take on the values $4/n$, $8/n$, $20/n$, or $50/n$, where n is the total number of firms or establishments in the industry. For example, in an industry with 200 firms, the top four concentration ratio is obtained by setting $p(x_0)$ equal to .02. With 400 firms, $p(x_0)$ would be fixed at .01. Since concentration ratios are generally available only for the largest firms, we shall in the empirical sections below allow $p(x_0)$ to vary among industries, and deal with the top 4, 8, 20, and 50 concentration ratios.

Let us now denote $\ln x = X$. If $f(x)$ is a lognormal distribution, then X is normally distributed, with

$$(7) \quad \text{mean} = M = \ln(m) - \frac{1}{2}V^2,$$

and

$$(8) \quad \text{variance} = V^2 = \ln\left(1 + \frac{v^2}{m^2}\right).$$

Equation (4) can therefore be expressed as

$$(9) \quad p(X_0) = \int_{x_0}^{\infty} N(X | M, V^2) dX.$$

Since we know the values of M , V^2 , and $p(X_0)$, we can compute X_0 from equation (9) and a table of the normal distribution. We can then compute x_0 from the relation $X_0 = \ln x_0$.

The incomplete first moment of $f(x)$, when $f(x)$ is known to be lognormal [7, p. 158, equation 7.8a] can be expressed as:

$$(10) \quad m' - m'(x_0) = \int_0^{x_0} xf(x) dx = e^{(M+\frac{1}{2}V)} F_{N*}[k(x_0)]$$

where $F_{N*}[k(x_0)]$ is the standardized normal distribution function valued at $k(x_0)$, and

$$(11) \quad k(x_0) = \frac{(X_0 - M)}{V} - V.$$

Then, under the assumption of lognormality, the theoretical value which corresponds to $[m(x_0)/m]$ in equation (6) is given by

$$(12) \quad \frac{m'(x_0)}{m'} = 1 - \frac{e^{(M+\frac{1}{2}V)} F_{N*}[k(x_0)]}{e^{(M+\frac{1}{2}V^2)}} \\ = 1 - F_{N*}[k(x_0)].$$

If the assumption of lognormality is satisfied, $[m(x_0)/m] - [m'(x_0)/m']$ should not be significantly different from zero. Hence the difference between $[m(x_0)/m]$ and $[m'(x_0)/m']$ can be considered an indication of the goodness of the lognormal assumption.

We employ a test of significance based on the assumption that $[m(x_0)/m]$ is normally³ distributed. Since we treat $[m'(x_0)/m']$ as a constant (it is computed through the use of equation (12) above), $\sigma^2_{[m'(x_0)/m']} = 0$. Hence the variance of $[m(x_0)/m] - [m'(x_0)/m']$ is simply the variance of $[m(x_0)/m]$.

To obtain an estimate of the variance of $[m(x_0)/m]$, we simulate, assuming that firm sizes are lognormally distributed, with population parameters M (mean of logarithms of firm size) and V^2 (variance of logarithms of firm size).

The Simulation in Theory

The distribution of the concentration ratios from a specified lognormal population (M and V^2 given) can be easily simulated [2, pp. 28-29]. Suppose that we wish to construct a sample of size n from a lognormal population with parameters M and V^2 . To do so, we use a table of random normal deviates. If we denote by U_i ($i = 1, \dots, n$) consecutive values from the table, then the transformation $x_i = e^{M+VU_i}$ gives a sample of size n from a lognormal population with parameters M and V^2 . After ranking from largest to smallest, the sums of the top 4, 8, 20, and 50 simulated establishment and company sizes can be easily obtained. We now sum all the x_i to obtain the simulated total industry size, and divide it into the top 4, 8, 20, and 50 sums, obtaining the

³ In addition to the normal approximation, we alternatively considered a distribution-free test of significance. As described in the next section, for each observed concentration ratio, we have simulated a sample distribution, computed from $L=100$ samples of size n drawn from a lognormal population with parameters M and V^2 . At the 5 per cent level, a two-tailed distribution-free test of significance would take the following form:

$$\begin{aligned} C_* &= (L+1) \frac{\alpha}{2} \\ &= (101) \frac{.05}{2} \\ C_* &= 2.525. \end{aligned}$$

Thus, we reject the null hypothesis that the observed ratio is from a lognormal distribution if there are fewer than 2.525 simulated values which exceed the *observed* concentration ratio. We also reject the null hypothesis if there are fewer than 2.525 simulated values less than the observed ratio.

This alternative test was performed on eighty-two 1958 value of shipments-company size distributions, and the results were compared with those obtained using the normal approximation. The comparison yielded a net change in the classifications of three industries from not lognormal to lognormal. No modification of the conclusions of this paper is indicated as a consequence of these reclassifications.

required concentration ratios. If we repeat the above experiment L times, this procedure will yield a sample distribution of the various concentration ratios. The standard errors of the ratios can then be directly computed.

Hence, the test will take the form:

$$(13) \quad |z| = \frac{\frac{m(x_0)}{m} - \frac{m'(x_0)}{m'}}{\sigma[m(x_0)/m]}.$$

Example Calculation

Given the values in Table 1 below, the calculations to determine the goodness of fit of the lognormal distribution are straightforward.

TABLE 1—STATISTICS OF THE DISTRIBUTION OF 1,999 COMPANIES, 1947,
SIZE BY VALUE OF SHIPMENTS

| | | |
|--|--------------------|---------|
| Variance of the logarithms of company size | V^2 | 3.61428 |
| Standard deviation of the logarithms of company size | V | 1.90113 |
| Mean of logarithms of company size | M | 6.02630 |
| Actual top four concentration ratio | $[m(x_0)/m]$ | .471 |
| Standard error of concentration ratio (simulated) | $\sigma[m(x_0)/m]$ | .0710 |

We have said that $F_{N^*}[k(x_0)]$ is the standardized normal distribution function valued at $k(x_0)$, and that $k(x_0)$ is equal to $[(X_0 - M)/V] - V$. Considering the share of the top four companies in the industry, we know the value of $p(x_0) = 4/1999 = .0020$. Substituting this value of $p(x_0)$ into equation (9), i.e.,

$$.0020 = \int_{x_0}^{\infty} N(X|M, V^2) dX,$$

and solving for X_0 using the table of the cumulative normal distribution, we find that $k(x_0) = .979$. Therefore,

$$F_{N^*} [.979] = .8365.$$

Subtracting from 1.0, we find the hypothetical concentration ratio, based on the assumption of lognormality, equal to .1635. To test our two ratios for a significant difference, we evaluate (13):

$$|z| = \left| \frac{.4710 - .1635}{.0710} \right| = 4.327.$$

Using a two-tailed test, at the 5 per cent level of significance, we find that the difference between the two ratios is significantly different from zero, and we conclude that the distribution in question is not lognormal.

This is so because our criteria for accepting or rejecting the null hypothesis will be the following: We first examine the distribution in the extreme upper tail. If it provides an acceptable fit in this extreme region, we then successively examine larger portions of the distribution. If the theoretical distribution does not provide an acceptable fit in the extreme upper tail, for our purposes, we reject the null hypothesis.

II. *The Data*

For the Census years 1947 and 1958, we analyze, for a sample of 90 four-digit manufacturing industries (old⁴ Standard Industrial Classification) the size distributions of establishments and companies, size measured by value of shipments and unadjusted value added.

The S.I.C., which the Census Bureau employs to classify its data, was not designed to aid in measuring market structure, and its shortcomings are well known. However, we shall here employ the classification system because it does provide us with the only available across-the-board data on manufacturing with which to test the lognormal hypothesis.

A few additional caveats and explanations are in order. First, 1947 and 1958 were years of expansion and deflation, respectively, and as a result, establishments and companies may appear either larger or smaller than they really are in any permanent sense. Second, our initial measure of size is value of shipments. However, when value of shipments is totaled for all establishments, industry by industry, the sums will contain some duplication, since the value of manufactured products includes the value of the manufactured materials from which they were made. Hence, as our second measure of size, we consider value added, which is a "net" concept and is additive without duplication. Though Census began publishing adjusted value added in 1958, we study the *unadjusted* data to maintain consistency between 1947 and 1958.⁵

Finally, Census definitions of establishments and companies are not at all clear. For example, an establishment is not necessarily identical with a plant. If a single plant engages in distinctly separate lines of activity, it must submit a separate report for each activity, and each

⁴ The industry classifications used in this study are based on the 1945 S.I.C. code, as amended. A revised edition of the S.I.C. *Manual* was issued in 1957, and the final reports and volumes of the 1958 *Census of Manufactures* are based upon this new classification system. The preliminary reports of the 1958 Census were, however, tabulated on the 1945 code. In order to maintain consistency in the classifications between 1947 and 1958, we decided to base our work on the old code.

⁵ Unadjusted value added is computed by subtracting from value of shipments the actual cost of materials consumed, assuming all products produced in a given year are sold in that year. Adjusted value added takes into account both the trade operations of manufacturing concerns and the change in work-in-process and finished goods inventories between the beginning and end of the year.

TABLE 2—FOUR-DIGIT INDUSTRY CLASSIFICATIONS IN THE SAMPLE

| | | | |
|------|------|------|-------------------|
| 2011 | 2825 | 3352 | 3591 |
| 2031 | 2834 | 3391 | 3593 |
| 2033 | 2893 | 3411 | 3611 |
| 2043 | 2898 | 3425 | 3612 |
| 2052 | 2911 | 3431 | 3613 |
| 2062 | 2952 | 3463 | 3614 |
| 2063 | 3011 | 3489 | 3615 |
| 2071 | 3021 | 3493 | 3621 |
| 2073 | 3141 | 3494 | 3641 |
| 2082 | 3221 | 3511 | 3651 |
| 2085 | 3229 | 3531 | 3661 ^a |
| 2092 | 3253 | 3541 | 3662 |
| 2111 | 3261 | 3542 | 3691 |
| 2121 | 3262 | 3554 | 3692 |
| 2234 | 3263 | 3561 | 3715 |
| 2252 | 3264 | 3562 | 3722 |
| 2295 | 3275 | 3564 | 3742 |
| 2432 | 3293 | 3565 | 3751 |
| 2522 | 3321 | 3572 | 3842 |
| 2661 | 3322 | 3576 | 3871 |
| 2671 | 3323 | 3581 | 3914 |
| 2812 | 3333 | 3583 | 3943 |
| 2823 | 3351 | 3585 | |

^a 1947 only.

activity will then be treated by Census as a specific establishment. On the other hand, two plants belonging to the same company may be considered as one establishment by Census if they are, say, located in the same county. A company, on the other hand, is defined as all the establishments which enjoy common ownership or control, and which are classified in the same four-digit industry. Thus, one would expect to find a multiplant firm classified in as many industries as it has establishments operating in diverse four-digit categories. The substance of Census treatment, however, is that establishments are grouped together into companies on the initiative of the reporting companies, i.e., based on the affiliations admitted by respondents. To the extent that companies do not report corporate interrelationships, there is an overstatement in the number of companies, and an understatement in their sizes.

Analysis of the Sample

The 90⁶ four-digit Census industries studied in this paper are drawn exclusively from the list of 142 industries selected for analysis by R. L. Nelson [9] in his monograph on industrial concentration,⁷ and were

⁶ Ninety industries for 1958; ninety-one for 1947.

⁷ The reader is referred to Chapter II of Nelson's monograph for a detailed discussion of the selection criteria, as well as for discussions regarding reconstruction of the basic data files, preparation of the input tapes, the computer program, etc.

Our original plan was to select a random sample of industries for analysis. We found, how-

TABLE 3—DISTRIBUTION AMONG TWO-DIGIT GROUPS OF NINETY^a INDUSTRY SAMPLE IN 1958

| Two-Digit Group | Number of Industries in Sample | Number of Industries in Census ^b | Coverage Per Cent |
|-----------------|--------------------------------|---|-------------------|
| 20 | 12 | 42 | 28.5 |
| 21 | 2 | 4 | 50.0 |
| 22 | 3 | 32 | 9.3 |
| 23 | 0 | 41 | 0.0 |
| 24 | 1 | 18 | 5.5 |
| 25 | 1 | 15 | 6.6 |
| 26 | 2 | 12 | 16.6 |
| 27 | 0 | 16 | 0.0 |
| 28 | 6 | 41 | 14.6 |
| 29 | 2 | 5 | 40.0 |
| 30 | 2 | 4 | 50.0 |
| 31 | 1 | 12 | 8.3 |
| 32 | 9 | 29 | 31.0 |
| 33 | 7 | 19 | 36.8 |
| 34 | 7 | 30 | 23.3 |
| 35 | 16 | 41 | 39.0 |
| 36 | 11 | 21 | 52.4 |
| 37 | 4 | 14 | 28.5 |
| 38 | 2 | 10 | 20.0 |
| 39 | 2 | 38 | 5.2 |
| Total | 90 | 444 ^c | 20.3 |

^a Industry 3661 included in 1947 only.

^b Calculated from [16, Pt. I, Table 2].

^c [16, Pt. II, Table 8] lists the total number of industries as 443. Discussions with the Census Bureau confirmed, however, that industry 2443, Cigar Boxes, was inappropriately omitted.

selected from that list on the basis of comparability of the classifications between 1947 and 1958.

Table 2 presents a list of the 91 four-digit industry classifications in the sample. Table 3 describes our sample for 1958 in terms of two-digit major industry categories. Included in our sample are 20.2 per cent of the 444 four-digit S.I.C. industries in manufacturing (excluding the 19 group). The coverage among industries is not uniform, ranging from none at all in major groups 23 and 27, to 52.4 per cent of the industries in major group 36.

Though the sample contains only 20.3 per cent of the four-digit industries in 1958, it does contain 27.2 per cent of total unadjusted value

ever, that the cost of reconstructing original data files, recoding, and of preparing input tapes was prohibitive. But these tasks had previously been performed for the Nelson sample of industries. Further, Nelson, whose computer program we used, had in his work already computed the mean and the variance of the logarithms of the 1947 value of shipments establishment and company distributions. It was therefore decided to draw exclusively upon the Nelson sample.

TABLE 4—UNADJUSTED VALUE ADDED IN SAMPLE (MILLIONS OF DOLLARS), NINETY 1958 CENSUS INDUSTRIES

| Two-Digit Group | Unadjusted Value Added in Sample | Total Unadjusted Value Added ^a | Coverage Per Cent |
|-----------------|----------------------------------|---|-------------------|
| 20 | 6,224 | 16,574 | 37.5 |
| 21 | 1,240 | 1,415 | 87.6 |
| 22 | 740 | 5,100 | 14.5 |
| 23 | — | 5,904 | 0.0 |
| 24 | 309 | 3,093 | 9.9 |
| 25 | 170 | 2,401 | 7.0 |
| 26 | 1,400 | 5,642 | 24.8 |
| 27 | — | 7,818 | 0.0 |
| 28 | 4,604 | 12,422 | 37.0 |
| 29 | 2,411 | 2,642 | 91.2 |
| 30 | 1,261 | 2,294 | 54.9 |
| 31 | 1,073 | 1,833 | 58.5 |
| 32 | 1,393 | 5,435 | 25.6 |
| 33 | 2,601 | 11,264 | 23.0 |
| 34 | 2,926 | 9,094 | 32.1 |
| 35 | 6,277 | 14,568 | 43.0 |
| 36 | 4,257 | 9,192 | 46.3 |
| 37 | 2,095 | 14,783 | 14.1 |
| 38 | 431 | 2,813 | 15.3 |
| 39 | 144 | 6,032 | 2.3 |
| Total | 38,213 | 140,319 | 27.2 |

^a Source: [15, p. 4].

added in manufacturing. Reference to Table 4 illustrates the large variation for particular two-digit groups, ranging from no coverage in major groups 23 and 27 to 91.2 per cent in group 29.

The sample coverage ranges from 11.4 per cent of those industries which have more than 1,000 companies, to 30.8 per cent of those industries which have less than 20 companies (see Table 5). The pattern for establishments is similar. At the 5 per cent level, a Chi-square test indicates that there is no significant difference in the coverage of industries in the various company and establishment size classes.

Finally, we find a bias of the company sample (Table 6) toward industries which are both larger (measured by value of shipments), and more highly concentrated (measured by the top four company ratio⁸ for the industry).

⁸ There is a systematic downward bias in the concentration ratios. This bias was created by the computer program not rounding the industry shares of individual companies, but simply dropping the last digit. The bias appears most strongly when a number of shares are cumulated to get the top 20 or 50 concentration ratios. We made no adjustment to correct for rounding.

TABLE 5—DISTRIBUTION OF NINETY SAMPLE INDUSTRIES, 1958, BY NUMBER OF COMPANIES^a

| Size Class: Number of Companies | Universe | Sample | Per Cent of Each Size Class in Sample |
|---------------------------------|----------|--------|---------------------------------------|
| 0-20 | 26 | 8 | 30.8 |
| 21-50 | 51 | 13 | 25.5 |
| 51-100 | 69 | 16 | 23.0 |
| 101-200 | 68 | 14 | 20.6 |
| 201-500 | 103 | 20 | 19.4 |
| 501-1,000 | 57 | 11 | 19.3 |
| 1,001 and over | 70 | 8 | 11.4 |
| Total | 444 | 90 | 20.3 |

^a Derived from [16, Pt. I, Table 2].

TABLE 6—INDUSTRIES IN SAMPLE AS PROPORTION OF TOTAL NUMBER OF INDUSTRIES IN MANUFACTURING

| Value of Shipments (millions) | Concentration Ratio Class | | | | | Total |
|-------------------------------|---------------------------|---------|---------|---------|--------|-------|
| | 80-100 | 60-79.9 | 40-59.9 | 20-39.9 | 0-19.9 | |
| 1,000.0 and over | .000 | .555 | .455 | .313 | .227 | .325 |
| 500.0-999.9 | .333 | .750 | .500 | .364 | .154 | .348 |
| 200.0-499.9 | .375 | .470 | .321 | .121 | .000 | .200 |
| 100.0-199.9 | .286 | .125 | .211 | .055 | .000 | .143 |
| 50.0-99.9 | .500 | .000 | .238 | .055 | .000 | .136 |
| Less than 50 | .000 | .000 | .042 | .000 | .000 | .016 |
| Total | .276 | .323 | .257 | .171 | .090 | .203 |

III. Results of the Test for Lognormality

We now present the results of our test for lognormality of the size distributions of establishments and companies for 1947 and 1958.

Classification of the Results

As has been indicated, the distinction between the lognormal distribution and other members of the lognormal family is most pronounced in the extreme upper tail of the distribution, and it is therefore this region that is important when testing for lognormality. If the extreme upper tail of the distribution is not lognormal, then for our purposes, the distribution is not lognormal.

We establish the following criteria to evaluate the results of our lognormal test.

1. Our primary interest is in the extreme upper tail of the distribution, i.e., whether there is a significant difference between the actual and hypothetical concentration ratios at the top four level.

2. If there is a significant difference between the hypothetical and actual concentration ratios at the top four level, we immediately reject the null hypothesis that the distribution is lognormally distributed. If there is no significant difference at the top four level, we continue to test by examining the difference between successively larger concentration ratios, i.e., top 8, 20, and 50. If we find a significant difference at any of these levels, even though the extreme upper tail shows no significant difference, we again reject the null hypothesis that the distribution is lognormal.

3. We eliminate from the statistical analysis those industries which had twenty or fewer firms or establishments, since a determination of lognormality would have to be based, at a maximum, on the comparison of only two concentration ratios (top four and top eight). We consider this little information as insufficient to make a determination of the shape of the distribution.

Based upon these criteria, the following categories of results were established.

Category 1. The upper tail of the distribution is lognormal. If the actual top 4 concentration ratio is not significantly different from the hypothetical top 4 concentration ratio and, in addition, the top 8, 20, and 50 ratios are also not significantly different, we *do not reject* the null hypothesis that the extreme upper tail of the distribution is lognormal.

Category 2. The upper tail of the distribution is lognormal, but we are unable to test successively higher ratios. In this category, we include those industries where we found no significant difference between the actual and hypothetical top 4 concentration ratios, but Census disclosure of information prohibits us from testing at successively higher ratios. In later analysis this category will be excluded.⁹

Category 3A. The upper tail of the distribution is lognormal, but we find a significant difference when we test successively higher ratios. In this category we include those industries for which we found no significant difference between the actual and hypothetical top 4 concentration ratios, but for which we did find significant differences when we tested at the top 8, 20, or 50 levels. We consider this finding as sufficient to *reject* the lognormal hypothesis.

Category 3B. Disclosure prohibits testing for lognormality at the top four concentration ratio, but we find significant differences at successively higher ratios. We are unable to test the actual and hypothetical top four concentration ratios for a significant difference because of Census disclosure, but we do find significant differences at higher levels, e.g., top

⁹ For the 1947 value of shipments-establishment distributions, where the number of such cases is large, we alternatively consider them as "lognormal."

TABLE 7—SUMMARY OF ACTUAL PATTERNS OF RESULTS OF LOGNORMAL TEST ON CENSUS DATA, BY CATEGORY

| Category | Concentration Ratio, Top | Actual Pattern Exhibited ^a |
|---|--------------------------|---------------------------------------|
| 1. Upper tail of distribution is lognormal | 4 | YY |
| | 8 | YY |
| | 20 | YY |
| | 50 | Y- |
| 2. Upper tail is lognormal, but we are unable to test successively higher ratios | 4 | YYYYYY |
| | 8 | DYYDYD |
| | 20 | YDYDDY |
| | 50 | YYDY-- |
| 3A. Upper tail is lognormal, but we find a significant difference when we test successively higher ratios | 4 | YYYYYYYYYYYYYY |
| | 8 | DYYYYNNNYNDDN |
| | 20 | YNNNNNNNDYNDY |
| | 50 | NNN-YN-YNYNNN |
| 3B. Disclosure at top 4 ratio but we find significant differences at higher ratios | 4 | DDDDDDDDDDDDDDDD |
| | 8 | YDNNNNNNDDYDYN |
| | 20 | NYDYNDNYDNYNNND |
| | 50 | NN--NNYNNNN--N |
| 4. Upper tail is <i>not</i> lognormal | 4 | NNNNNNNNNNNNNNNN |
| | 8 | YNNNDDNDNDNNYDDYY |
| | 20 | YYNNYYYYYNNNDDYDDNY |
| | 50 | YYYN-Y-N--N-NN-N-- |
| 5. We are unable to determine the lognormality of the extreme upper tail | 4 | DDDDDDDDDD |
| | 8 | DDDDYYYYD |
| | 20 | DYDYDYDYDD |
| | 50 | D-YY--YY- |
| 6. No analysis performed (20 or fewer establishments or companies) | 4 | |
| | 8 | |
| | 20 | |
| | 50 | |

^a Notes: Y—No significant difference between hypothetical and actual concentration ratios.

N—Significant difference between hypothetical and actual concentration ratios.

D—Disclosure.

--Less than specified number of establishments or companies in the industry.

8, 20, or 50. Again we consider this pattern as sufficient to *reject* the lognormal hypothesis.

Category 4. The extreme upper tail is not lognormal. We find a significant difference between actual and hypothetical top 4 concentration ratios. This finding leads us to *reject* the null hypothesis that the upper tail is lognormal.

Category 5. We are unable to determine the lognormality of the extreme upper tail. Disclosure at the top four level prevents our testing for a

TABLE 8—DISTRIBUTION OF EIGHT SAMPLES BY CATEGORY OF RESULT

| | Category | | | | | | | Total Number of Industries |
|--|----------|----|----|----|----|----|---|-------------------------------|
| | 1 | 2 | 3A | 3B | 4 | 5 | 6 | |
| 1958 Value of shipments—companies | 42 | | 31 | | 9 | | 8 | 90 |
| 1958 Value of shipments—establishments | 14 | 3 | 13 | 32 | 17 | 8 | 3 | 90 |
| 1958 Value added—companies | 41 | 2 | 27 | | 12 | | 8 | 90 |
| 1958 Value added—establishments | 15 | 3 | 13 | 23 | 26 | 7 | 3 | 90 |
| 1947 Value of shipments—companies | 36 | 1 | 28 | | 16 | 2 | 8 | 91 |
| 1947 Value of shipments—establishments | 7 | 10 | 19 | 16 | 24 | 13 | 2 | 91 |
| 1947 Value added—companies | 30 | | 31 | | 20 | 2 | 8 | 91 |
| 1947 Value added—establishments | 14 | 1 | 20 | 10 | 40 | 4 | 2 | 91 |

significant difference between the concentration ratios. Further, we have no evidence which would lead us to reject the lognormal hypothesis at the top 8, 20, and 50 levels. This category of result is therefore excluded from the analysis.

Category 6. No analysis performed. As stated above, we exclude from analysis all industries which had twenty or fewer establishments or companies.

Given the categories detailed above, we present in Table 7 the various patterns of results obtained in our tests of Census data, and their appropriate classification into one of the seven categories discussed above.

In Table 8 we present the distribution of our eight samples, by category of result of the lognormal test. Thus, we find for the 1958 value of shipments-company distributions, the extreme upper tails of 42 of our sample of 90 industries are lognormally distributed (i.e., classified in category 1). On the other hand, a total of 40 industries are classified as "not lognormal" (31 in category 3A, none in category 3B, and nine in category 4). It should be noted that the four "company" samples contain the highest proportion of industries classified as lognormal. The important point, however, is *that the lognormal distribution does not provide us with a universal description of the size distributions of sellers.*

TABLE 9—DIVERGENCE AMONG DISTRIBUTIONS BASED ON VALUE OF SHIPMENTS AND VALUE ADDED: INDUSTRIES CATEGORIZED "LOGNORMAL"

| | 1958 Companies | 1958 Establish- ments | 1947 Companies | 1947 Establish- ments |
|---|-------------------|-----------------------------|-------------------|-----------------------------|
| Number of industries in sample classified as "lognormal" when size measure is value of shipments | 42 | 14 | 36 | 7 |
| Number of industries in sample classified as "lognormal" when size measure is <i>both</i> value of shipments and value added | 36 | 10 | 28 | 4 |
| Number of industries in sample classified as "lognormal" when size is value of shipments, but <i>not</i> categorized as "lognormal" when value added is measure of size | 6 | 4 | 8 | 3 |
| <i>Value Added Categorization</i> | | | | |
| Category 2 | — | 2 | — | — |
| Category 3A | 3 | — | 7 | 1 |
| Category 3B | — | — | — | — |
| Category 4 | 3 | — | 1 | 2 |
| Category 5 | — | 2 | — | — |
| Category 6 | — | — | — | — |
| Number of industries in sample classified as "lognormal" when size measure is value added | 41 | 15 | 30 | 14 |
| Number of industries in sample classified as "lognormal" when size measure is <i>both</i> value of shipments and value added | 36 | 10 | 28 | 4 |
| Number of industries in sample classified as "lognormal" when size is value added, but <i>not</i> categorized as "lognormal" when value of shipments is measure of size | 5 | 5 | 2 | 10 |
| <i>Value of Shipments Categorization</i> | | | | |
| Category 2 | — | 2 | — | 5 |
| Category 3A | 5 | 1 | 2 | — |
| Category 3B | — | — | — | — |
| Category 4 | — | — | — | — |
| Category 5 | — | 2 | — | 5 |
| Category 6 | — | — | — | — |

Size Measures

There is a clear indication that value of shipments and value added, when employed as alternative measures of the size of companies and establishments, are highly (though not perfectly) related. Such correspondence is well illustrated by reference to the 1958 company distributions. (See Tables 9 and 10.) Here we find 36 industries, or 45 per cent of those in the sample (excluding industries with less than 20 companies) are classified lognormal when size is *both* value of shipments and value added. Only in five industries is the value added classification log-

TABLE 10—DIVERGENCE AMONG DISTRIBUTIONS BASED ON VALUE OF SHIPMENTS AND VALUE ADDED: INDUSTRIES CATEGORIZED "NOT LOGNORMAL"

| | 1958 Companies | 1958 Establish- ments | 1947 Companies | 1947 Establish- ments |
|---|-------------------|-----------------------------|-------------------|-----------------------------|
| Number of industries in sample "not lognormal" when size measure is value of shipments | 40 | 62 | 44 | 59 |
| Number of industries in sample classified as "not lognormal" when size measure is <i>both</i> value of shipments and value added | 33 | 60 | 42 | 59 |
| Number of industries in sample classified "not lognormal" when size is value of shipments but <i>not</i> categorized "not lognormal" when value added is measure of size | 7 | 2 | 2 | 0 |
| <i>Value Added Categorization</i> | | | | |
| Category 1 | 5 | 1 | 2 | — |
| Category 2 | 2 | — | — | — |
| Category 5 | — | 1 | — | — |
| Category 6 | — | — | — | — |
| Number of industries in sample "not lognormal" when size measure is value added | 39 | 62 | 51 | 70 |
| Number of industries in sample classified as "not lognormal" when size measure is <i>both</i> value of shipments and value added | 33 | 60 | 42 | 59 |
| Number of industries in sample classified as "not lognormal" when size is value added, but <i>not</i> categorized as "not lognormal" when value of shipments is measure of size | 6 | 2 | 9 | 11 |
| <i>Value of Shipments Categorization</i> | | | | |
| Category 1 | 6 | — | 8 | 3 |
| Category 2 | — | — | 1 | 4 |
| Category 5 | — | 2 | — | 4 |
| Category 6 | — | — | — | — |

normal when the value of shipments classification is not lognormal. Further, 33 industries or 40 per cent of those in the sample were identically found not lognormal when both measures of size are employed. In only about half a dozen industries is there a divergence in results between the value of shipments and value added measures.

We can conceive of two hypotheses to explain the regularity exhibited by the two alternative measures of size. The first is that the same factors making for lognormality of the size distributions operate on both value of shipments and value added. Unfortunately, the individual firm and establishment size data necessary to test this hypothesis are not available from the Census Bureau.

TABLE 11—COMPARISON OF RESULTS BETWEEN 1947 AND 1958 VALUE OF SHIPMENTS-COMPANY DISTRIBUTIONS

| 1947 Category \ 1958 Category | 1 | 2 | 3A | 3B | 4 | 5 | 6 | Total |
|-------------------------------------|----|---|----|----|---|---|---|-------|
| 1 | 28 | | 7 | | | | | 35 |
| 2 | | | | | | | 1 | 1 |
| 3A | 7 | | 16 | | 4 | | 1 | 28 |
| 3B | | | | | | | | 0 |
| 4 | 5 | | 6 | | 5 | | | 16 |
| 5 | 1 | | 1 | | | | | 2 |
| 6 | 1 | | 1 | | | | 6 | 8 |
| Total | 42 | | 31 | | 9 | | 8 | 90 |

Second, value added is derived (or estimated) from value of shipments. It may indeed be that the procedures employed are biased in such a way as to yield identically shaped distributions. In any case, one fact is clear. In the analyses which follow, we expect little or no divergence in conclusions on account of our measure of size.

Divergence in Results: 1947 and 1958

The finding of correspondence between our alternative size measures is closely paralleled when we compare the 1947 and 1958 results. Table 11 presents, for the value of shipments-company distributions, a comparison of results by year. Due to space considerations, we omit comparable tables for the other samples. Forty-two industries were classified as lognormal in 1958, while 35 were so classified for 1947. Interestingly, 28 industries were identically classified in both years. Further, 31 industries were identically classified as not lognormal in both years. Thus, a total of 59 industries or 72 per cent of the sample (excluding those industries with less than 20 firms) were identically classified in both 1947 and 1958. Based on these limited data, we conclude that the applicability of the lognormal function as a descriptive device is more or less uniform for the two years under investigation. That is to say, if an industry was lognormal in 1947, there is a high probability that it will be so classified in 1958. Conversely, if the business units in an industry were not lognormally distributed in 1947, the probability of their having been lognormally distributed in 1958 is small.¹⁰

¹⁰ This statement implies nothing about the shapes of the distributions in the intervening years.

TABLE 12—SUMMARY RESULTS OF CHI-SQUARE TESTS TO DETERMINE THE RELATIONSHIP OF LOGNORMALITY TO SIZE OF CLASSIFICATION

| Group | Degrees of Freedom | χ^2 | $\chi^2_{.05}$ | Results |
|--|--------------------|--------------------------------|----------------|---------------------------|
| 1947 Value of shipments—companies | 4 | 13.216 | 9.488 | significant difference |
| 1947 Value of shipments—establishments | 2 | No Test Performed ^a | | no significant difference |
| 1947 Value of shipments—establishments (Category 2 considered "lognormal") | | 0.042 | 5.991 | |
| 1947 Value added—companies | 3 | 1.966 | 7.815 | no significant difference |
| 1947 Value added—establishments | 1 | 0.0 | 3.841 | no significant difference |
| 1958 Value of shipments—companies | 3 | 5.206 | 7.815 | no significant difference |
| 1958 Value of shipments—establishments | 1 | 0.0 | 3.841 | no significant difference |
| 1958 Value added—companies | 4 | 3.658 | 9.488 | no significant difference |
| 1958 Value added—establishments | 2 | 2.909 | 5.991 | no significant difference |

^a Only seven industries classified in Category 1, lognormal tail.

Relationship of Lognormality to Particular Industry Characteristics

We have thus far found that the lognormal distribution is inappropriate as a generalized summary measure of concentration. The distribution may still be useful, however, if it can be shown that lognormality is related to specific characteristics of particular industries. In that case, we might at least be able to use the function as descriptive of that class of industries. Further, we might gain some insights regarding the stochastic process or processes which generate the distribution in particular cases.

The characteristics to be studied are industry size (total industry value of shipments and value added), the number of establishments and companies classified in the industry, and the degree of top-four concentration in the industry. In order to determine whether lognormality is significantly related to any of these characteristics, we constructed three contingency tables for each of the eight samples of industries, and used the Chi-square test to test the null hypothesis of independence.¹¹ It should be noted, however, that in each case the number of degrees of freedom for Chi-square is limited.

At the 5 per cent level of significance, we are unable to find a con-

¹¹ Category 2 results (upper tail is lognormal, but we are unable to test the top 8, 20, and 50 ratios) are generally excluded from the analysis. However, in the case of the 1947 value of shipments-establishment distributions, where the number of category 2 results is large, we performed an alternative test considering both category 1 and 2 as lognormal.

TABLE 13—SUMMARY RESULTS OF CHI-SQUARE TESTS TO DETERMINE THE RELATIONSHIP OF LOGNORMALITY TO NUMBER OF BUSINESS UNITS

| Group | Degrees of Freedom | χ^2 | $\chi^2_{.05}$ | Results |
|--|--------------------|--------------------------------|----------------|---------------------------|
| 1947 Value of shipments—companies | 4 | 20.935 | 9.488 | significant difference |
| 1947 Value of shipments—establishments | | No Test Performed ^a | | |
| 1947 Value of shipments—establishments (Category 2 considered "lognormal") | 1 | 5.845 ^b | 3.841 | significant difference |
| 1947 Value added—companies | 4 | 14.523 | 9.488 | significant difference |
| 1947 Value added—establishments | 1 | 1.150 ^b | 3.841 | no significant difference |
| 1958 Value of shipments—companies | 4 | 32.804 | 9.488 | significant difference |
| 1958 Value of shipments—establishments | 1 | 3.817 ^b | 3.841 | no significant difference |
| 1958 Value added—companies | 4 | 22.214 | 9.488 | significant difference |
| 1958 Value added—establishments | 2 | 13.053 | 5.991 | significant difference |

^a Only seven industries classified in Category 1, lognormal tail.

^b Yates correction for continuity applied.

sistent relationship between lognormality and industry size.¹² Actual and critical values of Chi-square are presented in Table 12.

There does, however, appear to be a relationship between the occurrence of lognormality and the number of establishments and companies which comprise particular industries (see Table 13). For each sample but two, the computed value of Chi-square exceeded the critical (5 per cent level of significance). More specifically, lognormality occurs with greater relative frequency among those industries which have a large number of firms and/or establishments. The cutoff point seems to be between 100–200 business units. Conversely, nonlognormality describes a higher proportion of the industry size distributions which have, in general, less than 100 companies or plants.

It is known [10] that there exists a clear and consistent correspondence between the fewness of companies and establishments classified in an industry, and the magnitude of the top-four concentration ratio for the industry. This being the case, and given that we have found a positive relationship between lognormality and large numbers of firms and establishments, we should a priori expect to find a corresponding

¹² In only one case did we find a Chi-square which exceeded the critical value, indicating the existence of a pattern. Here, actual lognormal frequencies were greater than the hypothetical in the large value of shipments size classes, implying a possible relationship between lognormality and "large" industries.

TABLE 14—SUMMARY RESULTS OF CHI-SQUARE TESTS TO DETERMINE THE RELATIONSHIP OF LOGNORMALITY TO TOP-FOUR CONCENTRATION RATIOS

| Group | Degrees of Freedom | χ^2 | $\chi^2_{.05}$ | Results |
|--|--------------------------------|--------------------|----------------|---------------------------|
| 1947 Value of shipments—companies | 3 | 5.674 | 7.815 | no significant difference |
| 1947 Value of shipments—establishments | No Test Performed ^a | | | |
| 1947 Value of shipments—establishments (Category 2 considered "lognormal") | 2 | 0.514 | 5.991 | no significant difference |
| 1947 Value added—companies | 3 | 1.297 | 7.815 | no significant difference |
| 1947 Value added—establishments | 1 | 0.018 ^b | 3.841 | no significant difference |
| 1958 Value of shipments—companies | 3 | 3.047 | 7.815 | no significant difference |
| 1958 Value of shipments—establishments | 1 | 2.114 ^b | 3.841 | no significant difference |
| 1958 Value added—companies | 4 | 2.781 | 9.488 | no significant difference |
| 1958 Value added—establishments | 2 | 1.581 | 5.991 | no significant difference |

^a Only seven industries classified in Category 1, lognormal tail.

^b Yates correction for continuity applied.

relationship of lognormality with low industry concentration. Surprisingly, Table 14 does not confirm the existence of such a pattern. Rather, we find no relationship between lognormality and the magnitude of the top-four concentration ratio in an industry.

There are clearly several possible reasons for our discovering this seeming anomaly. Thus, though we find a positive relation between the manyness of firms and establishments and lognormality, the correspondence is by no means perfect. Or, manyness of business units may in fact be related to some additional unknown variable or variables, which in turn may be related to lognormality. Other hypotheses are equally compatible with the results. In any case existing available data will not support tests of the alternatives.

Divergence Between Company and Establishment Distributions

Before concluding, we investigate the observed differences between the size distributions of establishments and companies, and attempt to explain them in terms of a simple process for generating the lognormal distribution, i.e., the "law of proportionate effect."

From Table 15 we see, for the 1958 value of shipments distributions, 22 industries classified as not lognormal by establishments, were lognormal when companies were analyzed. On the other hand, there were only two instances where the establishment distribution was lognormal,

TABLE 15—NUMBER OF INDUSTRIES CLASSIFIED "NOT LOGNORMAL": ESTABLISHMENTS AND COMPANIES COMPARED

| Distribution | 1958 Value of Shipments | 1958 Value Added | 1947 Value of Shipments | 1947 Value Added |
|---|-------------------------------|------------------------|-------------------------------|------------------------|
| (a) Number of company distributions "not lognormal" | 40 | 39 | 44 | 51 |
| (b) Number of establishment distributions "not lognormal" | 62 | 62 | 59 | 70 |
| (c) Number of industries classified in <i>both</i> (a) and (b) above | 36 | 37 | 37 | 46 |
| Establishment distribution classified as "not lognormal," but company distribution classified as below: | <u>26</u> | <u>25</u> | <u>22</u> | <u>24</u> |
| Category 1 (lognormal) | 22 | 19 | 16 | 17 |
| 2 | — | 1 | 1 | — |
| 5 | — | — | 1 | 1 |
| 6 | 4 | 5 | 4 | 6 |

and the company distribution was not. Similar patterns exist for the other samples.

These findings suggest the following hypothesis. The simple law of proportionate effect states that the chance of a given proportionate increase in size is the same for business units of all sizes. However, though the law may appear reasonable for companies because of the various paths of growth possible, e.g., addition of plants, diversification, and vertical integration, it would not, a priori, necessarily appear to hold for establishments, whose growth is more narrowly limited by increasing production or transport costs, or other physical and market constraints. If this is so, it is oftentimes only the company which can maintain the necessary growth consistent with the law of proportionate effect.

Consequently, in those industries where the establishment distributions are not lognormal, while the company distributions are, the difference between the top-four company and establishment concentration ratios should be greater than the difference between the ratios when both the establishment and company distributions for an industry are lognormal. This excess should exist because in the former case, constraints on the growth of large establishments prevent their achieving the large size consistent with the law of proportionate effect. That is to say, the greater the difference in the concentration ratios, the greater the size of the largest companies relative to the size of the largest establishments.

Table 16 presents the results of the test. For each of our four groups (1958 value of shipments, 1958 value added, 1947 value of shipments,

TABLE 16—COMPARISON OF MEAN DIFFERENCES IN TOP-FOUR COMPANY AND ESTABLISHMENT CONCENTRATION RATIOS^a

| Group | Establishment and Company Distributions —Both “Lognormal” | Establishment Distributions “Not Lognormal” —Company Distributions “Lognormal” |
|--|---|---|
| <i>1958 Value of Shipments</i> | | |
| Mean difference in top-four concentration ratios | .099 | .152 |
| Number of industries | (14) | (15) |
| <i>1958 Value Added</i> | | |
| Mean difference in top-four concentration ratios | .074 | .224 |
| Number of industries | (15) | (14) |
| <i>1947 Value of Shipments</i> | | |
| Mean difference in top-four concentration ratios | .139 | .221 |
| Number of industries | (7) | (15) |
| <i>1947 Value Added</i> | | |
| Mean difference in top-four concentration ratios | .108 | .150 |
| Number of industries | (14) | (17) |

^a Number of industries may be less than or equal to those previously reported, depending on top-four concentration ratio disclosures.

1947 value added) the mean difference in the top-four concentration ratio is larger (and substantially so) for those industries where the establishment distribution is not lognormal and the company distribution is, as opposed to those industries where both distributions are lognormal. Thus, the crude data which we are able to examine support the hypothesis of constrained establishment growth, and provide one explanation to account for the differences in the size distributions of establishments and companies.

IV. *Summary and Implications for Industrial Organization*

In recent years there has been substantial controversy regarding the usefulness of probability models to explain observed business size distributions. Some of the views advanced are extreme, and range from a call for re-evaluation of public policy decisions in the light of our knowledge of stochastic processes [14], to the belief that the models provide no promise toward understanding and describing concentration [13].

Our evidence suggests an intermediate position. We have found that the lognormal distribution does not provide a summary description of the size distributions of sellers over the broad array of industries in manufacturing. In none of our eight samples did the function describe more than 50 per cent of the size distributions studied. Yet its use does

not appear inappropriate for specific industries, especially when the company distributions are considered. However, we were unable to discover a consistent relationship between the occurrence of lognormality and specified attributes of the industries which were well described. This was true despite our finding that if an industry was lognormal, it was generally so regardless of the year considered or the measure of size employed.

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CONSUMER DEMAND IN THE UNITED STATES

*A Review Article*¹

By GEORGE L. PERRY*

Attempts to improve the specification and measurement of consumption functions continue to be a major object of empirical analysis in economics. In *Consumer Demand in the United States*, H. S. Houthakker and Lester D. Taylor add both to our stock of quantitative information about the demand for consumer goods and to our array of tools for analyzing consumer demand.

This book originated as a study for projecting the components of personal consumption expenditures for the government's Interagency Growth Study. Specifically, the research was designed to estimate a system of demand functions, based on time series data, for an 83 commodity breakdown of total consumption expenditures, and to use these demand functions for projecting the detailed components of personal consumption expenditures in a postulated 1970 economy. Reporting the results of this central task occupies roughly half of the 200-odd pages of the book.

In the other half, Houthakker and Taylor treat us to a range of interesting discussions arising from problems they had to solve along the way to estimating their demand equations and from off-shoots and extensions suggested by their main line of inquiry. We get some purely econometric material on problems of estimation and projection, including a discussion of the three-pass least squares method of estimation developed by Taylor and Wilson [10] which is used extensively in estimating the demand equations of the book. And we get a detailed development of a dynamic model of consumption demand applied both to durable goods and other components of consumption and to total consumption and saving. The H-T model is flexible and can accommodate a range of specific hypotheses about consumer behavior. For many readers these will be the most rewarding parts of the book; and it is a rewarding book.

It may be best to sketch what is in each of the seven chapters as the authors do in their introduction. Chapter 1 sets out the basic dynamic model of demand together with various special cases of the basic model. It is the one indispensable chapter even if one wishes to pick and choose from the rest in pursuit of some specialized interest. Chapter 2 discusses the variables and data that are used. Chapter 3 is the main chapter on econometric problems. It has sections on the projecting variance of estimated models, auto-

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¹ H. S. Houthakker and L. D. Taylor, *Consumer Demand in the United States, 1929-1970*. Cambridge: Harvard University Press, 1966. Pp. x, 214. \$6.00.

correlation problems, the three-pass estimating method, the problem of adding-up individual demand equations to get total consumption demand, and the results of Monte Carlo experiments for testing alternative forecasting methods. Chapters 4 and 5 present the main results of the study, the estimated demand equations and the projections to 1970, together with some tentative conclusions drawn from the results and some comparisons with results of others for six European countries. Chapter 6 switches to estimating aggregate consumption and saving functions and exploring further the dynamic model and some of its variations. This and Chapter 1 comprise material of considerable interest to students of the consumption function and to anyone interested in how to do useful empirical investigations generally. Indeed the last comment characterizes the book as a whole. Finally, Chapter 7 evaluates all the results and presents a principal-component analysis of the variables in the individual demand equations. It, in addition to Chapter 3, should be of particular interest to the applied econometrician.

I

The authors' basic dynamic model is central to the book and can be described briefly. In continuous time, expenditures at a given moment are made to depend on current income and a "state variable" representing the influence of past expenditures on the present. In the case of durables, this state variable may be concretely thought of as the stock of the good remaining from past expenditures; in the case of some other commodities and services, and possibly durables as well, the state variable is interpreted to measure "habit formation" or the "psychological stock of habits" for the good in question. Formally, either interpretation is expressed by the same form of equation: $q(t) = \alpha + \beta s(t) + \gamma x(t)$ [(2) on p. 9], where $x(t)$ is the rate of income at time t , $q(t)$ is the rate of expenditures, and $s(t)$ is the relevant stock. (Other explanatory variables, such as relative prices, are introduced in a manner parallel to income.) Where the durable inventory effect predominates, β will be negative; where the habit formation effect predominates, β will be positive. Now by assuming the change in the stock is given by $\dot{s}(t) = q(t) - \delta s(t)$ [(5) on p. 10], where δ is the depreciation rate, the stock variable can be eliminated to produce an equation in terms of observable variables. To do this, solve the first equation, (2), for $s(t)$ to eliminate it from the second equation, (5). Then differentiate the first equation with respect to time and substitute for \dot{s} the new form of the second equation, which now expresses \dot{s} in terms of q and x . The result is $\dot{q}(t) = \delta + (\beta - \delta)q(t) + \gamma \dot{x}(t) + \gamma \delta x(t)$, equation (8) on page 10. This is the basic equation of the model, expressed in continuous time and treating all equations as holding exactly. Following a careful translation of the model into discrete time, which itself is worth reading by anyone fitting data to a model, the authors derive the corresponding estimating equation (apart from estimating errors) of the form $q_t = A_0 + A_1 q_{t-1} + A_2 \Delta x_t + A_3 x_{t-1}$ [(37) on p. 14]. Thus the effects of stocks are incorporated without introducing them as a separate explanatory variable.

For most values of the coefficients, the values of the original parameters can be derived from the A 's (pp. 14 and 15) and estimates are thus obtained of the stock effect (β) and the short-run (γ) and long-run ($\gamma\delta/\delta-\beta$) marginal propensities to consume. (When additional explanatory variables are introduced, some parameter values become overidentified and the authors estimate using an identifying restriction technique which they discuss.) Since $\delta > 0$, for commodities whose consumption is dominated by habit formation ($\beta > 0$), the short-run MPC will be lower than the long-run, the effect of habit formation being to dampen short-run responses to income changes. For commodities dominated by physical stock adjustment ($\beta < 0$), the opposite is true. For either type of commodity, under steady linear growth (a constant absolute increase in x each period) it is easy to show that the marginal propensity to consume in each period is a constant equal to the long-run MPC. However there is no such simple relation under exponential growth at a constant rate.

II

Despite the many interesting and valuable side-aspects of the book the authors emphasize that the core of their work is in Chapter 4, where individual demand equations for 83 commodity groups are presented and their implications discussed; and they say it is mostly by this material their work should be judged. These results are based on annual data going back to 1929. Although history may form a judgment on the accuracy of the individual projections they present, I am not sure how to judge this body of material for this review. It is certainly the most carefully done estimation and projection of such a detailed breakdown of total consumption spending that I know of. By comparison, for instance, the vast Brookings-SSRC model [8, Ch. 7] estimates only five major categories of consumption rather than the 83 categories of this book. Yet their working over such a wide front did not keep Houthakker and Taylor from treating each category independently. They have tried a range of alternative specifications for each demand relation, including both linear and nonlinear static models in addition to the basic dynamic model and some variations on it. And they have tried variables for income distribution, age distribution, and some commodity stocks where these seemed appropriate. In short, they have explored a good deal before choosing any best equation for a particular commodity, at bottom relying on common sense and judgment about what is reasonable to guide them. I think this approach is the right one, contrasted, say, with a commitment to one functional form for all commodities or one mechanistic criterion for choosing among alternatives; and what they have done with such a mass of material is impressive. If I have one over-all misgiving it is with their use of total consumption expenditures (rather than income) as the main explanatory variable in all equations, but more on this below.

Having said I admire their general procedure and being unable to judge the predictive ability of their results, let me at least note a few of their findings. The dynamic equation turns out to be very useful. It is the preferred form for 72 of the 83 categories for which equations are estimated. Habit

formation ($\beta > 0$) turns out to characterize 50 of the commodities, accounting for 58 per cent of total 1961 consumption expenditures. Inventory adjustment ($\beta < 0$) characterizes 18 commodities, accounting for 28 per cent of total 1961 consumption expenditures. Total expenditure was excluded or had a negative sign in only five minor categories out of the total of 83: perhaps inferior goods (other fuel and ice, railway commuting) or changing tastes; and perhaps bad data or specification (toilet preparations). But prices came out much worse, appearing in only 45 of the final equations and frequently only at the borderline of significance.

This result of persistent price inelasticity will disturb many readers. Supply functions could not be specified; in the dynamic model, prices were constrained to act with the same lag structure as income; and relative prices did not change much for most commodities. Perhaps there is just a statistical problem in isolating the effects of prices on demand. It is interesting that 29 of the 45 commodities for which price variables were significant are services, the group with the largest relative price movement. But the authors accept their findings on prices and offer the interesting conjecture that prices become less important at high levels of income, in part because habit formation becomes more important. This is consistent with their finding that habit formation predominates in U.S. consumption (and is much less important in a study by Taylor [9] for Sweden, which they cite, where incomes are lower). In any case, whether the authors have captured the effect of relative prices successfully is not necessarily crucial for their estimates of the effect of income on demand or for the projections of future demands that they are after. Income coefficients would be more biased the greater the correlation of income to relative prices; if the correlation is low, the bias may be negligible (although other characteristics of the estimates will be affected). And if there were bias introduced by the failure to capture price effects, the importance for projecting into the future would depend on whether the past relation of income to relative prices were expected to continue; if it were, consumption projections would not be biased even if income coefficients were.

The authors get a few results that I found curiously satisfying. The stock of television sets has a strong negative effect on the demand for newspapers and magazines and on the demand for motion pictures; the stock of automobiles has a negative effect on the demand for shoes; the stock of washing machines has a negative effect on the demand for cleaning and laundering services. And, justifying one of the relations many of us try to establish for short-term forecasting, the authors find residential construction activity helps explain purchases of some categories of furnishings and household durables. But I should add that the results just cited generally came out of static forms of the demand equations which are frequently rejected in favor of the authors' preferred dynamic model.

The authors justify their use of consumption as the independent variable rather than income with three remarks: the study was designed to allocate a projected 1970 consumption total among its components; there is precedent from other studies, done where income data were unavailable; and

total expenditure is a better measure of "true" income because, in the short run, consumers have more control over expenditure than income. Certainly the first reason could have been circumvented and the second is irrelevant. The third is puzzling; perhaps I do not understand it, but if it means that some kinds of consumption are not sensitive to short-run changes in income, this is something most analysts would be anxious to measure and quantify. There are no doubt lags; indeed, the authors' own estimates on aggregate consumption as explained by income point them up. As it is, we cannot safely translate the dynamic characteristics observed with total consumption as independent variable, such as the preponderance of habit formation, to what they would be with income in its place. And it is certainly roundabout, and no doubt full of room for error, to use the aggregate consumption equation and then the individual commodity equations as a second step. Since aggregate consumption is generally more stable than income, for most commodities the dampening effect observed as habit formation might be more prominent with income as the independent variable. It is harder to speculate on what effect the change would have on the equations for individual durable goods. The equations given are appropriate to the narrow object of the study: allocating a given consumption total. But my guess is that most readers would have found equations based on income more valuable.

III

When they move to their analysis of total saving and consumption, Houthakker and Taylor change to disposable income as the main explanatory variable, and make use of quarterly as well as annual data. Their results are of considerable interest to anyone working with aggregate consumption functions. The authors note the similarity of their dynamic equation, featuring habit formation, to the early consumption theories of Duesenberry [3] and Modigliani [6]. These explained differences between the long-run and short-run marginal propensities to consume in terms of behavioral motives of just this sort. The H-T formulation, however, is more flexible and offers more scope for testing and refining. They let the data set the impact of habit on present consumption rather than having to choose approximations such as previous peak income. What is more, special cases of the general model can fit other explanations of behavior: a hypothesis based on the nature of saving and the motive to save turns out to work best, as discussed below. Another point to note is that, by starting from a continuous time model, they can account for apparent differences in the marginal propensity to consume when it is estimated from quarterly rather than annual data; and they make use of their model's ability to reconcile the two in evaluating several variations of their equation.

The most useful variation turns out to be an interpretation of saving as the accumulation of a nondepreciating asset, a view the authors associate with the early work of Tobin [11] who stressed the importance of assets in permitting dissaving, and of Zellner [12] who did some of the early empirical verification of the importance of assets in explaining consumption. The

Ando-Modigliani model [1] elaborating the implications for growth and fluctuations of the Modigliani-Brumberg life-cycle hypothesis [7] is also closely related, as is the parallel work done by Arena [2].

When savings are treated as assets with zero depreciation and saving is made the dependent variable in the H-T model, the basic estimating equation reduces to $y_t = B_1 y_{t-1} + B_2 \Delta x_t$ [(31) on p. 183], where y is saving and x is disposable income (in per capita terms, as most frequently used throughout the book, or in absolute terms). There is a good discussion of this model and some of its implications, although the authors mistakenly assert that the absence of a separate x_{t-1} term in this saving equation implies its absence in the corresponding equation for consumption (p. 185). Making use of the identity $x_t = q_t + y_t$ where q is consumption, this saving equation actually implies the consumption function $q_t = B_1 q_{t-1} + (1 - B_2) \Delta x_t + (1 - B_1) x_{t-1}$. What is readily apparent from this way of writing it is that in the long run static equilibrium of the model, all income is consumed, as we would expect from the treatment of accumulated saving as an asset with infinite life.

Ando-Modigliani [1] estimated consumption functions of the form $\Delta q_t = \alpha_1 \Delta x_t + \alpha_2 \Delta w_t$, where x is now labor income, q is now consumption excluding the purchases of durables but including the use of their services, and w is the net value of all assets owned by consumers. Arena [2] estimated a similar function, though with capital gains on assets treated as a separate variable and using disposable income rather than labor income. Ignoring capital gains, if we define w_t as assets held at the start of period t , the relation $y_{t-1} = \Delta w_t$ yields $\Delta q_t = (1 - B_2) \Delta x_t + (1 - B_1) \Delta w_t$ from the H-T consumption equation given above. Except for the different definition of variables and the treatment of capital gains, this special case of the H-T model is thus an alternative way to express the Ando-Modigliani and Arena models.

Against the conceptual advantage of recognizing capital gains when assets appear as a separate variable, the H-T equation offers a way of avoiding any explicit estimate of assets altogether. Theory favors excluding durable goods purchases, though a correct treatment would have to account for the whole spectrum of average lines involved; but forecasts or projections do require that we explain purchases. The H-T approach for individual durable commodities is based on the negative impact of stocks on purchases; thus it indirectly recognizes that the flow of services is what counts, while avoiding the need to measure stocks directly. What, if anything, is lost when durables are aggregated with other consumer demands in the total consumption model is hard to say.

There are some serious problems with the short, but rather conspicuous, section of the book aimed at applying Milton Friedman's permanent income hypothesis (pp. 179-82). The authors interpret the hypothesis to yield a relation $\dot{y}_p(t) = \beta y(t)$ when income grows exponentially, where \dot{y}_p is the time derivative of permanent income and y is measured income. They get this by differentiating the expression

$$y_p(T) = \beta \int_{-\infty}^T e^{(\beta-\alpha)(t-T)} y(t) dt$$

in Friedman's *Theory of the Consumption Function* [4, p. 144]. Actually, differentiating this expression yields $\dot{y}_p(t) = \beta y(t) + (\alpha - \beta)y_p(t)$; to get their expression requires the further assumption $\alpha = \beta$, an assumption I could find no way to justify. In fact, if we accept $\alpha = \beta$, the expression above for $y_p(T)$ shows y_p to be infinite for all finite T (unless $\beta = 0$, a case with equally implausible implications).

Even aside from this unhappy specification of Friedman's hypothesis, the authors' subsequent procedure is inappropriate. They are led to an equation in the second derivative of consumption and income [(18) on p. 181] which they estimate in its discrete form approximation [(19) on p. 181] as a test of Friedman's hypothesis. But their own model [(8) on p. 10] would yield exactly the same second-derivative equation. Thus the authors' version of Friedman is not distinguishable from their own dynamic model on the basis of (18). Both models imply this equation and neither is identifiable from it. The different characteristics they note in the estimates based on (18) and on first-derivative forms of their own model are not a comparison of the permanent-income hypothesis against the authors' dynamic model, but rather a reflection of the statistical rather than exact nature of the equations. I am in complete sympathy with the authors' protestation that the permanent-income hypothesis is hard to translate into operationally testable form. But it would have been better to let it go at that.

IV

The authors' preferred saving equation for projections is based on aggregate saving and income and quarterly data from 1953 to 1962. The estimates are $y_t = .960y_{t-1} + .380\Delta x_t$ [(51) on p. 189], where both coefficients are significant at the 1 per cent level. This equation yields an estimate of the short-run MPC of 0.61, compared with the long-run value of 1.00. The long-run value is reached when income stays indefinitely at the same level; continual growth, as assumed in the 1970 projections, yields a value between these two, the value being lower the faster the growth of income.

If there is something that leaves this reader uneasy it is the major conclusion of the aggregate projections that under reasonable growth rates to 1970, the saving rate rises sharply. Using (51), a 1970 range of 7.7 per cent to 11.5 per cent saving rates is given under various assumptions; with the old GNP data, the 1960-1962 average was 7.1 per cent. Consumer satiation seems to be just around the corner. The situation becomes even more alarming if we put this projected rise of, say, 2 percentage points in the saving rate together with the decline in the business investment share of GNP projected by Hickman [5] in his comparably detailed study. Unless easier monetary conditions expanded demand in other sectors, such as housing, substantially above its historical relation to GNP, then striking a full-employment saving-investment balance shows the federal government would need to run persistent full-employment deficits of over \$20 billion by 1970 with such projections.²

² This statement should be qualified in many ways. It rules out conceivable, though vast

Because the National Income Accounts data were changed rather drastically since the authors' regressions were run, I estimated the preferred aggregate equation with the revised data for the quarters from 1953 to 1962. The new estimates are $y_t = .909y_{t-1} + .478\Delta x_t$, with coefficients again significant at the 1 per cent level. Now the short-run MPC is 0.47. But projected saving rates seem more reasonable. With 4 per cent exponential growth, the saving rate is 5.0 per cent; with 5 per cent exponential growth, the saving rate is 6.1 per cent. Using these for a 1970 projection would compare very well with the average saving rate of 5.4 per cent for 1960-1962 in the revised data. However, treating the 6.8 per cent actual average annual growth rate of income between 1961 and 1966 as a constant rate, the equation would have predicted an 8.0 per cent saving rate for 1966, compared with the 5.3 per cent rate that occurred. The problem of projecting far ahead with this kind of aggregate equation does not seem easily solved.

There seems to me a strong presumption, based on our postwar performance, for an unchanged saving rate over any longer-run projection period without abrupt income changes. To be convincing, arguments to the contrary should be quite explicit about what is happening to change this. A well-documented argument based on demographic factors is the kind of thing I have in mind; but I know of none. The present model is explicit in the role it assigns to assets; but their quantitative impact on consumption is not well established. In particular, the different coefficient estimates reported when different time periods are used in estimation [(48) to (51) on pp. 188, 189] suggest the equation may be capturing cyclical effects on saving but at the expense of longer-run effects that are needed for projection. Some modification in the form of the equation may be necessary in order to capture both the short- and long-run influences on consumption. Where projected saving rates are sensitive to variations in the form of the equations and the time periods chosen for estimation as they are here, it might have been preferable to constrain the model to yield a constant saving rate in the long run, making adjustments for postwar shortages, if needed, in estimating. Under steady growth, the present model yields a saving rate proportional to the growth rate of income (p. 184), and the relation is sensitive enough to yield the range of projections discussed above.

If I seem to be raising rather a lot of questions, let me hasten to say my overall judgment of H-T's work is decidedly enthusiastic. In an already long review, I have only touched on much of the interesting and useful material they present. The book is a good example of how to do and present empirical work. The dynamic model is a genuine contribution to the study of consumer demand. This will be a valuable book to a wide range of readers.

and unrealistic, changes in the mix of taxes, the level of expenditures or other serious alterations in the current composition of either side of the budget which, through effects such as the balanced-budget multiplier and different spending propensities by different classes of taxpayers might invalidate this rough way of estimating potential total demand from the potential budget surplus. It also accepts the inelasticities with respect to monetary conditions of the projections cited.

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COMMUNICATIONS

Cross-Section Studies of the Consumption of Automobiles in the United States

This is a study of the consumption of automobiles in the United States during the period 1955-57. Depreciation is used as the primary measure of consumption in order to determine how well demand measured in this way can be explained by income and other social and demographic variables. Depreciation is defined as the change in wholesale price for each car owned by the household during each year of study.

This study differs from earlier work in three respects: (1) the limited dependent variable model developed by Tobin is used in order to handle correctly the problem of zero expenditure households. These families represent a significant fraction (25 per cent) of all households in the United States in recent years. (2) The data used are drawn from the Surveys of Consumer Finances for the years 1955, 1956, and 1957. The estimates for each of the years are compared and appear to be reasonably consistent. (3) A repair cost based on the age of each car is included in the total cost incurred by the household.

The conclusions are that (1) household consumption rises with income, but rises less sharply at higher than at lower incomes, (2) expenditure decreases as the head of the household approaches retirement age, (3) expenditure increases as the number of adults in the spending unit increases, (4) expenditure decreases as the number of children increases, (5) after adjustment for other factors, spending units living in New York and Chicago show lower than average, and those living in rural areas show higher than average consumption, (6) nonwhite spending units consume cars at a lower level than white.

The Measurement of Consumption

The model used to estimate the consumption of automobiles is as follows:

$$(1) \quad C = a_0 + a_1Y + a_2Y^2 + a_3A_1 + a_4A_2 + a_5A_3 + a_6A_4 + a_7A_5 \\ + a_8N_a + a_9N_c + a_{10}N_i + a_{11}L_1 + a_{12}L_2 + a_{13}R,$$

where C is the expenditure on automobiles, Y is income, A_1 - A_5 are age brackets for the head of the spending unit, N_a , N_c , N_i are the numbers of adults, children, and income earners respectively in the spending unit, L_1 and L_2 are location variables, and R is the race of the spending unit. The independent variables were chosen from the sample data¹ on the basis of their

¹ Except for the automobile price and repair cost data described below, the data originate from the Surveys of Consumer Finances collected by the Survey Research Center at the University of Michigan. Each of the independent variables in equation (1) originates from the survey data and is measured for the entire spending unit.

plausible association with the consumption of automobiles. Each of these variables requires a few explanatory remarks.

Consumption (C). Consumption expenditure is the imputed depreciation cost of each car owned by the spending unit plus an estimated repair cost. The survey data provided the year and make of each car owned plus the price paid for each car acquired during the year of study. It was necessary to construct average wholesale values for each year and make of car.

The expected value of a given year and make of automobile was computed in the following way. Let the index, i , indicate the body style, i.e. station wagon, j , indicate the model, i.e. "Fairlane," and k , indicate the year and make, i.e. 1955 Ford. The average wholesale price of the k th year and make is

$$(2) \quad P_k = \sum_i \sum_j x_i x_{jk} P_{ijk},$$

where x_i is the relative frequency of the i th body style, x_{jk} is the relative frequency of the j th model, and P_{ijk} is the individual wholesale price.²

The depreciation expense was based on the average rate of depreciation for each make. The relation between wholesale price and age was found to fit equation (3) closely using least squares techniques.

$$(3) \quad V_{kt} = A_k e^{-\alpha_k t},$$

where V_{kt} is the ratio of the wholesale price of the k th make of car of age t to its new price, A_k is the constant term, and α_k is the rate of depreciation for the k th car. Data on the price of one- to eight-year old models of each make were drawn over several years in order to average together the effects of variations in used car prices. The estimates, α_k (which varied from 25 per cent per year for Fords to 40 per cent per year for Packards), were all twenty standard deviations from zero. The depreciation expense for each car owned is computed by multiplying the average price, P_k , from equation (2) by the rate of depreciation, α_k , from equation (3).

In addition to the depreciation cost, a repair cost was estimated for each car held by the spending unit. A rough estimate of the cost of owning cars of different ages was obtained by assuming that repair cost is a linear function of age:

$$(4) \quad r_t = bt,$$

where r_t is the repair cost of a t year old car. Using the age distribution of cars and trucks in use and the total receipts of all automobile repair shops, b was found to be \$7.80. Thus a five-year old car would cost \$39.00 to repair. Any choice of repair costs based on such limited data is highly con-

² These data were obtained from the following sources:

1. Price data—National Automobile Dealers Association, *Official Used Car Guide*, Vols. 21–25, Jan. 1954–Jan. 1958.
2. Model frequencies—National Automobile Dealers Association.
3. Body style frequencies—American Automobile Association, *Automobile Facts and Figures*, 1955, 1960.

jectural. However, it seemed better to use some scale of costs than to ignore them altogether.

In addition to repairs there are many other costs associated with car ownership and operation. License and insurance fees, gasoline, oil, and tire costs can amount to several hundred dollars per car. In some states license fees are determined by weight. In others horsepower or age determine the cost. While some states require liability insurance, others do not. Insurance rates vary from county to county. All of these factors except gasoline, oil, and tire costs would affect higher-income families to a larger extent because these families tend to own newer cars. Since the expenditure data used in this study did not include any of these costs and since other data are not readily available if at all, there seemed to be no way to add such costs either as an independent component or as a function of the age, the make or the degree of use of the car, or some similar variable. Simply to assume that such costs are constant per automobile and to add them to other costs would not change the magnitudes of the estimates but would complicate the application of the limited dependent variable method of obtaining maximum likelihood estimates. For these reasons costs other than repair costs and depreciation are omitted from this study.

Income (Y). Income is disposable income in current dollars, i.e., personal income of all income earners in the spending unit less federal and state income taxes. About 0.5 per cent of the spending units in each sample did not disclose their incomes and these were eliminated from the study. In order to test the hypothesis that the relationship between income and expenditure was nonlinear a simple quadratic was used: that is, the independent variables in this partial relationship are income and income squared. If, as previous studies indicate, (1) the demand for cars decreases as the stock increases and (2) the demand for cars increases as income increases, then the proportion of income spent on automobiles may decrease as income increases.

Age of head of household (A). The survey data recorded age in six age brackets. Dummy variables were assigned as follows: $A_1=18-24$ years, $A_2=25-34$, $A_3=45-54$, $A_4=55-64$, and $A_5=65$ years and over. The hypothesis is that expenditures on automobiles by spending units in the age brackets denoted by A_1-A_5 differ significantly from those in the largest bracket, 34-45 years.

Number of adults (N_a). This variable represents the absolute number of persons over 18 years of age in the spending unit. The hypothesis is that families with more adults would tend to have greater demand for automobiles, other things being equal.

Number of children (N_c). This variable represents the number of persons in the spending unit under the age of 18. The hypothesis is that spending units with larger numbers of children would tend to have greater demand for automobiles.

Number of income earners (N_i). This variable represents the number of persons earning \$600 or more in the spending unit. Although the number of income earners is highly correlated with the number of adults, it is included

to determine, if possible, which of the variables, if any, is more important in explaining expenditure on automobiles.

Location (L). Suppose one were to classify regions into (1) the central metropolitan areas of the largest cities, (2) other cities and towns with population above 2,500 and suburban areas of the largest cities, and (3) rural areas, including towns with populations of 2,500 and below. The hypotheses in mind are that families living in central metropolitan areas would have less need for a car, and families in rural areas would have more need for a car, than others. Preliminary investigations have supported the latter hypothesis but not the former for metropolitan areas as defined by the Survey Research Center.³ Because of these results, I decided to restrict the number of cities to be classified as central metropolitan areas in order to see if any urban trend could be found. Only residents of New York and Chicago (about 7 per cent of the total sample) were counted as living in central metropolitan areas. The technique of dummy variables was again used.

Race (R). In order to test the hypothesis that nonwhite families have different spending habits, race was treated as a dummy variable. This category includes Negro, Puerto Rican, and other nonwhite spending units.

Two additional economic variables were recorded by the surveys which would seem to be important determinants of demand, but which were not included in the final equation estimated in this study. They are liquid assets and income changes. In early tests of this model no statistical relationship could be found between liquid assets and automobile expenditure as defined here. One reason for this result is that liquid assets and income are highly correlated. The effect of recent income changes was also examined in preliminary investigations. The survey recorded income changes in five categories: increases of more than 25 per cent and less than 25 per cent, no change in income, and decreases of less than 25 per cent and more than 25 per cent. Using dummy variables, I have been able to find no significant differences in automobile spending habits between any of the categories of income change and the category of constant income.

The statistical model is important because of the existence of a substantial number of noncar owners in the population of consumers investigated. In 1941 only 50 per cent of all spending units owned automobiles. In 1950 and 1960 the proportion of owners rose to 60 per cent and 74 per cent respectively. If the noncar owners are eliminated from a study, nothing can be said about the decision whether or not to own an automobile. If these families are treated in the same manner as families that own cars, a definite bias appears. Furthermore, a change in the values of the independent variables, such as a general rise in incomes, could produce an apparent shift in the least squares estimates when no such change in actual spending habits had taken place. Tobin [11] developed a model which eliminates both of these sources of error. His limited dependent variable model takes into account the fact that many observations may be massed as a single value of the dependent variable—possibly zero as in the present study.

³ Those cities are New York, Chicago, Los Angeles, Washington, Baltimore, Boston, Philadelphia, Pittsburgh, Cleveland, St. Louis, and San Francisco.

The Results

The major quantitative results are presented in Table 1. The estimates are obtained for each of three years, 1955, 1956, and 1957, in order to judge their stability. For each of these years, Table 1 presents (a) the coefficients of regression relating automobile expenditures and each of the independent variables and (b) the corresponding standard errors. The constant terms for the three years respectively are -140, -134, and -206.

One test of the hypotheses formulated above can be made by comparing the coefficients to their standard errors. Except for the first four age variables, the number of children, and the number of income earners, all of the hypotheses are accepted at the .01 level of significance. The effect of number of children is significant but of different sign than predicted. A stronger test is the likelihood ratio test which is performed by recomputing the likelihood function using subsets of the original variables. One such restricted model is tested below. The relationship of each of the independent variables to expenditure on automobiles is discussed in turn.

The coefficients of regression for income and income squared are both significantly larger than their standard errors; and they are relatively stable from year to year. Assigning the independent variables their respective mean values, the resulting elasticities of income were found to be 1.43, 1.45, and 1.74 for the three years of study.⁴

The age bracket for 65 years and over shows significantly lower levels of consumption than the 35-44 age bracket. Spending units in the 65 and over age bracket spend at least \$133 less on automobiles than the 35-44 age bracket. The 55-64 bracket also shows some tendency for lower spending although not markedly. The 18-24, 25-34, and 45-54 age brackets, however, show regression coefficients which are either different in sign among the three years or small relative to the standard error. The three studies taken together reject the hypothesis that the age of the head of the spending unit is related to expenditures on automobiles—at least for the younger age brackets.

Each additional adult increases total expenditure by from \$59 to \$79. At least part of the explanation here is the fact that 42 per cent of the spending units with only one adult own automobiles, whereas 85 per cent of the two-adult spending units own automobiles.

The addition of children reduces expenditure per child \$14 to \$17. Of the spending units with automobiles, 43 per cent had no children. The fact that this relationship is negative must be influenced by this large proportion of spending units with no children. These families would have more money to spend on automobiles.

The number of income earners has no significant relationship with expenditure on automobiles. The coefficients of regression are about the same as or smaller than the standard error. Almost all of the spending units—

⁴ The income and expenditure data used in this study are all in current dollars. The 1956 and 1957 data were not adjusted for price changes. The consumer price index rose about 2 per cent per annum during the three years of study.

TABLE 1.—COEFFICIENTS OF REGRESSION AND CORRESPONDING STANDARD ERRORS FOR ESTIMATED RELATIONSHIPS BETWEEN AUTOMOBILE EXPENDITURES AND VARIOUS INDEPENDENT VARIABLES^a

| Independent Variable | 1955 Data | | 1956 Data | | 1957 Data | |
|--------------------------|---------------------------|----------------|---------------------------|----------------|---------------------------|----------------|
| | Coefficient of Regression | Standard Error | Coefficient of Regression | Standard Error | Coefficient of Regression | Standard Error |
| Income | .0595 | .0030 | .0643 | .0027 | .0671 | .0029 |
| Income squared | — .00000049 | .000000083 | — .00000067 | .000000081 | — .00000071 | .000000066 |
| Age: | | | | | | |
| 18-24 | -17.8 | 24.4 | -26.4 | 22.8 | 18.3 | 26.3 |
| 25-34 | 19.1 | 40.6 | 3.2 | 15.1 | 1.9 | 17.9 |
| 45-54 | 66.7 | 18.1 | 29.0 | 15.8 | -1.1 | 18.9 |
| 55-64 | -80.8 | 19.6 | -31.5 | 18.5 | -39.5 | 22.4 |
| 65 or over | -143.3 | 21.1 | -147.9 | 20.6 | -132.8 | 24.6 |
| Number of adults | 64.2 | 10.7 | 58.8 | 10.1 | 78.5 | 24.6 |
| Number of children | -14.2 | 4.5 | -14.5 | 4.3 | -16.8 | 4.8 |
| Number of income earners | 9.64 | 14.5 | 4.2 | 10.0 | 16.8 | 12.3 |
| Location | | | | | | |
| Metropolitan area | -175.6 | 24.5 | -200.3 | 22.1 | -263.8 | 25.1 |
| Rural area | 75.0 | 15.1 | 54.3 | 12.0 | 62.0 | 14.7 |
| Race: | | | | | | |
| Nonwhite | -128.1 | 22.6 | -88.8 | 20.9 | -129.2 | 28.1 |

^a The relationship estimated for each year is equation (1): $C = a_0 + a_1Y + a_2Y^2 + a_3A_1 + a_4A_2 + a_5A_3 + a_6A_4 + a_7A_5 + a_8N_c + a_9N_c + a_{10}N_c + a_{11}L_1 + a_{12}L_2 + a_{13}R$, where C = expenditure on automobiles and the independent variables are as indicated in the left hand column of the table.

96 per cent in 1956—had either one or two income earners; and the expenditure for each of these two groups is roughly the same.

In a limited sense, the location variables support the hypothesis that population density is inversely related to the consumption of automobiles. Residents of central metropolitan areas—New York City and Chicago—spend at least \$175 less on automobiles, while residents in rural areas spend at least \$54 more than the rest of the spending units. The availability of public transportation and the lack of parking space in the city, along with the greater need for transportation and the lack of substitute services in rural areas, all serve to explain this behavior. The results are limited, however, with respect to central metropolitan areas because only New York and Chicago are included as such. Generalizations should not be made until more large cities are studied. One might expect that residents of, for example, Los Angeles, would not behave as indicated because of the better roads and the lack of public transportation in that area.

The coefficients of regression for the race variable indicate that nonwhites spend considerably and significantly less on automobiles than whites. Nonwhites spend at least \$89 less than whites. Nonwhites include Puerto Rican and other ethnic groups as well as Negroes. Negroes composed 86 per cent of the nonwhite group. These results do not agree with the common conception of spending habits on automobiles by nonwhites. Further study is needed to provide an adequate explanation of this behavior. The expenditures of whites and nonwhites should be studied on a regional basis taking into account differences, if any, in rents, credit terms, and income stability as well as the demographic factors considered in this study.

Of the independent variables studied, two yield no consistent or significant relationship: age, except for the upper age brackets, and the number of income earners. One final question, therefore, arises. Suppose the age brackets below 65 and the number of income earners are eliminated from the study. What is the effect on the other parameters and, especially, on the coefficients of regression with respect to income and their stability over time?

Table 2 presents the maximum likelihood estimates of the model with the four age variables A_1 , A_2 , A_3 , A_4 , and the number of income earners, N_i , omitted. While most of the coefficients are similar to those in Table 1, the coefficients for the number of adults and the number of children are not as stable as in the previous model.

In order to examine the stability of the elasticities of income in Table 2 the elasticities were computed for this restricted model. The elasticities are 1.60, 1.53, and 1.67 for the years 1955, 1956, and 1957 respectively. These elasticities are all within the range of elasticities found for the original model, 1.43–1.74.

Likelihood ratio tests were performed for each of the three years of study. The hypothesis tested was that the subset of coefficients for the variables A_1 , A_2 , A_3 , A_4 , and N_i , are all equal to zero. The statistic which is tested is $-2 \log \lambda$ which is chi squared distributed with five degrees of freedom. λ is

TABLE 2—COEFFICIENTS OF REGRESSION AND CORRESPONDING STANDARD ERRORS FOR ESTIMATED RELATIONSHIPS BETWEEN AUTOMOBILE EXPENDITURES AND VARIOUS INDEPENDENT VARIABLES^a

| Independent Variable | 1955 Data | | 1956 Data | | 1957 Data | |
|----------------------|---------------------------|----------------|---------------------------|----------------|---------------------------|----------------|
| | Coefficient of Regression | Standard Error | Coefficient of Regression | Standard Error | Coefficient of Regression | Standard Error |
| Income | .070 | .0028 | .060 | .0026 | .067 | .0027 |
| Income squared | — .00000065 | .00000008 | — .00000021 | .00000008 | — .00000072 | .00000063 |
| Age: | | | | | | |
| 65 and over | —121. | 18.2 | —132. | 17.4 | —123. | 19.4 |
| Number of adults | 13.6 | 4.9 | 56.7 | 9.5 | 67.0 | 11.8 |
| Number of children | —13.9 | 4.2 | —9.75 | 3.68 | .038 | .016 |
| Location: | | | | | | |
| Metropolitan area | —105. | 18.6 | —195. | 22.3 | —264. | 25.3 |
| Rural area | 56.5 | 13.8 | 54.3 | 11.9 | 54.9 | 14.6 |
| Race: | | | | | | |
| Nonwhite | —113. | 17.2 | —90.9 | 21.0 | —132. | 23.8 |

^a The relationship estimated for each year is as follows: $C = a_0 + a_1Y + a_2Y^2 + a_3A_6 + a_4A_5 + a_5N_c + a_6N_c + a_7L_1 + a_8L_2 + a_9R$, where C = expenditure on automobiles and the independent variables are as indicated in the left hand column of the table.

the ratio of the value of likelihood function at its maximum for the restricted model to the corresponding maximum value for the unrestricted model. This hypothesis was rejected at the .01 level of significance for all three years of study. On the basis of this test alone it must be concluded that some or all of the variables omitted did contribute to some extent to the explanation of the variation in automobile expenditures. Table 1 shows that the coefficients of the age variables 45-54 and 55-64 for the 1955 data were significantly different from zero. These two variables for the 1956 data were barely significant and for the 1957 data the age variable 55-64 and the number of income earners have coefficients slightly larger than their standard errors. The omission of these variables from the restricted model could have produced the negative results obtained from the likelihood ratio tests and some of the instability in the coefficients of the family size variables. Likelihood ratio tests on other subsets of the original variables were not performed.

The principal conclusions of this study of the demand for automobiles in the United States are as follows:

1. There appears to be a strong and relatively stable relationship between the consumption of automobiles as defined in this study and the disposable income of the spending unit. This relationship yields an elasticity of expenditure with respect to income which is somewhat higher than the two other studies which were based on cross-section data.
2. The age of the head of the spending unit is not related to expenditure for most age brackets. When the head of the spending unit reaches the age of 55 a negative relationship between age and consumption does appear and especially so in the age bracket of 65 and over.
3. The location of the spending unit does affect consumption. Spending units living in rural areas spend more on automobiles, and residents of the central metropolitan areas of New York and Chicago spend less than others.
4. When the spending unit has more than one adult, automobile expenditures increase; and when children are present in the spending unit, expenditures seem to decrease. The number of income earners when used with the two variables just mentioned does not contribute to the explanation of the demand for automobiles.
5. Nonwhite families spend much less on automobiles than white families with similar incomes, locations, and family composition.
6. In a restricted form of the model in which the age and number of income-earners variables which were not significant were omitted, most of the remaining relationships appeared to be consistent with the original model. Some variation appeared in the variables representing the number of adults and the number of children. The income elasticities computed from the restricted model supported the conclusion that the income elasticity over the three years of study was fairly stable.

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Some Evidence on the Interest Elasticity of Consumption

Although there has existed a theoretical tradition concerned with the relationship between consumption (saving) and the rate of interest, which is acceptable in its elementary form to many economists, the related empirical issues remain not only unsolved, but essentially unexamined [5]. This paper reports upon one attempt to estimate the compensated interest elasticity of consumption by introducing an interest variable into a consumption function which is quite similar to Friedman's "Permanent Income Hypothesis" [4] and the "Life Cycle Hypothesis" developed in a series of articles by Modigliani, Brumberg, and Ando [1] [6]. In the two sections of this paper we shall derive the consumption function appropriate for estimating the substitution effect of a change in the rate of interest and report on some regressions obtained from testing the resulting model.

I

If an individual has a planning horizon n periods in length for which he plans the level of his consumption and the value of his desired net worth at the end of his planning horizon, his utility function may be expressed as

$$(1) \quad U(C_0, \dots, C_n, W_n)$$

where C_i denotes planned real consumption in period i and W_n the value of his desired real net worth. Denoting expected real labor income in the i th period as Y_i^e and net worth at the beginning of the planning period as W_0 , which for economy of notation is defined to include interest income accrued over period minus one but payable on the first day of period zero, the budget constraint for such an individual can be written as

$$(2) \quad W_0 + \sum_{i=0}^n \frac{Y_i^e}{(1+r)^i} - \sum_{i=0}^n \frac{C_i}{(1+r)^i} - \frac{W_n}{(1+r)^n}$$

where r denotes the rate of return available on the individual's saving.

Defining a rational consumer as one who acts as though he maximized (1) subject to (2), we execute this constrained maximization problem to obtain the necessary conditions for a maximum. These conditions may then be differentiated totally and solved for the differential of present consumption which is

$$(3) \quad dC_0 = V_{0,n+1} \left[dW_0 + \sum_{i=0}^n \frac{dY_i^e}{(1+r)^i} + \left(\sum_{i=1}^n \frac{i(C_i - Y_i^e)}{(1+r)^{i+1}} + \frac{W_n}{(1+r)^{n+1}} \right) dr \right] - \sum_{i=0}^n \frac{V_{0i}}{(1+r)^{i+1}} (dr)$$

where V_{ij} denotes the ratio of the ij th cofactor and the determinant of the bordered Hessian arising from the second order conditions of the maximization process.

It is now convenient to assume that (3) can be approximated by a differential equation having locally constant coefficients the solution of which, upon substituting standard notation, is

$$(4) \quad C = \alpha_0 + \alpha_1 PW_0 + \alpha_2 r$$

where α_0 is the constant of integration and

$$(5) \quad PW_0 = W_0 + \sum_{i=0}^n \frac{Y_i^e}{(1+r)^i} + \left(\sum_{i=1}^n \frac{W_{0+i}}{(1+r)^i} \right) r.$$

PW_0 contains current net worth (W_0), the present value of expected labor income

$$\left(\sum_{i=0}^n \frac{Y_i^e}{(1+r)^i} \right),$$

the present value of property or interest income

$$\left(\sum_{i=1}^n \frac{W_{0+i}}{(1+r)^i} r \right),$$

and is defined as the present worth of the consumer in period zero.

An understanding of the last term in PW_0 is important for an appreciation of the analysis to follow and therefore requires special comment. W_{0+i} denotes the net worth held during the i th period, the value of which is determined by the initial level of net worth and the optimal consumption plan of the individual. The product of this term and the rate of return on saving is the interest income which will be forthcoming in the $i+1$ th period. Consequently, the last term in PW_0 denotes the present value of all future interest income and represents the income effect of a change in the rate of interest. For a proof of this see [8].

Having expressed present consumption as a function of an interest variable and present worth, where the coefficient of the interest variable is interpreted as the substitution effect only, our task is now to relate the unobservable present worth to observable variables. One way of doing this is to use estimates of net worth and total income which have been used in previously fitted consumption functions. Since our income variable must be a surrogate for current and expected future total income, we may invoke a variant of Friedman's hypothesis and use an exponentially weighted average of current and past total income which we shall refer to as "normal" income and define as

$$(6) \quad Y_t^* = B \sum_{i=0}^{\infty} (1-B+\alpha)^i Y_{t-i}$$

where Y_t is total income in period t , B the coefficient of adjustment in an adaptive expectations model, and α a growth factor to account for the trend in measured or observed income. Alternatively, we may express (6) as

$$(7) \quad Y_t^* = B Y_t + (1-B+\alpha) Y_{t-1}^*$$

which reveals more readily the necessity of making certain computational decisions the nature of which are discussed below.

Denoting the available estimates of current net worth as W , we substitute Y^* and W for PW_0 and express the consumption function as

$$(8) \quad C = b_0 + b_1 Y^* + b_2 W + b_3 r + u$$

where u denotes the error term.

It is now necessary to make two final assumptions. First, although we have derived a consumption function for the individual, we assume it to be applicable to aggregate per capita consumption functions also. This requires that several problems be ignored, but we do so with precedent. Secondly, we note that individuals plan their consumption levels based upon the level of the net of tax variables. This implies that our income variables be interpreted as disposable income and the interest variable as a net rate of return.

Data on disposable income are readily available but such is not the case for the interest variable. The existence of a progressive tax structure over the period examined in the regressions requires that we construct an average marginal tax rate for the economy by which to adjust the published gross interest rates. (For a detailed account of how the tax rate was computed see [8].) Adjustments of the theory to take these considerations into account are slight, amounting to little more than a change in notation. Therefore, it will be assumed that the above notation refers to net of tax concepts wherever applicable.

II

The data used in estimating equation (8), the results of which are displayed in Table 1, can be conveniently labeled according to the source from which the income and consumption data were obtained. Such labels will be the "Goldsmith data" referring to R. W. Goldsmith's *A Study of Saving in the United States* [3], and the "MBA data" referring to the data used by Modigliani and Ando in their "The 'Life Cycle' Hypothesis of Saving" article [1], [2]. Several interest rates were used but only the results from using the net yield on corporate Aaa bonds are displayed in Table 1 [9].

TABLE 1—RESULTS OF FITTING THE GOLDSMITH AND MBA DATA TO THE EQUATION

$$C = A_0 + A_1 Y^* + A_2 W + A_3 r + U$$

(Numbers in parentheses are *t*-values)

| Constant | Normal Income | B^a | Net Worth | Interest | R^2 | D.W. | N_{σ}^b |
|------------------|------------------|-------|-----------------|--------------------|--------|------|----------------|
| GOLDSMITH | | | | | | | |
| 393.02 (5.15) | .4210 (5.82) | .90 | .0323 (2.25) | -2372.0 (-2.15) | .96369 | 1.64 | -.026 |
| MBA | | | | | | | |
| 192.24 (8.43) | .4425 (14.62) | .65 | .067 (11.36) | -2210.4 (-4.51) | .99902 | 1.60 | -.022 |

^a B is the coefficient of adjustment in the adaptive expectations model.

^b (The elasticity estimates were computed using the 1949 values of the relevant variables.)

The Goldsmith data cover the period 1897–1949, excluding the war years, while the MBA data cover the period 1929–1959 and also exclude the war years. In constructing Y^* it is, of course, necessary, as can be seen from equation (7), to begin with an initial Y_{t-1}^* which is not generated by a formula such as (6) or (7). In the Goldsmith data, the first observation on Y_{t-1}^* was taken to be the observed Y_t in 1897. Starting with this as a base, subsequent series in Y_t^* were generated using (7). For the empirical work reported in Table I, the initial year appearing in the regression was 1905; thus the initial value of Y_t^* actually used was an estimate based on eight previous years of observed values of Y_t . In the case of the MBA data, the available series was judged to be too short to warrant the loss of a number

of observations, hence, the initial value of Y_t^* was taken to be the first value of Y_t .

In both samples the interest variable enters with significantly negative coefficients which are similar in magnitude and are consistent with the theory that the substitution effect is negative. The low values observed for the MPC out of normal income stem from the fact that current net worth is also present in the regressions. These marginal propensities can be converted into magnitudes that are comparable to those derived from other studies in which no wealth variable appeared, by assuming a given relationship of wealth to income. If the average values of W/Y^* over the period of observation are used for this purpose, the "comparable" marginal propensity to consume out of income is $(b_1 + b_2 W/Y^*)$. The figures thus computed for the two regressions are in the neighborhood of .75, an order of magnitude which is not surprising in comparison with other estimates of marginal propensities to consume.

Both equations show little indication of serial correlation, the Durbin-Watson statistic being conclusive at the 2.5 per cent level of significance. Moreover, even though substantial correlation existed among the regressors, each variable has a significant t -value, thus alleviating any concern over multicollinearity.

The results obtained from using net of tax corporate bond yields in a consumption function containing income and wealth offer convincing evidence that consumption is responsive to changes in the rate of interest. With the interest elasticity of saving related to the interest elasticity of consumption in the following manner:

$$N_{sr} = - N_{sr} \frac{C}{S},$$

a range of estimates for N_{sr} would be .19 to .24 when computed from the elasticity estimates in Table 1 and the average value for C/S . Such elasticity estimates are much larger than those assumed to exist by many economists who feel that saving is unresponsive to changes in the rate of interest [7, p. 40]. Evidence for such a conclusion is often sought in the constancy of the savings-income ratio during periods when the interest rate fluctuated considerably. It must be recognized, however, that income effects were present when interest rates were changing and that the constancy of the savings-income ratio may have been the consequence of an offsetting substitution effect. The evidence in Table 1 indicates that a substantial substitution effect did in fact exist.

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Bank Adjustments to Monetary Policy: Alternative Estimates of the Lag

Questions relating to the length of lags in the monetary process have an important bearing on discussions of monetary policy. Considerable attention has been given to lags between movements in key monetary variables such as the money supply or interest rates and movements in national income or selected components thereof. Empirical studies suggest that there are substantial lags between changes in such variables and changes in activity. These findings have led some to conclude that monetary policy cannot play an effective countercyclical role.

It has been shown that such a conclusion may be ill founded [6]. Nonetheless, even if it could be shown further that economic activity responds promptly to changes in key monetary variables, the effectiveness of monetary policy would still remain open to question. If the "inside lags" are too great, policy could be impaired. For example, if it could be demonstrated that the commercial banking system, the chief mechanism through which monetary impulses are transmitted, is slow to respond to disequilibrating factors, serious doubts would be justified concerning the counter-cyclical role of monetary policy. Much recent empirical research makes just such a showing.

A substantial body of evidence has accumulated suggesting that sluggish behavior pervades commercial bank adjustments. In studies of various types of portfolio adjustments, lags have been reported ranging from several months to several years [1] [2] [3] [4]. Lags such as these are descriptive of a banking system which would not be an effective conductor of counter-cyclical actions of the central bank.

Results reported in this paper show that, contrary to such studies, the time required for reserve adjustments by commercial banks is a matter of weeks rather than months or years. Such findings help restore the presumption that the banking system is capable of transmitting monetary policy actions in time for them to be pertinent to the economic environment within which they were conceived.¹ This paper also suggests that other researchers have found long lags primarily because they have tested their models with data aggregated over decision units and over time periods. Such findings have a bearing on the use of the Koyck distribution and may tend to cast some doubt on the whole range of lag estimates that have been made available to policymakers and reported by economists.

An excess reserves model is specified below. This model is then used as the prototype for the alternative lag estimates discussed in section II. Conclusions are presented in section III.

I. *An Excess Reserves Model*

In this section, a model of an individual bank's reserve adjustment process is specified. Such adjustments are the constituent elements of the inside lag between central bank actions and changes in bank credit and the money supply. Prompt bank responses to a changing monetary environment are necessary in order for monetary impulses to be transmitted quickly. Although the implications of this article relate to the lags between central bank actions and adjustments of individual banks, there is no explicit monetary policy variable—such as total reserves—from which lags are estimated. In specifying a model at the level of the decision-making unit, as in this study, it seems most meaningful to think in terms of variables that impinge in a direct way on individual commercial banks. The effects of monetary policy on individual banks appear in the guise of variables which the bank encounters in the ordinary course of its operations.

The underlying hypothesis of the model is that the individual bank takes action during a given interval to remove a discrepancy between its desired excess reserve position at the end of an interval and the position that would prevail in the absence of action. The familiar stock-adjustment model is well suited to this hypothesis. Also, in the earlier studies alluded to above, the stock-adjustment model was frequently employed. In order for results of tests of the present model to shed light on earlier findings, it seems desirable to make the models as comparable as possible. Accordingly, it is specified that

$$(1) \quad \Delta X = \lambda [\bar{X}_{t+1}^* - (X_t + \Delta X_t^A)]$$

where

ΔX = adjustments in excess reserves;

λ = adjustment coefficient;

¹ On the other hand, to the extent that the banking system makes quick adjustments in its quantities, it may imply reduced movements in interest rates, the market clearing mechanism. Thus, if monetary policy actions are specified in terms of interest rate movements, rapid adaptive behavior of the commercial banking system may slow the impact of policy changes. I am indebted to Frank de Leeuw for suggesting this point.

\bar{X}^* = desired excess reserves;
 X = actual excess reserves;
 ΔX^A = anticipated change in excess reserves; and
 t = time subscript.

The dependent variable, ΔX , consists of those changes in excess reserves which result from bank action. A bank can affect excess reserves by altering its holdings of earning assets, by changing its balance due from other banks, and by borrowing or repaying debt. These are summarized into a single variable.² The adjustment coefficient, λ , denotes the extent to which the bank removes a discrepancy between its desired reserve position at the end of a period and the reserve position which it anticipates in the absence of action. Estimates of this coefficient are of major interest in this paper; its size determines the lag—i.e., the number of periods required (approximately) to close discrepancies between desired and actual positions.

A bank's desired excess reserve position is not observable, but is specified to be a function of a vector of variables as follows³:

$$(2) \quad \bar{X}_{t+1}^* = g(r_{t+1}, L_{t+1}, B_{t+1}, S_{t+1}, T_{t+1}, RP_{t+1})$$

where

$$\begin{array}{ll} r = \text{opportunity costs;} & \frac{\partial \bar{X}^*}{\partial r} < 0. \\ L = \text{loan demand;} & \frac{\partial \bar{X}^*}{\partial L} < 0. \\ B = \text{cost of borrowing;} & \frac{\partial \bar{X}^*}{\partial B} < 0. \\ S = \text{bank size;} & \frac{\partial \bar{X}^*}{\partial S} < 0. \\ T = \text{trend;} & (\text{no presumption concerning effect}) \\ RP = \text{reserve settlement period}^{3a}; & \bar{X} \geq 0. \end{array}$$

² In the empirical work, the dependent variable and other dollar value variables are deflated by a size variable. It would be sufficient to assert that by hypothesis the demand for excess reserves is homogeneous of degree one in all dollar variables. In addition, however, there were practical computational considerations which had a bearing on the form the empirical work ultimately took.

³ Opportunity costs were proxied, alternatively, by 3-month Treasury bill rate, the federal funds rate, and the yield on long-term government bonds. Loan demand was proxied by changes in commercial and industrial loans, the Federal Reserve Board index of industrial production, and metropolitan area payroll employment. The cost of borrowing was proxied by the Federal Reserve discount rate and by the outstanding stock of borrowing at the beginning of the period.

^{3a} Some weekly reporting banks, the country banks, meet reserve requirements on a bi-weekly basis. Because penalties may be levied against banks that fail to meet reserve requirements, it is hypothesized that excess reserves will not be negative for a settlement period.

The remaining term in equation (1), ΔX^A , denotes anticipated changes in excess reserves. One may imagine that there are numerous factors which impinge exogenously on excess reserves—including manifestations of central bank actions—but that some of the factors are anticipated. For example, there may be anticipated changes in deposits based on known characteristics of depositors or on knowledge of lags between previous asset actions and current deposit experience. The approach taken in this paper was to imagine that these factors merge into seasonal loan and deposit anticipations. In an effort to measure such anticipations, what are here termed exogenous variables, ΔE ,⁴ are introduced in such a manner that a separate coefficient is estimated for each month. Based on seasonal experience regarding loan demands and deposit movements, a bank regards changes in exogenous variables that occur in January differently from those that occur in February, March, and so forth.

The equation to be estimated is specified as linear in its arguments, and parameters are estimated by ordinary least squares. The regression model is:

$$(3) \quad \Delta X_t = b_0 + b_1 r_{t+1} + b_2 L_{t+1} + b_3 B_{t+1} + b_4 S_{t+1} + b_5 RP_{t+1} + b_7 X_t \\ + b_{8-19} \Delta E_t.$$

As is well known, the coefficient relating the stock of excess reserves at the beginning of the period to the dependent variable, b_7 , is an estimate of λ , the adjustment coefficient. Alternative estimates of λ are discussed in the following section.

II. *Alternative Estimates of the Lag*

The model specified above was tested at both the micro and macro level. Macro-tests were performed both with weekly data and with data aggregated across weeks into months. The data were from 19 individual banks located in the Eighth Federal Reserve District. Observations spanned an eight-year period from the beginning of 1957 to the end of 1964. During this period, there were two recessions and two expansions; accordingly, monetary policy shifted several times.

From among numerous tests, a "basic" equation was selected.⁵ Results of tests with this equation are presented in Table I. Coefficients of determination range from .88 to .09, with a mean coefficient of .56. The column of regression coefficients listed beneath b_7 , the lagged stock of excess reserves,

⁴ The exogenous variable is here defined to consist of the sum of changes in cash items in process of collection, changes in demand and time deposits, and changes in the capital account. Where appropriate, items are adjusted for their impact on required reserves; the exogenous variables are of interest here only to the extent that they impinge on excess reserves.

⁵ Variables included in the basic equation are those which met the usual criteria: their coefficients were acceptable in terms of signs and according to standard tests of significance; they contributed sufficiently to the overall goodness of fit of the equation.

are of principal interest. These coefficients, estimates of the adjustment coefficient, are arrayed in Table II according to their implied speed of adjustment. Micro estimates are discussed below.

Micro Variables—Micro Relationships

Observations taken at the level of the individual bank were used to estimate micro relationships. First, individual bank data were used to estimate relationships for each of 19 separate commercial banks. Next, one set of relationships was estimated by "pooling" the data over the 19 banks.⁶ Thus each relationship, each regression coefficient, was averaged across 19 banks, with dummy variables used to capture differences among banks.

Estimates of λ derived from tests with data from the separate banks carried a negative sign as hypothesized and were significant in each test. The implied adjustments (i.e., to a 95 per cent closure) were quite rapid, ranging from .8 to 9.8 weeks. The average lag was 3.2 weeks (Table II). The estimate of λ from the "pooled" data was $-.690$, implying a 2.6-week lag (Table II). Such an adjustment is about one-fifth more rapid than the average adjustment of the 19 separate estimates.

Lags such as these at the individual bank level are sufficiently rapid to suggest that the commercial banking system is capable of prompt responses to countercyclical monetary policy actions. However, at this stage, such a conclusion is premature. Results reported here are particular to the banks studied. The results can be placed in juxtaposition to the long lag estimates reported in earlier studies, but unless more can be shown the matter rests here. On the other hand, if substantially longer lag estimates can be derived from tests of the model with data organized in a manner comparable to those used by other bank adjustment studies, a presumption can be created that the long lag estimates reported elsewhere may similarly reflect data organization.

Aggregated Variables—Aggregate Relationships

We might suppose that aggregating values of dependent and independent variables across decision units would tend to obscure observable empirical relationships. For, even if each bank made a prompt adjustment to a disequilibrating situation, as this study suggests, such adjustments would serve to place other banks in disequilibrium. For example, if an individual bank found that it had surplus excess reserves, it might remove such a surplus by buying earning assets. Such an adjustment would serve to shift deposits and, hence reserves, to yet another bank—and so forth. Such a process is simply the bank credit expansion or contraction mechanism described in basic money and banking texts.

In addition, the adjustment made by a decision unit is direct whereas that made by an aggregate is not. Given changes which are exogenous to a decision unit (an open system), the unit can regain equilibrium by bringing actual conditions into alignment with desired conditions. In terms of the

⁶ The data were treated as though they were from one bank for 152 years (8 years \times 19 banks).

TABLE I—LINEAR REGRESSIONS OF ΔX ON THE BASIC EQUATION

| Bank | b_0 Regression Constant | b_1 Outstanding Borrowing | b_7 Lagged/ Stock of Excess Reserves | b_{1-12} Extent to which the Changes in Excess Reserves Were Anticipated During | | | | | | | | | | | | R ² |
|------|---------------------------------|-----------------------------------|--|--|-----------------|------------------|------------------|-----------------|-----------------|-----------------|------------------|------------------|------------------|------------------|------------------|----------------|
| | | | | Jan. | Feb. | Mar. | Apr. | May | June | July | Aug. | Sept. | Oct. | Nov. | Dec. | |
| #13 | .000 | -.012 (.082) | -.968 (.070) | -.916 (.090) | -.958 (.099) | -1.027 (.067) | -.960 (.123) | -.983 (.130) | -.947 (.084) | -.135 (.006) | -1.174 (.084) | -1.018 (.063) | -1.143 (.096) | -1.065 (.071) | -1.053 (.055) | .88 |
| #5 | .000 | -.017 (.082) | -.504 (.046) | -.947 (.075) | -.927 (.080) | -.971 (.083) | -.987 (.102) | -.845 (.089) | -.775 (.072) | -.848 (.086) | 1.082 (.098) | -.989 (.077) | -1.110 (.104) | -.549 (.068) | -.740 (.067) | .80 |
| #11 | -.001 | -.062 (.117) | -.744 (.116) | -.447 (.248) | -.326 (.367) | .054 (.249) | -.756 (.272) | -.945 (.040) | -.965 (.083) | -.967 (.055) | -.788 (.277) | -.892 (.045) | -.678 (.248) | -.531 (.257) | -.644 (.218) | .80 |
| #18 | .000 | -.002 (.070) | -.391 (.041) | -.976 (.070) | -.976 (.117) | -.958 (.089) | -1.019 (.105) | -.742 (.097) | -.932 (.094) | -.993 (.112) | -1.115 (.099) | -.902 (.080) | -1.075 (.088) | -.916 (.089) | -1.081 (.105) | .78 |
| #20 | .000 | -.088 (.037) | -.264 (.361) | -.864 (.085) | -.884 (.095) | -.859 (.091) | -.976 (.068) | -.692 (.088) | -.838 (.089) | -.774 (.090) | -.741 (.089) | -.969 (.073) | -.864 (.071) | -.823 (.088) | -.865 (.105) | .77 |
| #26 | .000 | -.039 (.027) | -.567 (.061) | -.627 (.081) | -.784 (.092) | -.530 (.112) | -.639 (.070) | -.582 (.099) | -.545 (.108) | -.685 (.083) | -.696 (.083) | -.716 (.085) | -.685 (.077) | -.561 (.077) | -.617 (.092) | .66 |
| #27 | .000 | -.080 (.045) | -.478 (.060) | -.758 (.095) | -.848 (.121) | -.738 (.099) | -.797 (.109) | -.648 (.089) | -.658 (.084) | -.593 (.110) | -.515 (.088) | -.800 (.094) | -.730 (.076) | -.592 (.094) | -.678 (.108) | .65 |
| #25 | .000 | -.137 (.047) | -.844 (.064) | -.545 (.102) | -.564 (.151) | -.643 (.132) | -.603 (.138) | -.505 (.120) | -.609 (.114) | -.606 (.101) | -.553 (.142) | -.547 (.117) | -.771 (.081) | -.771 (.125) | -.692 (.131) | .62 |
| #8 | .000 | -.072 (.076) | -.632 (.099) | -.422 (.077) | -.741 (.115) | -.526 (.075) | -.560 (.084) | -.459 (.068) | -.440 (.056) | -.522 (.063) | -.334 (.072) | -.652 (.073) | -.366 (.078) | -.393 (.073) | -.387 (.094) | .58 |
| #12 | .000 | -.361 (.067) | -.689 (.077) | -.424 (.087) | -.615 (.107) | -.138 (.088) | -.488 (.127) | -.509 (.091) | -.605 (.102) | -.459 (.084) | -.409 (.083) | -.623 (.083) | -.556 (.112) | -.321 (.067) | -.651 (.084) | .57 |

TABLE 1.—(Continued)

| | | | | | | | | | | | | | | | | |
|----------------------|------|-----------------|------------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----|
| # 1 | .000 | -.368 (.126) | -.717 (.062) | -.682 (.108) | -.332 (.135) | -.724 (.138) | -.652 (.120) | -.441 (.126) | -.579 (.099) | -.250 (.138) | -.568 (.085) | -.716 (.112) | -.624 (.098) | -.618 (.108) | -.584 (.141) | .54 |
| #14 | .000 | -.252 (.141) | -.644 (.067) | -.435 (.097) | -.482 (.136) | -.754 (.130) | -.589 (.194) | -.420 (.161) | -.680 (.109) | -.642 (.132) | -.573 (.109) | -.744 (.113) | -.743 (.113) | -.858 (.112) | -.396 (.093) | .53 |
| # 3 | .000 | -.288 (.141) | -1.028 (.079) | -.434 (.174) | -.880 (.232) | -.644 (.197) | -.683 (.258) | -.739 (.189) | -.672 (.152) | -.602 (.162) | -.572 (.216) | -.564 (.169) | -.416 (.144) | -.574 (.167) | -.347 (.120) | .48 |
| #16 | .000 | -.178 (.087) | -.732 (.060) | -.452 (.110) | -.254 (.202) | -.744 (.169) | -.629 (.171) | -.373 (.192) | -.730 (.209) | -.308 (.187) | -.900 (.284) | -.689 (.178) | -.500 (.118) | -.531 (.204) | -.275 (.130) | .45 |
| #22 | .000 | -1.97 (.049) | -.506 (.057) | -.565 (.093) | -.370 (.131) | -.448 (.129) | -.378 (.124) | -.145 (.139) | -.277 (.126) | -.308 (.109) | -.435 (.123) | -.445 (.146) | -.403 (.102) | -.519 (.103) | -.683 (.109) | .44 |
| # 2 | .000 | -.416 (.134) | -.437 (.053) | -.683 (.126) | -.553 (.158) | -.597 (.166) | -.419 (.159) | -.580 (.144) | -.568 (.126) | -.438 (.137) | -.701 (.155) | -.738 (.169) | -.541 (.125) | -.963 (.200) | -.252 (.137) | .42 |
| #19 | .000 | -.188 (.064) | -.356 (.055) | -.272 (.097) | -.230 (.196) | -.508 (.123) | -.247 (.127) | -.310 (.121) | -.418 (.096) | -.082 (.136) | -.096 (.136) | -.446 (.093) | -.217 (.113) | -.195 (.119) | -.517 (.093) | .31 |
| #24 | .000 | -.036 (.056) | -.336 (.043) | -.496 (.188) | -.536 (.227) | -.609 (.194) | -.735 (.140) | -.543 (.234) | -.150 (.186) | -.711 (.235) | -.409 (.218) | -.093 (.213) | -.656 (.189) | -.937 (.263) | -.514 (.232) | .31 |
| #21 | .000 | -.220 (.423) | -.574 (.161) | -.628 (.375) | -.590 (.457) | -.522 (.600) | -.738 (.558) | -.502 (.237) | -.540 (.361) | .071 (.422) | -.899 (.425) | -.134 (.488) | -.242 (.391) | -.384 (.484) | -.856 (.395) | .09 |
| Pooled ^a | .000 | -.176 (.027) | -.690 (.022) | -.564 (.035) | -.674 (.047) | -.647 (.037) | -.617 (.045) | -.748 (.026) | -.645 (.031) | -.156 (.006) | -.608 (.037) | -.803 (.026) | -.586 (.037) | -.593 (.035) | -.703 (.033) | .49 |
| Aggregate Weekly | .000 | -.222 (.056) | -.438 (.057) | -.411 (.055) | -.317 (.121) | -.412 (.087) | -.289 (.076) | -.235 (.072) | -.359 (.062) | -.256 (.064) | -.239 (.069) | -.446 (.066) | -.445 (.057) | -.343 (.064) | -.485 (.068) | .52 |
| Aggregate Monthly | .000 | -.050 (.038) | -.099 (.053) | -.488 (.055) | -.015 (.278) | -.982 (.206) | -.560 (.241) | -.459 (.294) | -.536 (.090) | -.726 (.148) | -.364 (.194) | -.827 (.119) | -.570 (.119) | -.525 (.223) | -.601 (.075) | .83 |

^a Coefficients of the bank variables are not shown. Values range from .0002 to -.0009, with *t*-values ranging from 4.3 to 0.4; the largest partial *R*² was .003.

present analysis, a bank can bring actual reserves into line with desired reserves. On the other hand, an aggregate (if it is a closed system) cannot alter conditions which are truly exogenous to it. The banking system cannot "shed" reserves. But as individual banks do so, those variables in terms of which its desired position is defined—e.g., total deposits, interest rates, etc.—change until desired reserves are brought into conformity with actual reserves.

Notwithstanding these considerations, aggregation across decision units

TABLE II—SPEED OF ADJUSTMENT

| Bank | Adjustment Coefficient | Period of Adjustment ^a | |
|-------------------|------------------------|-----------------------------------|--------|
| 3 | −1.028 | .8 | weeks |
| 13 | −.968 | .9 | " |
| 25 | −.844 | 1.6 | " |
| 11 | −.744 | 2.2 | " |
| 16 | −.732 | 2.3 | " |
| 1 | −.717 | 2.4 | " |
| 12 | −.689 | 2.6 | " |
| 14 | −.644 | 2.9 | " |
| 8 | −.632 | 3.0 | " |
| 5 | −.594 | 3.3 | " |
| 21 | −.574 | 3.5 | " |
| 26 | −.567 | 3.6 | " |
| 22 | −.506 | 4.3 | " |
| 27 | −.478 | 4.6 | " |
| 2 | −.437 | 5.2 | " |
| 18 | −.391 | 6.0 | " |
| 19 | −.356 | 6.8 | " |
| 24 | −.336 | 7.3 | " |
| 20 | −.264 | 9.8 | " |
| \bar{X} | .605 | 3.2 | " |
| Pooled | −.690 | 2.6 | " |
| Aggregate weekly | −.438 | 5.2 | " |
| Aggregate monthly | −.099 | 28.7 | months |

^a To a 95 per cent closure.

did not substantially increase the lag in bank adjustment from actual to desired reserves positions. Aggregation across banks, but not time, lowered the estimate of λ to $-.438$, implying a lag in adjustment of 5.2 weeks (Table II). While this lag is nearly twice as great as that with the micro data, it does not approach those reported in earlier studies. The aggregation of only 19 banks was insufficient to capture the blurring of empirical relationships which might occur at a higher level of aggregation. In a small open system, the adjustments of one decision unit need not impinge upon other units in the open system. It may be that longer lags would have been estimated had the system been closed.

Aggregation Across Time

The data were brought into close conformity with those used in other studies by averaging the aggregate weekly observations into months. Tests with these data yielded an estimate of λ of $-.099$, implying a 28.7-month lag (Table II). A lag of this length is comparable to those reported in earlier studies. Thus, once the data were made comparable, the lag estimates became comparable. The lengthening turns partly on the choice of period in terms of which the adjustment coefficient is specified. Thus, for example, there is no means by which quarterly observations can capture an adjustment period of one week or one month.

Beyond this, however, there was a sharp decline in the absolute value of estimated λ . Such a decline may reflect an increased prominence of specification error. Yair Mundlak has shown [5] that the determination of the period of adjustment is a crucially important ingredient in the specification of a distributed lag model. In particular, if the "true" period of adjustment is relatively short, it is inappropriate for a model aggregated over time to include a lagged stock of the dependent variable. Moreover, the statistical testing of such a misspecified model is likely to yield estimates of λ with low absolute values.

To the extent that one is inclined to accept this study's conclusion that the true period of adjustment is a matter of weeks, it follows that the specification of a monthly stock-adjustment model in this paper was inappropriate. In turn, the statistical results were likely to have resulted in an estimate of λ which was biased downward (absolute value). These observations tempt one to suggest that those studies which show that the banking system is slow to adapt—studies wherein stock-adjustment models were tested with monthly or quarterly data—may similarly reflect specification error.

III. Conclusions

Results from this study suggest that individual commercial banks respond rapidly to conditions which place them in disequilibrium. Thus, the inside lag between disequilibrating actions of the central bank—such as would be involved with an abrupt change in the rate of expansion in the reserve base—and equilibrating responses of the banking system can be relatively short. Based on individual bank observations taken at weekly intervals, this paper reports lags ranging from one to ten weeks. In earlier studies based on aggregate data organized around months or quarters, speeds or adjustment were reported ranging from several months to several years.

Why the difference? How can it be that studies of commercial bank behavior can reach results which are at such variance? One possible explanation is that their differences are a consequence of differences in the models. While such an explanation may have merit, based on this study it seems plausible to suggest that the estimates simply reflect differences in the way the data are organized. For, when data aggregated across banks and then across weeks into months were used, the lag estimate rose sharply. It rose

from weeks with micro, weekly data, to years with macro data aggregated across months.

At the very least, such results constitute evidence that estimates of structural coefficients based on aggregate data may yield very little information about the response characteristics of the institutions which make up the aggregate. A tentative but stronger conclusion is that the lag estimates of other studies reflect substantial specification error, and that these estimates are biased toward long lags. If so, policymakers have been provided with lag estimates which create an inaccurate impression of the length of the inside lag.

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* The author is associate professor of finance, University of Illinois. This study was begun and the computational work completed while the author was an economist at the Federal Reserve Bank of St. Louis. The generous support of that institution is greatly appreciated.¹ Discussions on the subject of this paper with George Betz and Edward Greenberg were quite beneficial. The draft was vastly improved by suggestions of Allan Meltzer and John Gurley.

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External Financing and the Rate of Economic Growth

This paper demonstrates that the degree of external financing of investment relative to internal financing, by business enterprises, depends on the growth rate of the economy.¹

Every growing economy has many industries, expanding or contracting at different rates. Within each, different business establishments expand or contract at rates varying from the industry average. Thus, the economy

¹ Importance of external financing is much discussed in finance books, but seemingly without reference to the general rate of economic growth. Cf. [6, pp. 338-40], [5, Ch. 16], [8, Ch. 25], [4, Ch. 15, 16].

consists of many business establishments growing or declining at a variety of rates.

Business enterprises do not have balanced portfolios of business establishments growing or declining at varying rates. Hence, firms show a variety of growth rates varying from the economy's average. Firms with high growth rates require external financing for investment since internal sources are inadequate. Firms with lower rates, thus less need for investment expenditures, do not. This note analyzes the relationship between total external finance and investment on a net basis, i.e., ignoring depreciation as a source and replacement investment (thus assumed equal to depreciation) as a use of funds.

Every Western-type economy is composed of many firms, with different rates of expansion or decline. These may be arranged, according to growth rates, in a frequency distribution, assumed normal around the economy's average growth rate.² Standard deviation σ measures the dispersion of firm growth rates.

The magnitude of σ depends on the economy's average growth rate ρ . Even if $\rho=0$, there is dispersion of firm growth rates; some firms decline, others grow, even though industry growth rates may all be zero.³ As the economy's growth rate rises, dispersion of firm growth rates around it also rises. With higher economic growth, some industries expand rapidly during earlier stages of their life cycle, others decline during later stages [3]. Hence, some firms grow rapidly; others decline. The faster economic growth, the faster is change, obsolescence, replacement of old by new [2, pp. 7-9], i.e. Schumpeterian creative destruction [7, Pt. 2, Ch. 7]. Thus the faster the growth, the greater is σ .⁴

$$(1) \quad \sigma = f(\rho), \quad \frac{d\sigma}{d\rho} > 0$$

We assume that p/k , the net rate of profit earned on capital by different firms in an economy in the same time period does not depend on their relative growth rates but is everywhere a constant, ϕ .⁵

$$(2) \quad \frac{\dot{p}}{k} = \phi$$

Every firm has some rate of growth or decline r , which equals the ratio of net investment i (increase in its capital stock) to capital stock k .⁶

² This assumption can be dropped at the expense of mathematical complexity.

³ A stationary state with all industry plus all business establishment growth rates zero may be visualized. Then business enterprise growth rates are zero.

⁴ Probably σ approaches an asymptote $\sigma(\max)$ as the rate of economic growth increases and an asymptote $\sigma(\min)$ as the rate of economic decline increases.

⁵ Alternately, the profit rate of individual firms could be an increasing function of the firm's growth rate for any given average profit rate in the economy.

⁶ Strictly speaking, eq. (3) specifies the growth rate of the firm's capital stock. It equals the growth rate of the firm's net value added when the capital/net value added ratio is constant.

$$(3) \quad \frac{i}{k} = r$$

Investment expenditures by the firm are a use of funds. The source of funds for these may be internal and/or external to the firm. Net internal sources equal net profits minus dividend payments.

The profit retention function may be described thus: firms with zero growth rates on the average have high relative dividend payments since there is no net need for internal funds for investment. With higher growth rates, firms on the average cut dividends as needs for financing investment rise [1, pp. 234-35]. When the firm's growth rate i/k equals profit rate p/k , it needs a sum equal to net profits to finance investment. This would argue for zero dividends, thus completely financing investment internally. However, it is well known that firms may pay out some dividends even when they need on balance all of internal funds generated, making up the difference through external finance. Thus we specify a dividend function such that the dividend payout ratio declines with increase in the firm's growth rate and such that a minimum payout ratio α is reached at or prior to the point when the firm must first resort to external finance. We define a critical growth rate r^* of the firm above which the firm must resort to external finance. With profits as the source of funds and both investment and dividends as the use of funds, we have at r^* ,

$$(4) \quad \frac{p}{k} - \left[r^* + \alpha \frac{p}{k} \right] = 0 \quad \text{or} \quad r^* = \frac{p}{k} (1 - \alpha)$$

and,

$$(5) \quad \rho = \frac{I}{K}$$

$$(6) \quad \rho \text{ is given exogenously.}^7$$

Figure 1 shows the frequency distribution of firm growth rates around the national rate, assumed independent of the firm's capital stock. The shaded right hand tail, beyond r^* , indicates firms needing external sources of finance due to high growth and thus high investment outlays. We recenter.

$$(7) \quad x = r - \rho$$

$$(8) \quad x^* = r^* - \rho$$

As in any normal curve,

$$(9) \quad y = \frac{1}{\sigma\sqrt{2\pi}} e^{-x^2/2\sigma^2}$$

⁷ Strictly speaking, the average growth rate of the economy's capital stock is specified exogenously. This equals the average growth rate of the economy's output when the capital/output ratio is constant. For convenience, we simply use the term "rate of growth" (for both firms and economy). Cf. footnote 6.

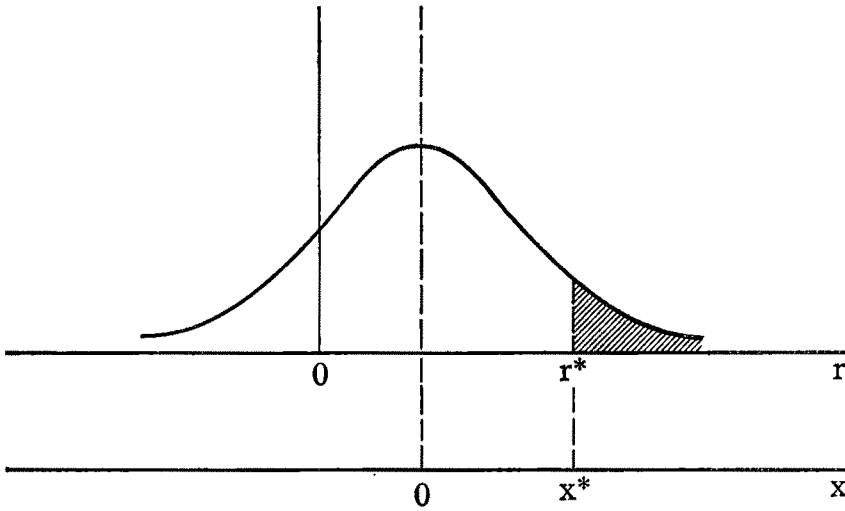


FIG. 1. FREQUENCY DISTRIBUTION OF GROWTH RATES OF FIRMS

Total external finance needed in the economy is derived thus: for all firms with growth rates above the critical growth rate, take the difference between sources (net profits) and uses of finance (net investment plus dividends), multiply by frequency of occurrence, and sum.⁸

$$(10) \quad E = K \int_{x=x^*}^{x=\infty} \left[\frac{i}{k} + \alpha \frac{p}{k} - \frac{p}{k} \right] y dx$$

where E is the total amount of external finance for the economy and K the total stock of capital in the economy, which is known.

$$(11) \quad K = \bar{K}$$

Combining and integrating, external finance/total investment is

$$(12) \quad \frac{E}{I} = \frac{[\rho - \phi(1 - \alpha)]Q + Z}{\rho}$$

where

$$Q = \left[\frac{\text{Normal}}{\text{Curve}} \right]_{-\infty}^{\infty} \frac{x}{\sigma} = \frac{r^* - \rho}{\sigma}$$

and

$$Z = \frac{\sigma}{\sqrt{2\pi}} e^{-(r^* - \rho)^2 / 2\sigma^2}$$

⁸ Essentially, this equation assumes that the economy is composed of a large number of small firms.

Table 1 lists calculations for percentage of total investment financed externally for a sample of values of: average growth rate; average profit rate; standard deviation of firm growth rates distribution; minimum dividend payout ratio. For example, with a 10 per cent profit rate, 4 per cent growth rate, 8 per cent standard deviation, and zero minimum payout ratio (99.74 per cent of firms in the range -20 per cent to +28 per cent growth rates), 26.25 per cent of investment is financed externally.

TABLE 1—SELECTED COMBINATIONS OF FACTORS DETERMINING EXTERNAL FINANCING

| $\rho = 4\%$ $\phi = 10\%$ | $\alpha = 0$ | $\rho = 4\%$ $\sigma = 8\%$ | $\alpha = 0$ | $\phi = 10\%$ $\sigma = 8\%$ | $\alpha = 0$ | $\rho = 4\%$ $\phi = 10\%$ | $\sigma = 8\%$ |
|-------------------------------|-------------------|--------------------------------|-------------------|---------------------------------|-------------------|-------------------------------|-------------------|
| σ | E/I (per cent) | ϕ | E/I (per cent) | ρ | E/I (per cent) | α | E/I (per cent) |
| 4 | 2.93 | 9 | 32.40 | 1 | 52.20 | 0.00 | 26.25 |
| 5 | 7.01 | 10 | 26.25 | 2 | 33.31 | 0.05 | 29.48 |
| 6 | 12.48 | 11 | 21.01 | 3 | 28.02 | 0.10 | 32.39 |
| 7 | 19.01 | 12 | 16.65 | 4 | 26.25 | 0.15 | 35.84 |
| 8 | 26.25 | 13 | 13.05 | 5 | 25.92 | 0.20 | 39.56 |
| 9 | 33.74 | 14 | 10.13 | 6 | 26.38 | | |
| 10 | 42.16 | 15 | 7.76 | 7 | 27.35 | | |

An estimate has been made for the standard deviation of the firm growth rates distribution based on 75 U.S. corporations, largely in manufacturing, for the 1959-64 period. For this sample, the standard deviation was 7.8 per cent and the average growth rate 5.8 per cent.⁹ The median rate of return on invested capital for the 500 largest U.S. corporations was 9.1 per cent in 1963; the rate of profit for all U.S. manufacturing corporations was some 10.2 per cent that year [10, Tables 678, 688].

Combining equations (1), (2), (4), and (12), it can be seen that the degree of external financing of investment relative to internal financing is a function of the rate of economic growth, when ϕ and α are given.

If we consider the degree of external financing of investment on a gross rather than a net basis, i.e., introducing differences between depreciation and replacement investment, the results of Table 1 must be modified. External finance needed to finance gross investment will then be substantially lower, since depreciation allowances exceed replacement investment for growing firms, the difference rising with the growth rate. If price changes are introduced, depreciation on original cost instead of a revalued basis di-

⁹ This sample, selected from a larger sample of 372 corporations from [9], comprised those companies which showed no mergers, acquisitions, new consolidation of subsidiaries, etc. Both standard deviation and average growth rate are unweighted averages for the companies concerned. It should be noted that the average growth rate of 5.8 per cent exceeds that for undeplicated GNP (5.4 per cent), in part because of the inclusion of one high growth company, in part due to the fact that sales of some U.S. firms include those of their foreign subsidiaries. The average growth rate of GNP in 1958 prices was 3.9 per cent during this period.

minishes the real value of depreciation when prices rise, and increases the degree of external finance.

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The Neoclassical Theorem Once Again: Closed and Open Economies

In a recent note in this *Review* [2], Alvin Marty has presented a diagrammatic exposition of the neoclassical theorem (Golden Rule) that among balanced growth paths consumption per man is maximized on the balanced growth path where the marginal product of capital is equal to the natural rate of growth, or equivalently where the elasticity of output (or the share of capital under constant returns to scale and competitive distribution) equals the saving rate [3] [4]. The purpose of this note is to provide an alternative, simpler diagrammatic proof, showing that the Golden Rule is an application of the usual economic rules for optimization and to extend the Golden Rule to simple open economies.

Golden Ages and Golden Rules in a Closed Economy

Assume for the moment a one-good economy, with no technical change. Within this context the neoclassical theorem is a rule for choosing the constant saving rate, and therefore capital-to-labor ratio and balanced growth path, which will yield the maximum sustainable per capita consumption. To derive the theorem we will use Figure 1, first constructed by John Fei, and recently used by James Tobin [5]. On the horizontal axis we measure

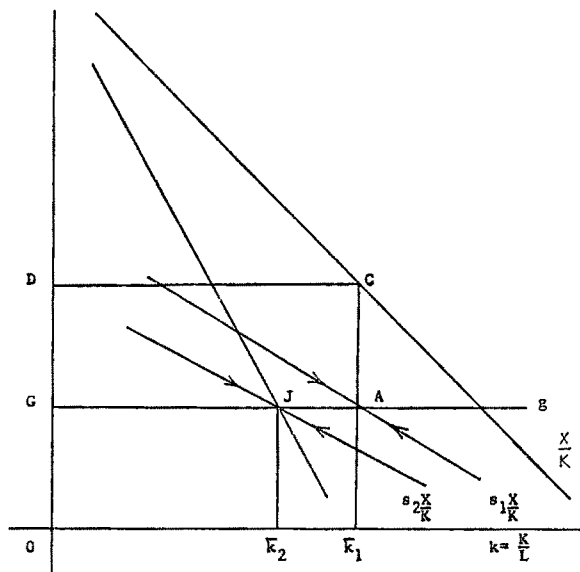


FIGURE 1

the ratio of capital to labor (capital intensity). On the vertical axis we measure pure numbers, such as growth rates, or, as output (X) and capital (K) are measured in the same units, the ratio of output to capital.¹

The curve X/K represents the average product of capital, which falls as the capital intensity rises, due to the law of diminishing returns to a variable factor.² The height of the horizontal line, Gg , represents the growth rate of labor, g , which Harrod has christened the natural rate of growth.³

Assume that some constant saving ratio, s , prevails for all time. It is only through this saving and implied consumption behavior that demand enters the picture. Once we make this assumption the theorem becomes technological. The growth rate of capital is sX/K and we represent it on the graph by lines proportional to X/K , $s_1(X/K)$ and $s_2(X/K)$. If $s = s_1$, the system can now be seen to converge to some equilibrium, A , with a constant K/L , by the usual simple reasoning, that if capital grows faster than labor the capital to labor ratio must increase, etc. The graph thus shows us that because of the technological phenomenon of diminishing returns a constant

¹ We assume throughout only one commodity to avoid index number problems. This also means that the marginal product of capital is a pure number, which can be identified with the rate of interest.

² The lines are drawn straight only for simplicity. The average product is shown starting from some positive number greater than g and ending at zero. Of course, this is not necessarily so, for example, some C.E.S. production functions approach a positive number rather than zero asymptotically; but for a state of balanced growth to occur, it is sufficient that the average product of capital be greater than the growth rate of labor as K/L approaches zero, and less than g as K/L approaches infinity.

³ Of course, g need not be a constant; it might depend on the income per head, or the natural rate of growth might also represent the rate of growth of labor in efficiency units.

saving ratio cannot maintain anything but $\bar{k}_1 A$ (equals g) as the growth rate of capital, so that eventually capital and labor must grow at the same rate. Our choice of s determines where a point like A lies along Gg , as well as the equilibrium capital intensity, \bar{k}_1 .

The rectangle $O\bar{k}_1CD$ is $X/K \cdot K/L$ or the output per man associated with \bar{k}_1 and s_1 . Now notice that, for any balanced growth path, the growth rate of capital is always represented by OG , or $\bar{k}_1 A$. Saving per man is represented by a rectangle $O\bar{k}_1AG$. The rectangle above the population growth curve ($GACD$), obtained by subtracting saving per man from output per man, equals consumption per man; and the objective is to maximize the area of this rectangle.

Let us recall the problem of a monopoly with constant marginal costs, like Gg in Figure 1, and facing an average revenue or demand curve, like X/K . For any output the rectangle $GACD$ represents profit. Maximum profit is attained by increasing output until marginal revenue equals marginal cost. The same principle applies in the case of a neoclassical economy seeking maximum sustainable consumption per man. The marginal to the average product of capital, in the same sense that marginal revenue is marginal to the demand curve, is the marginal product of capital (f_K), that is the addition to output per man from a rise in the capital to labor ratio. The additional cost incurred by increasing the equilibrium capital to labor ratio is simply the extra saving per man which must be made in every year to keep the capital to labor ratio at this new, higher level. A small increase in the equilibrium capital to labor ratio always requires OG extra saving per man, the rest of the extra output per man goes to consumption.

Just as the profit-maximizing firm increases output until the marginal revenue on the last unit produced equals its marginal cost, so an optimizing community should increase the capital to labor ratio by varying the saving rate and capital intensity until the addition to output per man equals the extra saving per man. On all previous increases in capital intensity the increase in per capita product exceeded the increase in per capita saving, yielding additions to per capita consumption. Any further increase in capital intensity will yield a change in per capita output which is smaller than the required additional saving, and therefore per capita consumption will be reduced.

By the above reasoning an optimizing community chooses a saving rate (s_2) and capital intensity (\bar{k}_2) such that the marginal product of capital equals the growth rate of population ($f_K = g$). Since this is an equilibrium capital intensity, which will be maintained by balanced growth, the growth rate of capital also equals the growth rate of population ($s(X/K) = g$). Therefore the growth rate of capital equals the marginal product of capital, ($s(X/K) = f_K$), which implies that the saving rate must equal the elasticity of output with respect to capital, or under competitive distribution, the share of capital

$$\left(s = \frac{f_K K}{X} = \frac{f_K}{\text{Average Product of Capital}} \right).$$

Innovation

The above diagrammatic formulation is also useful because the effect of innovation on the growth path can easily be ascertained. A single once-and-for-all innovation may be classified in the Hicksian sense as capital saving, neutral, or labor saving, depending on whether the marginal product of capital increases less, the same, or more than the marginal product of labor⁴ at the existing capital to labor ratio. In a constant returns to scale production function, this is equivalent to inquiring whether the marginal product of capital increases relatively less than, the same as, or more than the average product of capital [1, pp. 212-15, 257-59]. The effect on the equilibrium of a single technological change is to leave equilibrium growth rates unchanged but to raise the optimum K/L and equilibrium consumption per man. The exact increase depends on the type and intensity of innovation, and the extent of diminishing returns as the capital-to-labor ratio increases.

When we turn to continuous innovation, the case of pure labor augmentation is easily analyzed. In this case each laborer becomes λ per cent more productive each year. We may now define the labor force in efficiency units \hat{L} , where an efficiency unit is equivalent to the productive ability of a man in the base year.

If we now interpret capital intensity on the X axis as capital per efficiency laborer, rather than the capital-to-labor ratio, then the average and marginal products remain the same for each capital intensity [6]. If we let the height of line Gg represent the growth rate of efficiency labor ($\lambda+g$), then it is easy to see that a constant saving rate again implies some stable capital intensity. The rectangle $GACD$ now represents a constant consumption per efficiency laborer, and, since the number of laborers per efficiency laborer declines at λ per cent per year, consumption per man increases at the rate of λ per cent every year. Constraining ourselves to a choice among balanced growth paths, the objective is now to maximize the consumption per efficiency laborer since that will also lead to a choice of the highest of the rising per capita consumption time paths, and therefore maximize the steadily rising per capita consumption in every period. Using the above method we find that we should again choose a saving rate s_2 and implied capital intensity K/\hat{L} such that the marginal product of capital equals the growth of efficiency labor ($f_K = \lambda+g$). Since we are choosing between balanced growth paths, the growth of capital also equals the growth of efficiency labor, ($s(X/K) = \lambda+g$), and we obtain the same result; the rate of saving should be set equal to the elasticity of output with respect to capital ($s = f_K K/X$).

Is the optimum saving rate chosen above the same as the optimum rate in an economy with no technical change? Labor augmenting technical progress means that at each interest rate the ratio of capital to efficiency labor is equal to the ratio of capital to labor in a world without technical change. This means that we can compare on one graph, using the same mar-

⁴ Although they can be easily handled, the very labor-saving and very capital-saving types of innovations are not considered.

ginal and average product schedules, two economies which differ only because one has labor-augmenting technical change, provided we write capital per efficiency laborer on the horizontal axis. In the economy with labor-augmenting technical progress, our optimum savings rule will lead us to choose a higher marginal product of capital and therefore a lower capital intensity, just as it would if, instead of technical progress, the second economy had a more rapidly growing labor force. This will mean the optimum saving rate, equal to the share of capital, will be greater, equal to, or less than the optimum saving rate in a world without technical progress, as the elasticity of substitution

$$\left(\frac{\text{percentage change in } K/L}{\text{percentage change in } \frac{\text{Wages}}{\text{Interest}}} \right)$$

is less than, equal to, or greater than one.

Golden Ages in an Open Economy

We now consider the case of the small economy in a large world which, by virtue of its relative smallness, faces an interest rate on world capital markets that is invariant to its own borrowing or lending activities.⁵ We assume that the profit-maximizing behavior of cosmopolitan capitalists insures that capital will flow into or out of this small country as domestic investment opportunities (created within its own borders) at the world rate of interest exceed or fall short of saving from national income (accruing to its citizens).⁶

With the marginal product of domestic capital held equal to the world rate of interest in this way, the question of optimal saving in the open economy is fundamentally different from that in the closed economy. As we have seen, the closed economy could adjust its equilibrium marginal product of capital (f_k) relative to its equilibrium growth rate (g) by choosing among alternative saving ratios. But the open economy, by assumption, must regard the marginal product of its capital as given, and equal to the world interest rate. Since its equilibrium growth rate is also given, exogenous forces, not social choice, determine f_k relative to g . However, by choosing among alternative saving ratios, the open economy can adjust its long-run debtor-creditor position vis-à-vis the rest of the world, seeking maximum sustainable per capita consumption.

We will find that among equilibrium growth paths, the addition to na-

⁵ In the spirit of the discussion of the closed economy we continue to assume a one-good world. This assumption totally suppresses terms-of-trade and monetary considerations which are usually entered into the analysis of capital movements. It is not, of course, necessary to consider these variables in a priori explanations of capital movements although one may wish to do so at another level of analysis.

⁶ The term "domestic" refers to the labor and capital employed in the country, and the income and product they generate. "National" refers to income accruing to citizens (or residents, in this case) of the country. National income and domestic product differ in the present model if there are international interest payments or receipts.

tional income per man from an increment of saving is equal to the world rate of interest ($=f_k$) while the extra saving per man is equal to the natural rate of growth, g . This is a familiar notion developed in the foregoing sections. However, for the open economy, each of these is invariant to saving behavior. Consequently, the country should save "as little as it can" ("as much as it can") if its natural rate of growth exceeds (falls short of) the world rate of interest.

In terms of the marginal cost—marginal revenue interpretation of golden rule saving suggested above, marginal cost and marginal revenue are, by assumption, horizontal straight lines which coincide only by accident. In analogous circumstances, the profit-maximizing firm can always increase profits (decrease losses) by expanding (contracting) production if $MR > MC$ ($MR < MC$). This implies indefinite expansion of contraction unless some constraints are imposed (e.g., production ≥ 0). We shall see that constraints are also, in general, required to limit the debtor-creditor position of the open economy seeking an optimum Golden Age. First, it is necessary to define a Golden Age for open economies and to establish existence and stability conditions.

Phelps [4, p. 794] defines a Golden-Age path as "... a growth path in which literally every variable changes (if at all) at a constant relative rate." For the closed economy, the definition implies that "... if investment is positive then output, investment and consumption must all grow at the same (constant) rate" [4, p. 794]. Here, of course, there is no distinction between domestic and national accounting variables.

For the open economy, national income exceeds (falls short of) domestic product if there are interest receipts (payments) on net foreign credit (debt). Consequently, we must extend Phelps' basic definition to require that national income and each of its components grow at the same (constant) rate as domestic product and each of its components. We shall see that a necessary condition is that the ownership of capital by residents (whether employed at home or abroad) grows at the same (constant) rate as national income (whether earned at home or abroad).

Figure 2 portrays the small, open economy. Imposing a world interest rate (the horizontal Rr line) means that international capital flows will guarantee that a particular capital-labor ratio, \bar{k} , will always prevail so long as technical progress either is absent or can be described as labor augmenting.⁷ Domestic product per worker is thus locked in at $O\bar{k}CD$. Consequently, a constant world rate of interest ensures that domestic capital and output grow at the same rate, g , as labor. In the figure, the growth rate of labor is represented by the horizontal line G_{lg} . The growth rate, g , and the interest rate coincide only by accident and each is invariant to national saving behavior.

This completes the picture of the domestic economy. We now turn to a consideration of national income variables.

National income and additions to the nationally owned stock of capital

⁷ Subsequent references to labor will assume that it is measured in efficiency units.

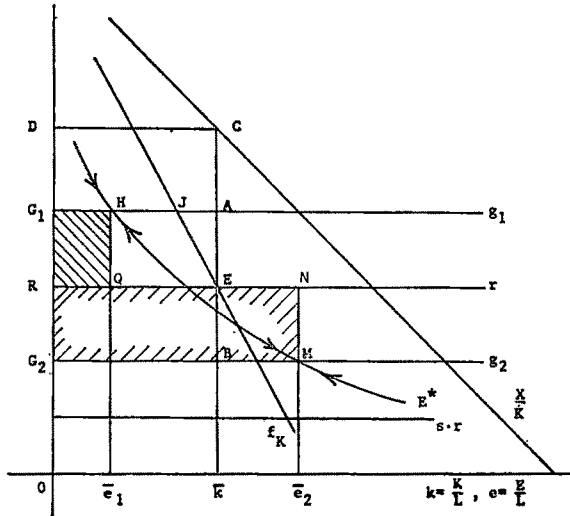


FIGURE 2

depend on saving behavior and we now derive conditions such that these two variables grow at the same rate. Define E as national equity, or the nationally owned stock of capital. Its analog in the closed economy is the domestic capital stock, K . The open economy is a net debtor (creditor) if E falls short of (exceeds) K . Saving is the net addition to equity:

$$\frac{dE}{dt} = S.$$

Saving is a constant proportion of national income, which is composed of wage payments to domestic labor plus interest payments on national equity whether employed at home or abroad.⁸ We assume that the same proportion of the two kinds of income is saved. Saving, by assumption, cannot be negative.

$$S = sY = swL + srE, \quad s \geq 0.$$

⁸ More conventionally, national income is domestic product minus (plus) net interest payments (receipts).

$$Y = X - rD$$

where X is domestic product and D is the net debt position,

$$D = K - E.$$

Separating X into its distributive components,

$$X = wL + rK$$

and substituting these last two expressions into the first yields:

$$Y = wL + rE.$$

Dividing through by E , letting

$$E^* = \frac{1}{E} \cdot \frac{dE}{dt} = \frac{1}{E} \cdot S \quad \text{and} \quad e = \frac{E}{L},$$

$$E^* = sr + sw(r) \cdot \frac{1}{e},$$

where w is a function of r .⁹ The growth of equity, E^* , is a declining function of the equity-labor ratio, e .

By plotting the equity-labor ratio, e , on the horizontal axis along with the capital-labor ratio, k , we can superimpose the equation just derived on the picture of the domestic economy to portray the growth of national income variables as well as those which refer to domestic product.

From our definition, a golden age exists if the E^* line intersects the labor growth line, G_1g_1 , at point H where an equilibrium equity-labor ratio is established at \bar{e}_1 . Equity, E , grows at the same rate, g_1 , as labor. It is easy to show that this guarantees that income grows at that rate, g_1 , also.¹⁰ Existence conditions for a Golden Age are consequently nothing more than intersection conditions for the E^* and the Gg lines at a value of $0 \leq e < \infty$. Algebraically, existence of a Golden Age requires that

$$g = sr + sw(r) \cdot \frac{1}{e}$$

have a solution for $0 \leq e < \infty$. Solving for e ,

$$e = \frac{sw(r)}{g - sr}$$

The necessary and sufficient condition for existence is therefore

$$g > sr$$

on the assumption that $sw(r) \geq 0$. Graphically, this condition simply ensures that the E^* line does not lie wholly above Gg . Since $E^* \rightarrow sr$ as $e \rightarrow \infty$, E^* will always exceed g if $g < sr$ for $e < \infty$. If the growth of equity, E , forever

⁹ For linear, homogeneous production functions and with competitive distribution: $w = f_L(k)$ and $r = f_K(k)$. Since r is a parameter in this case, $k = f_K^{-1}(r)$. By substitution, $w = f_L[f_K^{-1}(r)]$, or simply, $w = w(r)$.

¹⁰ Since $Y = wL + rE$, it follows that

$$\frac{1}{Y} \cdot \frac{dY}{dt} = Y^* = \frac{wL}{Y} L^* + \frac{rE}{Y} E^*,$$

or

$$Y^* = \frac{wL}{Y} g + \frac{rE}{Y} g,$$

or

$$Y^* = g.$$

exceeds the growth of labor and domestic product, Golden Age conditions are violated.¹¹

Moreover, if the condition for a Golden Age solution is satisfied, the solution is stable. Without rigorous demonstration, we offer the following intuitive proof: since the E^* line slopes downward to the right, the intersection at a point like H on the G_1g_1 line establishes an equilibrium e at e_1 , for if e is displaced, say, to the right of e_1 , labor will grow faster than equity, thereby restoring e to its original position.

Golden Rules for Open Economies

The analysis of Golden Rule saving for closed economies suggested that the growth rate (g) could be viewed as the marginal cost of additional saving and the marginal product of capital (f_k) as the associated marginal revenue.¹² In choosing among Golden Age saving ratios, the closed economy could vary the marginal product of capital (r) so that consumption was maximized where $MC=MR$, or $g=r$. For the open economy, the marginal cost of additional saving (g) equals its associated marginal revenue (r) only if g and r coincide by accident. If that should happen, however, society should be indifferent among alternate saving ratios.

More generally, $g \neq r$. If $g > r$ the MC of additional saving exceeds the associated MR so the Golden Rule of saving is to save nothing. Conversely, if $g < r$, $MC < MR$, so society should save as much as it can without violating Golden Age existence conditions.

These rules can be illustrated in Figure 2. Assume first that $g > r$ and a Golden Age is established at points H (for national income variables) and E (for domestic product variables) with $g=g_1 > r$, $e=\bar{e}_1$ and $k=\bar{k}$. In per worker terms, national income is wages, $RECD$, plus returns to equity, $O\bar{e}_1QR$.¹³ Saving is $O\bar{e}_1HG_1$.¹⁴ Consumption is national income minus saving, $(RECD + O\bar{e}_1QR - O\bar{e}_1HG_1)$, or $(RECD - RQHG_1)$. Since wages, $RECD$, are invariant to saving behavior, due to capital flows, consumption is maximized when the shaded rectangle, $RQHG_1$, is minimized. This is accomplished by choosing a Golden Age where nothing is saved. National income is then wages, $RECD$ and all wages are consumed.

Assume now that $g < r$ and a Golden Age is established at points M (for national income variables) and E (for domestic product variables) with $g=g_2 < r$, $e=\bar{e}_2$ and $k=\bar{k}$. In per worker terms, national income is wages, $RECD$, plus returns to equity, $O\bar{e}_2NR$. Saving is $O\bar{e}_2MG_2$. Consumption is

¹¹ An interesting extension of this analysis is to the unstable case where $E^* > sr > g$. In that case, national income always grows faster than domestic product and labor. The question of optimal savings in that circumstance involves a number of considerations beyond the scope of this short note.

¹² With g and r as constants, marginal costs and revenues are equal to average values.

¹³ Wages are domestic product, $O\bar{k}CD$, minus returns to domestic capital, $O\bar{k}ER$, or $RECD$. Returns to equity are equity, $O\bar{e}_1$, times the rate of interest, OR , or $O\bar{e}_1QR$.

¹⁴ $\frac{S}{L} = \frac{dE}{dt} \frac{1}{L} = \frac{dE}{dt} \frac{1}{E} \cdot \frac{E}{L} = E^* \cdot e = g \cdot e$. In this example, $\frac{S}{L}$ equals OG_1 times $O\bar{e}_1$ or $O\bar{e}_1HG_1$.

national income minus saving, $(RECD + O\bar{e}_2NR - O\bar{e}MG_2)$, or $(RECD + G_2MNR)$. Again, wages, $RECD$, are invariant to saving, so consumption is maximized when the shaded rectangle G_2MNR is maximized. This is accomplished by saving as much as possible without violating existence conditions for Golden Age growth. That is:

$$s = \frac{g}{r} - \epsilon \text{ where } \epsilon > 0 \text{ but as small as possible.}$$

For example, if the growth rate, g , equals 3 per cent and the world interest rate equals 6 per cent, the economy should save just under one-half its national income. If it were to save more, Golden Age conditions are violated. The growth of national income would forever outpace the growth of domestic product and labor. While this situation may be preferred to any Golden Age, we are not equipped to analyze it with the tools at hand.

Clearly, if $g=r$, neither of these shaded rectangles exists. Marginal cost always equals marginal revenue. Consumption is unrelated to saving. This result gives us another opportunity to contrast optimal saving in closed and open economies: in the closed economy, consumption is maximized when saving is such that $g=r$; in the open economy, consumption is invariant to saving behavior when $g=r$ (a parametric conjunction).

To put the argument another way: for the open economy, and as between equilibrium growth paths, the gross decline in consumption per worker due to an increment in saving is always the growth rate, g . This effect is countered by a concomitant rise in national income per worker, always equal to the world interest rate, r . The net effect on per worker consumption is r minus g . Consequently, if $r < g$ ($r > g$) consumption per worker is maximized when nothing ("as much as possible") is saved. If $r=g$, the net effect of changing saving on consumption is zero.

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Patterns of Stability in Foreign Trade: OECD and COMECON, 1950-1963

The stability of the trade between nations is only one aspect of their economic relations—in addition to the volume and the growth of trade, the terms of trade, and the exchange rates to name a few—but it is a rather important one. The experience of the 1930s demonstrated how instabilities within countries can be transmitted to others through trade; serious concern with the problem of international economic instability continues to the present [6, pp. 406–51] [13] [20] [24] [25, pp. 93–126].

One of the advantages claimed for economic planning of the Soviet type is that it makes for stabler international trade:

In contrast to the autarky and the unpredictable conjunctural fluctuations inherent in the foreign trade of the capitalist countries, trade in the world socialist market develops in conformity with the requirements of the planned economies of the socialist-camp countries. There can be temporary disproportions in this market caused, for example, by inadequacies in coordinating the economic plans of the socialist states. But such disproportions are of a particular and temporary nature. . . . [3, pp. 44–45];

The greatest evil which always beset all underdeveloped countries on the foreign market . . . was that the cyclical factors and fluctuations inherent in the capitalist economic system led to sharp and quite sudden fluctuations in demand for their goods. Trade with the Soviet Union imparts an increasingly apparent element of stability . . . on the commodity markets [23, p. 19].

Claims such as these have been very frequent [8] [18, p. 43] [27] [50, pp. 15–17] but have not been tested by the writers although the necessary data exist. This paper reports the results of statistical investigations of imports and exports stabilities for the OECD and the COMECON blocs. The investigation covers: (1) the comparative stability of the trade of individual countries composing the blocs vis-à-vis all countries, the blocs, and the underdeveloped countries; (2) the comparative stability of total trade of each bloc, for trade within each bloc, and between each bloc and the underdeveloped countries; and (3) the behavior of trade of the two large powers, the Soviet Union and the United States.¹ Part I discusses the approach used in the study and presents the results. Part II evaluates the reliability of the findings and Part III examines the determinants of the trade stability patterns.

¹ J. D. Coppock and E. Neuberger have dealt with some aspects of the problem of comparative trade stabilities. Among other differences, Coppock used for "total Soviet trade" only the figures on trade with the non-Communist countries. These figures amounted to a small fraction of the actual total during the period under consideration [1, pp. 70–71]. Neuberger, on the other hand, limited his research to trade in primary products and considered only a few countries [20, 21].

I. *Approach and Results*

The study of trade stability can be based either on the country's aggregate trade vis-à-vis the "rest of the world" or on binary, country-pair comparisons involving a large number of trading partners for each country. This study is based on country-pair comparisons. Total trade flows are much too aggregative and can conceal important differences in stability. For example, the imports of a developed country from other developed countries might be quite stable, but imports from less developed countries might be subject to large fluctuations. Or the shift from one supplier to another will not be reflected in the totals yet both suppliers will be affected in opposite directions by the shift.

The decision to use the country-pairs in turn dictated the next choice: whether to study the stability of the physical volume of trade, the prices, or the values of imports and exports. The present study employs trade data expressed in current prices; data in constant prices were not available in sufficient detail for country-pair comparisons. The use of current values data is not necessarily a drawback. In some respects, most importantly in regard to current purchasing power of underdeveloped countries, fluctuations in value terms are as important as fluctuations in price or physical volume [37, p. 2].

Several statistical measures of instability are available. This study uses the "normalized standard error," which is defined as the standard error of estimate of the least squares fit to yearly trade data divided by the average volume of trade.² This measure eliminates from the original time series data the growth and volume components; the resulting stability indicators are pure numbers. Regressions were estimated using both the actual observations and their logarithms. The first type of estimate assumes that a constant absolute increment (or decrement) represents perfect stability, the second accepts a constant rate of increase (or decrease). The smaller of the two stability measures was chosen for each country to represent the stability of imports or exports. More complex fits were not attempted.

For each country considered in this study, two stability indicators were calculated. One is the median value of all individual country-pair stability indicators; the median is used for it does not assume either cardinality or exactness of the individual indicators. The other is an aggregate stability indicator which is derived by weighting the individual indicators by the corresponding volume of trade;³ the aggregate stability indicator represents

² The stability indicator for each import and each export flow is defined as

$$S_i = \frac{SEE_i}{A_i},$$

where SEE_i stands for the standard error of estimate and A_i stands for average volume of import or export from country i . For other possible measures of instability and their relative merits, see [1, pp. 23-26] [14, pp. 48-50] [21, pp. 23-28, 85-93].

³ The procedure is equivalent to normalizing the summation of all the standard errors of estimate by the yearly import or export volume totals. The aggregate stability indicator for

the stability of imports and exports of each country, or each bloc, vis-à-vis all other countries taken together and is thus somewhat comparable to the usual measures of stability for total imports and exports [1, pp. 49-79]. The calculations are presented in Tables 1 and 2 for a number of countries and for the OECD and COMECON blocs. They cover total trade, trade with the OECD and COMECON groups, and trade with the underdeveloped countries.

The results of the calculations can be summarized in the following way:⁴

1. The United States exhibited a surprising stability for both total imports and exports flows; these were more stable than either OECD, COMECON, or Soviet flows. U. S. imports provided about the same stability for the rest of the OECD countries as the Soviet Union provided for the rest of the COMECON countries. It offered stabler markets for the underdeveloped countries than did all the OECD countries, the Soviet Union, or all COMECON countries. U. S. exports exhibited similar stability.

2. The stability of Soviet total imports and exports was average; it was about the same as the OECD and COMECON averages. The Soviet imports from and exports to the underdeveloped countries were considerably less stable than the corresponding U. S. flows; they appear less stable than the total OECD flows, but more stable than the total COMECON flows.

3. The OECD had better stabilities of imports and exports than the whole COMECON bloc and it had more stable trade with the underdeveloped countries than did the COMECON. The OECD imports from the COMECON countries were more stable than the COMECON imports from the OECD countries.

4. The trade instabilities of both the OECD and the COMECON blocs were much higher vis-à-vis the underdeveloped countries than with their other trading partners.⁵

5. Intra-bloc trade appears to have been more stable for the COMECON countries than for the OECD bloc.

II. *Reliability of Findings*

The reliability of the results obtained can be considered in two parts: first, the adequacy of the underlying data; and second, the statistical significance of the differences in stability indicators.

Basic figures for imports and exports are available in United Nations,

each country (W_j) equals

$$W_j = \frac{\sum_{i=1}^M S_i \frac{A_i}{\sum_{i=1}^M A_i}}{\sum_{i=1}^M \frac{SEE_i}{\sum_{i=1}^M A_i}}$$

where M stands for the number of trading partners of country j .

⁴ See Table 2 for significance of differences in stability indicators.

⁵ The only exception was the U.S. exports which did not differ statistically from total U.S. exports and the Soviet imports which did not differ from total Soviet imports.

TABLE 1—STABILITY INDICATORS OF SELECTED OECD AND THE COMECON COUNTRIES^a

| Import from ^b and Export to: | Total ^c | | | | OECD | | | | COMECON | | | | Underdeveloped ^d | | | |
|--|--------------------|----------------|--------|-----|--------|-----|--------|-----|---------|-----|--------|-----|-----------------------------|-----|--------|-----|
| | Import | | Export | | Import | | Export | | Import | | Export | | Import | | Export | |
| | M ^e | W ^e | M | W | M | W | M | W | M | W | M | W | M | W | M | W |
| Austria (65; 70) | .28 | .15 | .26 | .16 | .23 | .12 | .16 | .11 | .18 | .17 | .21 | .22 | .40 | .37 | .34 | .38 |
| Belgium (87; 91) | .29 | .14 | .27 | .19 | .13 | .09 | .18 | .14 | .22 | .19 | .27 | .33 | .40 | .30 | .29 | .33 |
| France (80; 83) | .29 | .20 | .27 | .20 | .22 | .17 | .17 | .14 | .25 | .19 | .36 | .36 | .35 | .23 | .27 | .25 |
| Germany (F.R.) (95; 96) | .21 | .18 | .22 | .17 | .13 | .12 | .12 | .12 | .20 | .20 | .29 | .31 | .24 | .27 | .23 | .25 |
| Greece (51; 38) | .30 | .24 | .35 | .26 | .22 | .17 | .36 | .24 | .30 | .30 | .31 | .26 | .42 | .53 | .41 | .51 |
| Italy (91; 93) | .32 | .23 | .27 | .20 | .21 | .20 | .18 | .14 | .33 | .34 | .28 | .28 | .34 | .28 | .51 | .29 |
| Portugal (51; 58) | .39 | .21 | .29 | .35 | .21 | .15 | .22 | .43 | .48 | .49 | .42 | .39 | .43 | .27 | .36 | .18 |
| Turkey (38; 35) | .51 | .34 | .47 | .35 | .41 | .29 | .38 | .28 | .50 | .46 | .50 | .49 | .66 | .44 | .66 | .63 |
| United Kingdom (99; 101) | .22 | .15 | .18 | .15 | .11 | .12 | .12 | .11 | .18 | .17 | .26 | .23 | .28 | .22 | .19 | .20 |
| United States (105; 106) | .21 | .14 | .20 | .18 | .12 | .09 | .16 | .13 | .33 | .28 | .71 | .63 | .24 | .18 | .21 | .24 |
| Total OECD (1, 326; 1, 241) | .29 | .17 | .26 | .15 | .19 | .13 | .19 | .13 | .29 | .25 | .34 | .35 | .35 | .23 | .30 | .26 |
| Bulgaria (19; 24) | .37 | .20 | .39 | .15 | .37 | .48 | .34 | .36 | .19 | .14 | .18 | .10 | .74 | .66 | .57 | .50 |
| Czechoslovakia (28; 28) | .32 | .18 | .28 | .18 | .33 | .36 | .23 | .25 | .13 | .10 | .14 | .11 | .38 | .37 | .50 | .45 |
| Germany (D.R.) (56; 54) | .33 | .15 | .28 | .13 | .26 | .24 | .19 | .19 | .12 | .09 | .10 | .09 | .49 | .44 | .44 | .46 |
| Hungary (43; 55) | .37 | .23 | .33 | .27 | .29 | .27 | .25 | .25 | .16 | .17 | .22 | .22 | .59 | .55 | .54 | .51 |
| Poland (50; 55) | .41 | .20 | .40 | .23 | .35 | .31 | .36 | .24 | .16 | .10 | .18 | .16 | .64 | .55 | .60 | .65 |
| Romania (36; 35) | .45 | .25 | .40 | .21 | .50 | .52 | .46 | .35 | .20 | .17 | .24 | .14 | .39 | .34 | .52 | .46 |
| Soviet Union (60; 57) | .33 | .21 | .29 | .18 | .36 | .41 | .27 | .21 | .12 | .13 | .10 | .10 | .43 | .42 | .42 | .40 |
| Total COMECON (277; 296) | .36 | .21 | .33 | .19 | .34 | .36 | .28 | .24 | .15 | .12 | .16 | .12 | .48 | .44 | .52 | .46 |

^a A complete table for all OECD countries is available on request from the author.

^b Figures in parentheses give the number of individual import and export flows for each country.

^c The totals include, among others, such countries as Australia, Finland, Israel, New Zealand, and South Africa, which do not belong to either OECD, COMECON or the Underdeveloped group.

^d For a listing of "underdeveloped countries" see [26, pp. 107-38].

^e M: median value of stability indicators.

W: aggregate stability indicators (see footnote 3).

Sources: OECD [7, pp. 152-82] [22, pp. 58-145] [40, pp. 38-150] [41, pp. 82-208] [42, pp. 54-178] [43, pp. 114-238] [46, Table 1140] [47, Table 1211]; COMECON [2, pp. 399-402] [4, pp. 252-54] [5, pp. 314-15] [9, pp. 258-60] [10, pp. 242-43] [11, pp. 208-9] [12, pp. 223-24] [15, pp. 10-15] [16, pp. 8-12] [17, pp. 11-15] [29, p. 246] [30, p. 570] [31, p. 548] [32, p. 382] [33, pp. 82-83] [34, p. 200] [35, p. 305] [36, p. 307] [48, pp. 363-64] [49, pp. 367-68].

International Monetary Fund, and OECD publications, and in various statistical and foreign trade yearbooks of the COMECON countries (see Sources in Table 1). Fourteen years (1950 to 1963) were used to calculate the stability indicators for most country-pair flows. A smaller number of observations was accepted for some COMECON countries⁶ and for the underdeveloped countries that gained independence recently. In a few instances, a minimum number of six observations was accepted in order to gain more

⁶ For example, the time coverage is 1955 to 1963 for the Soviet Union and 1953 to 1963 for the German Democratic Republic.

TABLE 2—SIGNIFICANCE OF MEASURED DIFFERENCES IN STABILITIES OF IMPORTS AND EXPORTS

| Import from and Export to: | Total | | | | | | Underdeveloped | | | | | |
|----------------------------|----------------|-------------|----------------|--------|-------------|------|----------------|-------------|------|--------|-------------|------|
| | Import | | | Export | | | Import | | | Export | | |
| | N ^a | Me- dian | s ^b | N | Me- dian | s | N | Me- dian | s | N | Me- dian | s |
| United States | 105 | .21 | | 106 | .20 | | 75 | .24 | | 74 | .21 | |
| Soviet Union | 60 | .33 | 2.39 | 57 | .29 | 2.67 | 25 | .43 | 3.52 | 20 | .42 | 3.07 |
| United States | 105 | .21 | | 106 | .20 | | 75 | .24 | | 74 | .21 | |
| OECD | 1,326 | .29 | 1.97 | 1,241 | .26 | 2.87 | 777 | .35 | 2.66 | 549 | .30 | 4.50 |
| United States | 105 | .21 | | 106 | .20 | | 75 | .24 | | 74 | .21 | |
| COMECON | 277 | .36 | 5.37 | 296 | .33 | 5.65 | 81 | .48 | 5.69 | 105 | .52 | 15.7 |
| OECD | 1,326 | .29 | | 1,241 | .26 | | 777 | .35 | | 549 | .30 | |
| COMECON | 277 | .36 | 4.65 | 296 | .33 | 5.91 | 81 | .48 | 5.01 | 105 | .52 | 7.13 |
| Soviet Union | 60 | .33 | | 57 | .29 | | 25 | .43 | | 20 | .42 | |
| COMECON | 277 | .36 | 1.11 | 296 | .33 | 1.46 | 81 | .48 | .90 | 105 | .52 | 1.42 |
| OECD-OECD | 325 | .19 | | 328 | .19 | | | | | | | |
| COMECON-COMECON | 42 | .15 | 1.93 | 42 | .16 | 2.01 | | | | | | |
| OECD-COMECON | 102 | .28 | | 100 | .34 | | | | | | | |
| COMECON-OECD | 100 | .34 | 3.24 | 97 | .28 | 3.09 | | | | | | |
| United States | | | | | | | | | | | | |
| Developed ^c | 31 | .15 | | 31 | .18 | | | | | | | |
| Underdeveloped | 74 | .24 | 3.39 | 75 | .21 | .78 | | | | | | |
| Soviet Union | | | | | | | | | | | | |
| Developed ^c | 35 | .29 | | 33 | .22 | | | | | | | |
| Underdeveloped | 25 | .43 | 1.96 | 24 | .42 | 2.76 | | | | | | |
| OECD | | | | | | | | | | | | |
| Developed ^c | 575 | .23 | | 473 | .23 | | | | | | | |
| Underdeveloped | 751 | .35 | 10.2 | 767 | .30 | 7.18 | | | | | | |
| COMECON | | | | | | | | | | | | |
| Developed ^c | 206 | .31 | | 193 | .27 | | | | | | | |
| Underdeveloped | 71 | .48 | 3.22 | 103 | .52 | 7.47 | | | | | | |

^a Number of individual imports or exports flows.^b The Mann-Whitney *U*-test:

$$z = \frac{U - \mu_U}{\sigma_U}, \quad U = n_1 n_2 + \frac{n_1(n_1 + 1)}{2} - R_1,$$

where n_1 and n_2 are the sample sizes ($n_1 > 8$, $n_2 > 8$) and the R_1 is the sum of ranks assigned to the group whose sample size is n_1 ;

$$\mu_U = \frac{n_1 n_2}{2}, \quad \sigma_U = \sqrt{\frac{n_1 n_2 (n_1 + n_2 + 1)}{12}}.$$

The null-hypothesis that the two countries (or one country and a group of countries, or two groups of countries) are equally stable should be rejected for two-tail probabilities at 5 per cent level if $z > 1.96$ (see [28, pp. 116–27]).

^c Total trade less trade with underdeveloped countries.

Source: Table 1.

comprehensive country coverage. Some minor adjustments of the basic figures were necessary to obtain continuous series. These consisted either of filling-in missing observations between two years by linear interpolation, or linking-in data from a different source at the beginning or the end of a continuous series. Such adjustments were few compared to the total number of observations and could not distort the empirical results. The reliability, comparability, and accuracy of figures obtained from official publications

were not subjected to separate scrutiny.⁷ The uncertainties connected with recording basic international trade data are well known and do not have to be reiterated here [19, pp. 137-80]. In defence of our findings, two facts should be pointed out: first, we are interested in comparing trade stability of countries, or blocs, not in the absolute stability levels; and second, the stability indicators are measured as deviations from trends. These considerations reduce both the importance of exact and comparable definitions of "imports" and "exports" and the importance of errors in basic data. If the errors of observation in the time series are consistent, that is, the figures are always too high or too low, they will not significantly affect the trend and the variations around it. And if these errors lead to consistent overestimation (or underestimation) of stability for all countries, the influence of such errors will tend to cancel out in the intercountry or interbloc comparisons of stability levels. Only in the case of errors in basic data leading to overestimation of stability of one country or bloc and underestimation in the other will the comparison be seriously distorted. There is no a priori reason to believe that such should be the case, but it is of course not possible to prove that such biases do not exist. However, most of our conclusions are not sensitive to substantial errors in stability indicators; they would hold even if we allowed for significant errors in basic observations.

Second, the empirical results which demonstrate differences in relative import and export stabilities can be tested for statistical significance. The Mann-Whitney nonparametric U-test, which is based on sum of ranks and does not assume normality, homoscedasticity, or exactness of observations, was used.⁸ The results of the tests are presented in Table 2. In most cases considered, the null-hypothesis that the trade flows of the countries or of the blocs compared were equally stable should be rejected.⁹

III. DETERMINANTS OF STABILITY PATTERNS

The relationship between the stability of export totals and various explanatory variables was investigated recently by J. D. Coppock [1]. We attempted to carry the analysis further and to explain stability of individual import and export flows of each country with its many partners. The possible explanations of these instability patterns are rather complex. They would run in terms of divergent price and income elasticities of imports and exports, growth and stability of incomes of the importing countries, the commodity composition of trade, the share of imports and exports in domestic consumption and output, commercial policies, as well as sporadic events and political preferences. We attempted to establish for each

⁷ One general comment should perhaps be made on the comparability of statistics for the two blocs. The OECD statistics are in current dollars, the COMECON statistics are expressed in the "foreign trade" currencies of the individual countries reflecting world market prices; the latter approach constant prices since they are adjusted only from time to time (see, e.g., [51, p. 71] [3, p. 59]). Since fluctuations in value tend to be larger than fluctuations in volume (see, e.g., [1, p. 4] [38, p. 39]) the use of "foreign trade" currency units for the COMECON bloc countries tends to underestimate their instability.

⁸ The test does require independence of observations. It is likely that the individual country-pair indicators of stability are not completely independent.

⁹ The parametric *t*-test yielded similar results.

country the relationship between the stability of expenditures on imports from its many trading partners and the following explanatory variables where data were available: the volume of imports; the rate of growth of imports; and the level of income of the exporting country as a surrogate of the commodity composition of imports which could influence stability.¹⁰ Similarly, we attempted to explain the stability of exports by the following: the volume of exports; the rate of growth of exports; the growth of income in the importing country; the stability of income of the importing country; and the level of income of the importing country. In addition, two classificatory variables were introduced for both import and export regressions, one indicating membership in the OECD bloc and the second indicating membership in the COMECON bloc. The regression equations took the following form for each country

$$\text{Import: } S_j = a_1 + b_1V_j + b_2G_j + b_3Y_i^L + b_4C_0 + b_5C_e$$

$$\text{Export: } S_i = a_2 + c_1V_i + c_2G_i + c_3Y_j^G + c_4Y_j^S + c_4Y_j^L + c_5C_0 + c_6C_e$$

where S stands for the stability indicator for individual imports or exports flows; V stands for the average volume and G for the rate of growth in volume of imports or exports; Y^S for stability of income, Y^G for growth of income and Y^L the level of income, with subscripts j and i for the importing and the exporting countries, respectively. C_0 and C_e are the classificatory variables for the OECD and the COMECON respectively ($b_4=1$ and $b_5=0$ for the OECD, and $b_4=0$ and $b_5=1$ for the COMECON; likewise for c_5 and c_6).

Unfortunately, the results were not satisfactory for either import or export regressions. The modest results obtained are undoubtedly due both to the omission of important explanatory variables for which no data are available, such as price and income elasticities, and to the rough nature of the data used in the regressions. The regression coefficients for import stabilities in a few countries are given in Table 3. Two numerical variables, the volume and the growth of imports,¹¹ and two classificatory variables explain at most only one-half of the stability of imports of the individual countries. The attempt to explain the export stabilities was no more successful. The most important explanatory variables were the two classificatory variables. In a stepwise regression, they appeared first in the majority of cases and their coefficients were significant in practically all cases. In the absence of additional numerical variables, membership in the two blocs appears to be the deciding factor in import and export stabilities for either the OECD or the COMECON bloc countries.

IV. *Concluding Remarks*

Empirical results of this study shed new light on the trade stability patterns of the two blocs. The economic performance with respect to the trade stability of the OECD in general and the United States in particular was

¹⁰ We assumed here that countries with low per capita incomes typically export primary products.

¹¹ The level of income of the exporting country was dropped since none of the regression coefficients was significant.

TABLE 3—REGRESSION COEFFICIENTS RELATING IMPORT STABILITY TO TRADE VOLUME AND GROWTH, SELECTED COUNTRIES

| Country | Degrees of Freedom | Constant Term | Coefficients of ^a | | | | R ² |
|----------------|--------------------|---------------|------------------------------|-------------------|--------------------------|-------------------|----------------|
| | | | Volume ×10 ³ | Growth | Classificatory Variables | | |
| | | | | | for OECD | for COMECON | |
| Belgium | 56 | -.10 | -.0042 (.0026) | .4791 (.2567) | -.2031 (.0574) | -.1933 (.0753) | .36 |
| France | 55 | .22 | -.0520 (.0200) | .0820 (.0344) | -.0872 (.0494) | .1799 (.1699) | .32 |
| Germany (F.R.) | 55 | .22 | -.0008 (.0016) | .4727 (.8386) | -.1525 (.0519) | -.1839 (.0696) | .49 |
| United Kingdom | 60 | .16 | -.0029 (.0015) | .1755 (.1095) | -.1248 (.0534) | -.1316 (.0788) | .27 |
| Soviet Union | 31 | .62 | -.0035 (.0058) | -.1145 (.2362) | -.0313 (.0957) | -.2213 (.2302) | .20 |

^a Numbers in brackets are standard errors.

Sources: Table 1 [39, pp. 321-24] [44, p. 483] [45, pp. 527-28].

surprisingly good when compared to the Soviet Union and the COMECON countries. There is little factual basis for the charge frequently levied that the foreign trade of the free market countries of the OECD was less stable than that of integrally planned economies of the COMECON bloc; the opposite is closer to the truth. While the trade of the OECD countries with underdeveloped countries was more stable than that of the COMECON, both blocs exhibited greater instability in their trade with underdeveloped countries than in their remaining trade. Membership in the two blocs appears important in explaining the trade stability patterns.

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A Note on Tariffs and Subsidies

Assume circumstances in which optimum, from the viewpoint of a single country, would be achieved by free trade. The government, however, decides to encourage an import-substituting industry to expand to a given extent. Granting this decision, the question is whether the second-best solution is the encouragement ("protection") by a tariff on competing imports or by a subsidy to home producers in the industry.

There seems to be an agreement that, disregarding fiscal aspects, subsidies have an advantage. This is based mostly on an intuitive argument

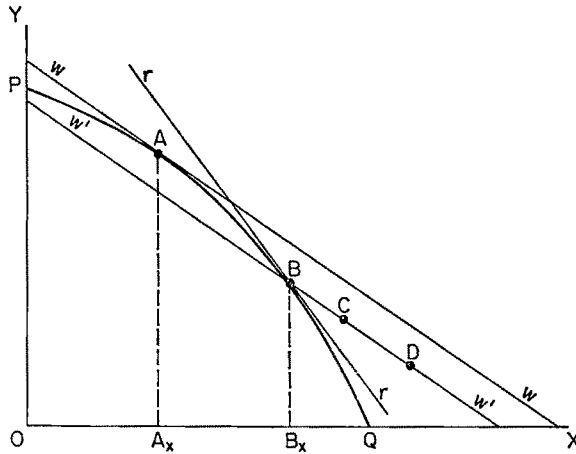


FIGURE 1

that while both tariffs and subsidies distort the production pattern, tariffs alone disrupt also the consumption pattern. The only rigorous proof of the argument, of which I am aware, has been provided by Corden.¹ The proof rests, however, on a highly restrictive assumption, which diminishes drastically its validity: "The internal distribution of income is unaffected by any changes in the model, and the device of community indifference curves can be employed" [2, p. 235]. In effect, the superiority of subsidies may be easily demonstrated without resort to this device.

The analysis will adopt the familiar model of two goods, incomplete specialization, perfect competition, perfect mobility of goods (save the contemplated interference by the government), and lack of any other conceivable distortions (such as external economies). It will also be assumed that world prices are given.²

In Figure 1, PQ is the transformation ("production-possibility") curve between goods X and Y . The slope of ww represents the transformation ratio between the two goods in the world market. With free trade, the economy's production locus will be at A ; while the consumption basket will be on ww , somewhere to the right of A —assuming that the country exports Y and imports X .

The government now decides to expand, by tariff protection, the production of X from OA_x to OB_x . This will be done by the imposition of a tariff on X which will make the domestic market-transformation ratio equal to the

¹ In [2]. See also [1] for a discussion of tariffs vs. subsidies in instances in which the economy suffers from domestic distortions.

² Without this assumption, free trade is not the optimal solution (for a single country). In other words, the choice between tariffs and subsidies will have to take into account the difference between the two in their terms-of-trade effect, from which the present analysis will abstract. See [2].

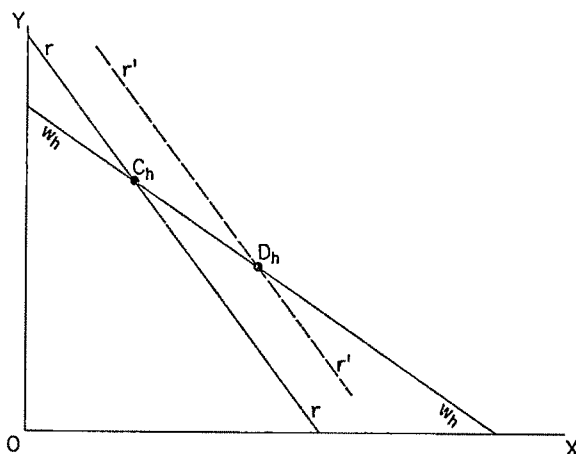


FIGURE 2

slope of rr , rather than ww . The new production locus is B . The new consumption-possibility curve, or budget restraint, is therefore $w'w'$ (having the same slope as ww). The new consumption basket, assuming that the tariff is not prohibitive, will be somewhere to the right of B —say, at C .

Now suppose that the government replaces the tariff by a subsidy of an equal rate. Suppose also that, by lump-sum transfers, everybody's income remains what it was with the tariff. This assumption leaves the income distribution unaffected by the method of (net) tax collection. The important feature of the model is, however, that a redistribution of income induced by changes in relative prices is *not* ruled out by assumption.

The domestic market-transformation line for producers remains rr ; the production locus will remain at B , and the budget-restraint line will remain $w'w'$. The substitution ratio for home consumers will, however, change. X now becomes cheaper, relative to Y . The consumption combination will thus move to the right of C —say, to D . It may now be easily shown that D is superior to C .

Figure 2 presents an individual (home) consumer, H . With the tariff, and the consumption combination for the economy of C (in Figure 1), the consumption basket of this individual is C_h . This may be viewed as his initial endowment of goods. With the shift to a subsidy, H 's budget-restraint line becomes $w_h w_h$. Assuming that the consumer reacts to relative-price changes, his consumption basket will move from C_h to D_h ³ (a move to the left of C_h would be irrational, while C_h itself is a limiting case). But since C_h is still an open possibility, D_h must be superior to it. And since no special properties have been attributed to individual H , the same conclusion must be valid for any other consumer in the economy. The subsidy position is thus superior

³ This assumption is implied in Samuelson's "revealed-preference" demonstration that an income-compensated demand curve must be negatively sloped. See [3].

to the tariff position for each and any consumer;⁴ and no attempt to offset consumer losses and gains is required in order to reach the conclusion that, for the economy as a whole, D is superior to C . Hence, subsidy is preferable to tariff.

If, on the other hand, a change in income distribution due to the form of (net) tax collection is not to be abstracted from by assumption, this proof may be understood as if it involved the use of the compensation principle. That is: it demonstrates that subsidies lead to a potential increase of welfare, since some distribution of income exists in which everybody is made better off than with tariffs (at the actual tariff-ridden income distribution).

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⁴ It should be noted that the reverse exercise would not be legitimate. Take consumer H at the subsidy position D_h . Suppose, now, that the subsidy is replaced by a tariff. It would not be possible to claim that with the initial endowment D_h , the consumer should move leftward on the budget-restraint line $r'r'$ to a superior combination, for the simple reason that $r'r'$ could *not* be a budget restraint for all the economy's consumers. The only uniform ratio at which all the consumers could sell X for Y (and vice versa) is the one represented by $w_h w_h$ (or ww), that is, the transformation ratio in the world market. Each consumer will now exchange his X (by buying less of it) for Y in the world market, at the ratio indicated by $w_h w_h$ (the difference between it and the transformation ratio as he sees it being the saving of the tariff duty); and this will proceed until point C_h is reached, in which the consumer's subjective transformation ratio is equal to rr , his market-transformation rate. But C_h , as has been seen, is inferior to D_h .

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"Price" vs. "Tariff" Elasticities In International Trade— A Suggested Reconciliation

Estimates of elasticities in international trade [4] have been derived by a variety of methods, one of which employs the effect of tariff changes on the volume of trade to get import-demand elasticities. A puzzling phenomenon related to this method is that the elasticities so obtained have been consistently higher—and by a wide margin—than those derived from a direct price-quantity relationship. Thus an implied "tariff elasticity" of -4.3 has been estimated for the United Kingdom for the early 1930s [8, pp. 168-69]; of -9 for Germany for the mid 1950s [9]; and of -6 for the United States

for 1955-59 [7]. In a more direct comparison Krause found the U.S. elasticity of demand for imports calculated with respect to the tariff change to be 2-3 times the elasticity calculated with respect to price [6]. This note suggests that the reason for the disparity might lie, at least partly,¹ in the difference between nominal and effective tariff rates.

While the nominal tariff refers to the rate published in the country's tariff schedule, the effective protective rate is defined as "the percentage increase in value added per unit in an economic activity which is made possible by the tariff structure relative to the situation in the absence of tariffs but with the same exchange rate" [5, p. 222]. In order to estimate the protective effect of the tariff, the rate applied to each product must be related to value added rather than to the gross value of output, allowance being made for tariffs on imported materials. For while nominal tariffs apply to the total value of imports, they protect only the portion of that value produced in the home country. At the same time, the level of protection accorded the production of a final good is also affected by the level of duty on imported inputs used in the production process.

In a simple case where commodity (j) is subject to a nominal import duty (T_j), and uses one imported input (i) on which the tariff is (T_i), the value added per unit of j in the absence of a tariff is:

$$v_j = p_j(1 - a_{ij})$$

and the value added per unit of j made possible by the tariff structure is:

$$v'_j = p_j[(1 + T_j) - a_{ij}(1 + T_i)]$$

where p_j is the price of a unit of j in the absence of tariffs and a_{ij} is the share of i in the cost of j in the absence of tariffs. The effective protective rate is:²

$$(1) \quad G_j \equiv \frac{v'_j - v_j}{v_j} = \frac{p_j[(1 + T_j) - a_{ij}(1 + T_i)] - (1 - a_{ij})p_j}{p_j(1 - a_{ij})} = \frac{T_j - a_{ij}T_i}{1 - a_{ij}}$$

Industrial countries levy higher tariffs on finished manufactures than on raw materials and intermediate goods. Correspondingly the effective rates on manufactures are considerably higher than their nominal counterparts. Indeed, for several countries or country-groups (United States, United Kingdom, European Economic Community, Sweden, and Japan) the observed differences between the two rates are often in the 50-80 per cent range [1 and 3].

In the course of successive GATT negotiations during the postwar period, tariff reductions were concentrated mainly in manufactured products (i.e., categories 5-8 SITC). Partly because finished goods were initially subject to higher nominal rates than products in earlier stages of fabrication, and partly because of the industrial countries' protective attitudes towards certain primary materials, tariffs on finished products were probably sub-

¹ For another possible reconciliation see [2].

² For the development, discussion and extension of this formula see [5].

ject to larger cuts than those on semifinished and primary commodities. In terms of the above notations: $\Delta T_j > \Delta T_i$.

How would this affect the effective protective rate? Assuming no change in a_{ij} , the "total differential" of G_j will yield the change in effective protection for any given changes in T_j and T_i :

$$(2) \quad \Delta G_j \sim \frac{1}{1 - a_{ij}} \Delta T_j - \frac{a_{ij}}{1 - a_{ij}} \Delta T_i.$$

Divide through by ΔT_j

$$\frac{\Delta G_j}{\Delta T_j} = \frac{1}{1 - a_{ij}} - \frac{a_{ij} \cdot \Delta T_i}{(1 - a_{ij}) \Delta T_j} = \frac{1}{1 - a_{ij}} \left(1 - \frac{a_{ij} \cdot \Delta T_i}{\Delta T_j} \right)$$

From this we have:

$$\frac{\Delta G_j}{\Delta T_j} \geq 1 \text{ if: } \frac{1}{1 - a_{ij}} \left(1 - \frac{a_{ij} \Delta T_i}{\Delta T_j} \right) \geq 1; \text{ or } 1 - \frac{a_{ij} \cdot \Delta T_i}{\Delta T_j} \geq 1 - a_{ij}.$$

We conclude that:

$$(3) \quad \frac{\Delta G_j}{\Delta T_j} \geq 1 \text{ if } \frac{\Delta T_j}{\Delta T_i} \geq 1.$$

In words, the change in the effective tariff on the final commodity will exceed or fall short of the change in the nominal rate applied to it, depending on whether or not the change in the nominal tariff on the final good exceeds the change in duty imposed on the imported inputs.³ Also, in order for the effective rate to remain unchanged, we must have

$$(4) \quad \frac{1}{1 - a_{ij}} \Delta T_j = \frac{a_{ij}}{1 - a_{ij}} \cdot \Delta T_i; \text{ or } \frac{\Delta T_j}{\Delta T_i} = a_{ij}.$$

Our hypothesis suggests that in most cases the changes in nominal tariff rates on finished goods exceeded those on primary and semimanufactured commodities. This is subject to empirical verification. But if true, it implies that postwar changes in effective rates exceeded the changes in nominal rates. And it is the effective rather than the nominal rate that determines the "production effect" of the tariff.

When import-demand elasticities are estimated by relating *nominal* tariff changes to changes in the volume of trade, the implied variation in relative prices is smaller than what has actually occurred. Thus, observed changes in quantity represent responses to larger price changes than are superficially apparent, and the resulting demand elasticities would be higher than elasticities obtained by other methods. Conversely, it should be recognized that when elasticities obtained by other means (i.e., without

³ In this formulation, the changes in duty are expressed in terms of percentage points rather than as percentages of the previous rates.

reference to tariff changes) are employed along with nominal tariffs, to estimate the effect of tariff policies (such as multi-lateral reductions or a free trade area in manufactures) on trade flows and welfare, they are likely to underestimate these effects. Only for an across-the-board change which applies to all commodities, will the results be conceptually accurate.

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A Note on Exchange Rate Changes and Capital Flows

The purpose of this note is fourfold: (a) To analyze, under fixed and floating exchange rate systems, the behavior of the forward foreign exchange market in response to two exogenous disturbances—(1) a change in expectations about the future spot rate and (2) a change in the short-term interest differential. (b) To show that under certain conditions the "two interest rate" policy of Keynes [2] will be a failure. (c) To show again (see [3]) that even in flexible exchange rate countries domestic monetary policy is, under certain conditions, not independent of international considerations. (d) To suggest the relevance of my conclusions to the problem of empirically testing the interest rate parity theory (IRPT).

The IRPT is assumed to hold in the long run, although not necessarily in the short run. Only one maturity shall be considered (say three months).

The domestic (say U.S.) and foreign interest rates for that period are r^d and r^f . Exchange rates are quoted as the domestic price of foreign currency: S =spot rate; F =market forward rate; F^* =forward rate satisfying the IRPT, i.e.,

$$F^* = S \left[\frac{1 + r^d}{1 + r^f} \right];$$

and S^e =spot rate expected by speculators (nonhedgers). Assume that r^d is given to the foreign countries either because of the relatively large size of the United States or by U.S. monetary policy, and assume that r^f is set by the monetary authorities. Consider an initial position where $S^e = F = F^* = S$, i.e., the IRPT is satisfied and interest rates are coincident ($r^d = r^f$).

Now suppose that expectations change so that $S^e < F = F^* = S$. What will happen in the fixed exchange rate case (say the U.K.)? Speculators will sell forward pounds at F in anticipation of covering at S^e . Arbitragers (hedgers) will buy these forward pounds and cover by selling spot at the pegged rate, S . As a result, F is forced down so that $S^e \leq F < F^* = S$. The capital flow will be severe as long as the authorities support the pound at S and do not change $r^d - r^f$. If the peg is to be maintained r^f must rise—a deflationary policy must be followed—so that $S^e = F = F^* < S$. The foregoing description is a capsule summary of recent (and not so recent) British exchange rate history. British monetary policy has often and to a large extent been determined by international considerations.

In a floating rate system (say Canada, 1950–62) no domestic monetary adjustments need be made. Speculation against the Canadian dollar will merely lower the spot rate as well as F so that $S^e = F^* = F = S$ once again. If everyone's expectation changed in the same way, no capital flow is required and adjustment is rapid. For this reason the domestic economies of flexible exchange rate countries are said to be insulated from the vagaries of international trade and payments.

Consider now the other possible exogenous disturbance, a change in ($r^d - r^f$). Assume r^d rises. Since interest rates are now higher in the United States than in the United Kingdom, spot funds will flow out of the United Kingdom; and they will be covered forward at F so that F is driven up until $S^e = S < F = F^*$. The United Kingdom will be following a "two interest rate" policy first advocated by Keynes—a low rate at home to maintain aggregate demand and a high rate vis à vis foreigners (since they gain on the forward hedge when $F > S$, i.e., $r^d \approx r^f + (F - S)/S$) to maintain balance of payments. But because the spot rate cannot change and no reason exists for expectations about future spot rates to change, such a policy cannot be maintained in the long run. As speculators become more confident of the "two interest rate" policy, which implies that the subsequent spot rate is less than today's forward rate, they will sell forward sterling in increasing amounts causing a massive capital outflow which must result in devaluation or a policy of domestic inflation. Thus in response to an exogenous shift in expectations a change in the interest differential is *required*—the "two

interest rate" policy is forced on the United Kingdom. But when the United Kingdom would like, independently, to undertake such a policy, it cannot be successful.

Finally consider the response to a rise in r^d under a floating rate. The initial result will differ. Since S^e is unchanged, the burden of the initial adjustment will be on S , which in this case can move, and not on F so that $F = F^* = S^e > S$. But such a position cannot be an equilibrium position for long. If speculators expect the Canadian monetary policy to be a permanent one, they will be able to profit by selling forward exchange and covering at maturity. Such actions will cause a once and for all capital outflow and a moving equilibrium position in which ${}_{t+i}F^*_t = {}_{t+i}F_t > S_t > {}_{t+i}S_t^e = S_{t+i}$, where all rates are depreciating over time, where ${}_{t+i}S_t^e$ accurately predicts the subsequent spot rate, S_{t+i} , and where the forward rate is rather mechanically tied to the spot rate by the IRPT. Note that there is not a continuing capital outflow as in a fixed rate system. Instead the adjustment is in a steady depreciation of the exchange rate. The decline in S means, however, that Canadian exports will increase, i.e., aggregate demand will increase, and inflationary pressure will result as in a fixed exchange country unless the monetary authorities allow a rise in r^f . Thus even under a floating rate, domestic monetary policy is not independent of international disturbances. The Canadian situation in the late 1950s and early 1960s was actually the reverse of the illustration just presented. The United States in 1959 went into a recession causing r^d to fall relative to r^f . An appreciation of the exchange rate resulted and Canada also experienced a recession because the Canadian authorities refused to allow r^f to fall.

Consider now the validity of regressions to test the traditional IRPT. The IRPT states that the relation of F to S is determined solely by the interest differential

$$\left[\text{i.e., } F/S = 1 + R, \text{ where } R = \frac{r^d - r^f}{1 + r^f} \right].$$

A more sophisticated modern theory [4] would maintain that speculative expectations are also important, at least in the short run. If this is so and if expectations are omitted from the tests of the IRPT, there will be a misspecification bias in the coefficient of R which will depend on the regression between R and the omitted variable (see [1]). Since F/S is being predicted the appropriate variable (at least conceptually) to represent expectations is S^e/S . Part of the importance of the earlier analysis is that it will permit a prediction of the direction of bias. In the United Kingdom, the situation described in paragraph 3 above has been typical. Such a situation implies a positive correlation between R and S^e/S (when S^e declined, r^f was forced to increase and R also declined), and this in turn implies an upward bias in the coefficient of R . The typical Canadian situation, described above implies a negative correlation between S^e/S and R (when r^d declined and R decreased, S^e increased more than the current S because speculators expected continued tight money in Canada and thus a continued appreciation of

subsequent spot rates); and this negative correlation, in turn, implies a downward bias in the observed coefficient of the interest differential. For example I ran, using weekly data for the United States-Canada and the United States-United Kingdom, regressions on the traditional IRPT for both Canada and the United Kingdom that resulted in coefficients of .80 for the interest differential, R . Inclusion of a missing expectations variable changed Canada's coefficient to .96 (much closer to the theoretical expectation of 1.0) and changed the United Kingdom's coefficient to .60 (much farther from the theoretical expectation of 1.0).

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Wages and the Guideposts

Economists seem quite unsure of whether the guideposts are working and are divided on whether they like them. Among those who don't, a further division exists between those who don't like them because they don't work and those who don't like them because they do. In any case, some facts would help everyone decide on which grounds to oppose or defend the guideposts, the facts, of course, being incapable of actually changing anyone's basic position for or against them.

One casual argument that is made is that the guideposts are no longer a factor, witness the various wage increases that have shattered them. I am prepared to argue from casual evidence, and shortly will. But some settlements, or even all settlements, being made above the Administration's central figure of 3.2 per cent show nothing. The interesting question is whether wage (and price) changes have been smaller than they would have been without the guideposts.

To test this, one needs an estimate of what they would have been; an investment in one method of making such an estimate [1] led me to look at this question. The method is to use an equation for estimating wage changes (straight time hourly earnings of production workers) in manufacturing based on postwar quarterly data. For the 1947-60 period, the equation estimated was [1, p. 50]

$$\dot{W}_t = -4.313 + 0.367\dot{C}_{t-1} + 14.711U_t^{-1} + 0.424R_{t-1} + 0.792\Delta R_t, \quad R^2 = .88,$$

(0.054) (2.188) (0.068) (0.176)

where

- \dot{W} is the percentage change in straight time hourly earnings over the past year
 \dot{C} is the percentage change in the Consumer Price Index over the year
 U^{-1} is the reciprocal of the percentage unemployment rate over the year
 R is the average profit rate in manufacturing over the year (after tax profits as a percentage of equity)
 ΔR is the quarterly first difference in R .

For the period starting with the first quarter of 1962, the differences between actual and estimated values of \dot{W} are given in the left column of Table 1 below. The right column gives the same differences based on a similar equation covering only the quarters from 1953 to 1960.¹

The pattern of forecast errors coincides so beautifully with guideposts that the burden of proof regarding their effectiveness would, at least, seem to have shifted a bit. A dummy variable would clearly be significant, more so if it reflected the increasing urgency with which the Administration has embraced guideposts as the years passed and the unemployment rate fell. The historical residuals from the wage equation give no basis for expecting this result. While residuals show some autocorrelation,² during the 1948 to 1960 period the longest run of residuals with the same sign was 6 quarters compared with the 15 successive negative quarters shown in Table 1. Furthermore, there was no suggestion of a negative trend in the wage change variable; if anything, the contrary might have been suspected. Before the run shown in Table 1, the equation had underpredicted wage changes most of the time since 1958 [1, p. 77]. The possibility of a longer trend was checked by using the equation for 1953-1960 to predict the wage changes of the preceding years, 1948 to 1953.³ The equation overpredicted wage changes by an average of 0.84 percentage points [1, p. 75]; underprediction would have been expected if the present results for recent years were the result of a long-term trend.

I find these results highly suggestive. But any good skeptic could suggest plausible reasons other than guideposts for them too. So I have looked for some further evidence from other data.

The best approach is suggested by the criticism that the guideposts are (necessarily) enforced unevenly, some industries and bargains being specially subject to their pressure because they are highly visible. Visibility enters in both because it is infeasible to police every small wage settlement

¹ In this case the equation was

$$\dot{W}_t = -4.712 + 0.680\dot{C}_{t-1} + 18.421U_t^{-1} + 0.360R_{t-1} + 1.244\Delta R_t, \quad R^2 = 0.80.$$

(0.132) (3.050) (0.120) (0.300)

² The Durbin-Watson statistic is 1.2 for the wage equation shown.

³ Such a check was suggested by the referee of this paper.

TABLE 1—ACTUAL MINUS ESTIMATED PERCENTAGE WAGE RATE CHANGES

| Year-Quarter | From 1947-60 Equation | From 1953-60 Equation |
|--------------|-----------------------|-----------------------|
| 1962-1 | 0.84 | 0.74 |
| -2 | 0.07 | 0.08 |
| -3 | -0.52 | -0.59 |
| -4 | -0.71 | -0.71 |
| 1963-1 | -0.97 | -0.96 |
| -2 | -0.37 | -0.48 |
| -3 | -0.19 | -0.22 |
| -4 | -0.18 | -0.31 |
| 1964-1 | -0.27 | -0.53 |
| -2 | -0.77 | -0.97 |
| -3 | -0.73 | -0.95 |
| -4 | -1.72 | -1.77 |
| 1965-1 | -1.68 | -1.82 |
| -2 | -1.63 | -1.75 |
| -3 | -2.11 | -2.35 |
| -4 | -1.61 | -1.88 |
| 1966-1 | -2.48 | -2.79 |

and because bad pattern-setting effects may be feared from visible bargains, making them even more important than their size alone would indicate. A separation into visible and invisible settlements would thus permit a rather strong test of the guideposts.

The first requirement is to separate the industries according to their visibility or susceptibility to guidepost pressures. An objective measure would be nice to have, but I did not find one. Instead, I consulted some experts who presumably have a learned opinion and asked them to separate the two-digit manufacturing industries into two groups. Where my experts disagreed or were uncertain, the industry was left out of both groups.

The next question was how to compare wage behavior in the two groups. Ideally, one would like an equation for each industry like the one above for all manufacturing. Then the pattern of recent residuals from the different industries could be compared. For various reasons, it is not as easy to get such wage equations with disaggregated data, the main problem being the irregular timing of wage changes. So I have resorted to a more casual test. I have simply taken the ratio of wage changes before guideposts to wage changes after guideposts for each industry. If the guideposts are having an impact, the ratio should be higher for the visible industries.

In order to minimize other effects, I chose periods for the ratio of wage changes that were as similar as our economic performance would allow: 1963 to 1966 and 1954 to 1957. Letting W stand for average hourly earnings of production workers and R^W stand for the ratio just described, we have

$$R^W = \left(\frac{W_t}{W_{t-1}} \right)_{1960s} \div \left(\frac{W_t}{W_{t-1}} \right)_{1950s}.$$

R_i^W stands for an individual industry's ratio; R_V^W for the mean ratio of in-

TABLE 2— R^W : RATIOS OF WAGE CHANGES IN THE 1950s TO WAGE CHANGES IN THE 1960s

| | Periods Used for Ratios ^a | | |
|---|--------------------------------------|--------------------|--------------------|
| | 1954-57 1963-66 | 1954-56 1964-66 | 1954-55 1965-66 |
| Industry | | | |
| Visible Group | | | |
| Ordnance and Accessories | 1.029 | 1.025 | 1.017 |
| Primary Metals | 1.034 | 1.034 | 0.038 |
| Fabricated Metal Products | 1.016 | 1.009 | 1.003 |
| Machinery | 1.010 | 1.011 | 0.992 |
| Electrical Equipment | 1.021 | 1.018 | 1.005 |
| Transportation Equipment | 1.008 | 1.005 | 1.012 |
| Chemicals and Allied Products | 1.020 | 1.019 | 1.010 |
| Petroleum and Related Products | 1.022 | 1.015 | 0.989 |
| Rubber and Plastic Products | 1.018 | 1.021 | 1.035 |
| Mean | 1.020 | 1.017 | 1.011 |
| Invisible Group | | | |
| Lumber and Wood Products | 0.999 | 1.001 | 0.984 |
| Furniture and Fixtures | 1.005 | 1.003 | 0.991 |
| Stone, Clay and Glass Products | 1.015 | 1.013 | 1.007 |
| Tobacco Manufacturers | 1.009 | 0.999 | 0.992 |
| Textile Mill Products | 0.987 | 0.981 | 0.965 |
| Paper and Allied Products | 1.015 | 1.013 | 1.002 |
| Leather and Leather Products | 1.006 | 1.013 | 0.993 |
| Mean | 1.005 | 1.003 | 0.991 |
| t statistic for difference between observed means significant at: **.05 level ***.01 level | 3.23*** | 2.79** | 2.61** |

^a The change from 1965 to 1966 was approximated by the change between the April-May average for each year.

dustries in the visible group; and R_I^W for the mean ratio of industries in the invisible group. The W_t/W_{t-1} terms were computed for different intervals within the basic time periods chosen, and there are thus R^W statistics based on the 1954 to 1955 wage change divided by the 1965 to 1966 wage change; the average wage change from 1954 to 1956 divided by the average wage change from 1964 to 1966; and the average wage change from 1954 to 1957 divided by the average wage change from 1963 to 1966. Again, because of the irregular timing of wage changes, the last of these, involving the ratio of average wage changes over three-year periods, was expected to give the sharpest comparisons.

Table 2 lists the industries in the visible and invisible groups together with the individual industry and group wage change ratios. With few excep-

tions, the individual R_i^W 's are higher for the visible industries. The indicated difference between R_V^W and R_I^W , the mean ratio for each group, is statistically significant at the one per cent level for the ratios using three-year wage changes and at the 5 per cent level for the others. Thus, when compared with the mid-1950s, wage changes in the mid-1960s have slowed down in the visible industries relative to the invisible ones. Indeed, in invisible industries, the average annual wage change from 1963 to 1966 was 3.8 per cent compared with 4.3 per cent from 1954 to 1957; in visible industries the comparable figures are 2.9 per cent and 5.0 per cent. The differential slowdown is 1.6 percentage points. It is stretching casual empiricism pretty far, but for the period spanned, such a difference between the visible and invisible industries looks quite consistent with the residuals for the aggregate manufacturing equation shown earlier, on the hypothesis that the invisibles were unaffected by guideposts and the visibles affected to the extent shown by the larger R^W ratios in Table 2 (or the 1.6 percentage point differential just cited).

Next, it is possible to clear up some misgivings about what else may be going on to yield these results by looking at employment data in the same way the wage data were examined. Employment changes are an imperfect substitute for detailed analysis of individual industries; but they may serve as a proxy for what one would like to know. Many views of aggregate wage determination, including the one expressed by the equation given earlier in this paper, are consistent with the view that short-run *relative* wage changes among industries can be identified with relative shifts in demand, and hence employment, among these industries. Other things being equal, the R^W ratios shown in Table 2 would be positively related to the corresponding ratios of employment changes. In particular, if higher employment-change ratios were observed for the visible industries, it would mean the invisibles experienced the relatively larger growth in labor demand in the 1960s compared with the 1950s, and this fact could explain what we have observed without recourse to guideposts.

Table 3 shows the employment-change ratios, designated by R^E , computed in the same way as the wage-change ratios of Table 2. Employment refers to production workers. For the three-year spans, the rank correlation between the R_i^W 's and R_i^E 's for individual industries in the invisible groups is 0.864, significant at the 5 per cent level. This supports the basic presumption that, in industries unaffected by guideposts, relative wage changes are positively related to relative employment changes. Within the visible industries subgroup, the rank correlation is only 0.200, an insignificant magnitude and perhaps not unexpected since the basic hypothesis calls for guideposts to interfere with wage changes here. (With ordnance and accessories removed the rank correlation becomes 0.648, significant at the 10 per cent.)

Alongside these results for individual industries, the telling comparison is between the mean wage-change and employment-change ratios for the two industry groups. While R_V^W exceeds R_I^W in each time span shown in Table 2 R_I^E exceeds R_V^E in each time span shown in Table 3. This is more support than the guidepost hypothesis needs: If R_V^E just equaled R_I^E it would sup-

TABLE 3— R^E : RATIOS OF EMPLOYMENT CHANGES IN THE 1950s
TO EMPLOYMENT CHANGE IN THE 1960s

| | Periods Used for Ratios ^a | | |
|---|--------------------------------------|--------------------|--------------------|
| | 1954-57 1963-66 | 1954-56 1964-66 | 1954-55 1965-66 |
| Industry | | | |
| Visible Group | | | |
| Ordnance and Accessories | 0.804 | 0.731 | 0.549 |
| Primary Metals | 0.990 | 1.019 | 1.095 |
| Fabricated Metal Products | 0.960 | 0.961 | 0.974 |
| Machinery | 0.955 | 0.979 | 0.941 |
| Electrical Equipment | 0.936 | 0.916 | 0.877 |
| Transportation | 0.950 | 0.904 | 0.971 |
| Chemical and Allied Products | 0.984 | 0.995 | 1.001 |
| Petroleum and Related Products | 1.012 | 1.002 | 0.968 |
| Rubber and Plastic Products | 0.955 | 0.970 | 1.025 |
| Mean | 0.950 | 0.942 | 0.933 |
| Invisible Group | | | |
| Lumber and Wood Products | 0.965 | 0.982 | 1.012 |
| Furniture and Fixtures | 0.979 | 0.992 | 1.006 |
| Stone, Clay and Glass Products | 0.990 | 1.025 | 1.030 |
| Tobacco Manufacturers | 0.997 | 1.048 | 1.038 |
| Textile Mill Products | 0.953 | 0.958 | 0.965 |
| Paper and Allied Products | 0.998 | 1.000 | 0.988 |
| Leather and Leather Products | 0.986 | 0.981 | 1.000 |
| Mean | 0.981 | 0.998 | 1.006 |
| t statistic for difference between observed means (none significant at .10 level) | -1.33 | -1.61 | -1.22 |

^a The change from 1965 to 1966 was approximated by the change between the April-May average for each year.

port the guidepost explanation offered for the findings with wage changes alone. In fact, the indicated difference between R_V^E and R_I^E is not significant at the 10 per cent level. These results still show that relative employment changes cannot account for the differential in wage behavior that we observe between the visible and invisible industries.

The employment data can be applied to our question somewhat more formally by specifying the following model. Assume the ratio of wage changes in the i th industry to the average wage change in manufacturing is proportional to the ratio of employment change in the i th industry to the average employment change in manufacturing. That is:

$$\frac{W_{t,i}}{W_{t-1,i}} \bigg/ \frac{\bar{W}_t}{\bar{W}_{t-1}} = a_i \left[\frac{E_{t,i}}{E_{t-1,i}} \bigg/ \frac{\bar{E}_t}{\bar{E}_{t-1}} \right]$$

where a bar over a variable now indicates the manufacturing average. If nothing changed this relation between the periods we are considering, we could compute the ratios as before, eliminate the unknown a_i 's, and expect a relation of the form

$$R_i^W - \frac{\bar{R}^W}{\bar{R}^E} R_i^E = 0$$

However under the guideposts hypothesis being tested, for visible industries we would expect

$$R_i^W - \frac{\bar{R}^W}{\bar{R}^E} R_i^E = d_i > 0$$

since \bar{R}^W is increased less by guideposts than R_i^W ; and for invisible industries we would expect

$$R_i^W - \frac{\bar{R}^W}{\bar{R}^E} R_i^E = d_i < 0$$

since \bar{R}^W is increased more than R_i^W . Thus letting d_V be the expected d_i for visibles and d_I the expected d_i for invisibles, we hypothesize $d_V > d_I$ and, in the strongest form of the test, $d_V > 0$ and $d_I < 0$.

Because the total manufacturing ratios, \bar{R}^W and \bar{R}^E , are not merely averages of the R_i^W and R_i^E for the individual industries used here (and because other things may have changed in the relation besides guideposts), the strongest test, requiring both $d_V > 0$ and $d_I < 0$ may well fail for extraneous reasons.⁴ However the weaker test, $d_V > d_I$, should be met if the effectiveness of guideposts can be identified by the differential wage behavior in visible and invisible industries.

Table 4 lists the d_i 's for the individual industries and for the visible and invisible industry subgroups. The hypothesis $d_V > d_I$ is accepted with various levels of confidence for the different subperiods, the one per cent level being reached for the three-year spans if the ordnance and accessories industry is excluded. Guideposts do seem to have slowed wage changes in visible industries relative to invisible industries to a significant degree.

Many questions remain. Accepting all the above results, have guideposts been desirable on balance? Generally we dislike measures that distort relative prices; but what if visible industries in the past distorted wages as markets tightened with the balance ultimately restored at the cost of inflation? And a final caveat. We cannot prove that only guideposts could have caused the wage behavior observed. In particular, my colleague Carlos Diaz suspects that growing import competition may have been as important, an interesting conjecture but hard to test.⁵ On the present evidence, I feel one

⁴ In fact $d_V > 0$ does fail significance tests; $d_I < 0$ is significant at the 1 per cent level using the three-year spans.

⁵ In a study on the guideposts which he is preparing for the Brookings Institution, John Sheahan considers this conjecture and tentatively rejects it.

TABLE 4—*d*: DIFFERENCE BETWEEN PREDICTED AND OBSERVED WAGE-CHANGE RATIOS

| | Periods Used for Ratios | | |
|--|-------------------------|----------------|----------------|
| | 54-57 63-66 | 54-56 64-66 | 54-55 65-66 |
| Industry | | | |
| Visible Group | | | |
| Ordnance and Accessories | 0.1836 | 0.2583 | 0.4452 |
| Primary Metals | -0.0071 | -0.0350 | -0.1020 |
| Fabricated Metal Products | 0.0058 | 0.0004 | -0.0110 |
| Machinery | 0.0052 | -0.0166 | 0.0128 |
| Electrical Equipment | 0.0360 | 0.0564 | 0.0919 |
| Transportation Equipment | 0.0085 | 0.0563 | 0.0008 |
| Chemicals and Allied Products | -0.0146 | -0.0324 | -0.0323 |
| Petroleum and Related Products | -0.0420 | -0.0319 | -0.0192 |
| Rubber and Plastic Products | 0.0140 | 0.0037 | -0.0320 |
| Mean | 0.0210 | 0.0288 | 0.0394 |
| Mean Excluding Ordnance Accessories | 0.0007 | 0.0001 | -0.0114 |
| Invisible Group | | | |
| Lumber and Wood Products | -0.0160 | -0.0293 | -0.0697 |
| Furniture and Fixtures | -0.0249 | -0.0379 | -0.0558 |
| Stone, Clay and Glass Products | -0.0268 | -0.0269 | -0.0655 |
| Tobacco Manufacturers | -0.0400 | -0.1005 | -0.0883 |
| Textile Mill Products | -0.0158 | -0.0242 | -0.0398 |
| Paper and Allied Products | -0.0343 | -0.0357 | -0.0266 |
| Leather and Leather Products | -0.0313 | -0.0167 | -0.0478 |
| Mean | -0.0270 | -0.0439 | -0.0562 |
| <i>t</i> statistic for difference between observed means: | | | |
| all industries: | 1.94** | 1.98** | 1.55* |
| excl. ord. and acces: | 3.00*** | 2.51** | 2.13** |
| significant at: * 10 per cent level | | | |
| ** 5 per cent level | | | |
| *** 1 per cent level | | | |

must now try to disprove the impact of guideposts rather than the other way around.

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* The author is associate professor of economics at the University of Minnesota. This paper was written in part while he enjoyed a month as visiting research professor at the Federal Reserve Bank of Minneapolis, and was completed during his tenure as a Ford Foundation faculty research fellow; neither organization is necessarily in agreement with any of the conclusions or opinions expressed here. His assistant at the University of Minnesota, C. H. Wong, did all the computations. He is grateful to Carlos F. Díaz and John Sheahan, both for criticism and for spotting some errors in an earlier draft.

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Price Elasticity and Linear Supply Curves

There is a lengthy section in almost every price theory text dealing with the behavior of price elasticity along linear demand functions. The point is to show (1) how elasticity changes along the function and (2) how these elasticities are related to total expenditures at alternative prices and quantities. Yet price elasticity behavior along positively sloped linear and other supply curves usually is treated much less fully, presumably because there is no question whether or not a price change increases or decreases sellers' total revenue. Prices and total revenues move together. About the most shown is that linear supply functions (or linear segments of other supply curves) are unit elastic if they pass through the price-quantity origin when extended, elastic if they intersect the price axis, and inelastic if they intersect the quantity axis [2, pp. 190-92] [3, pp. 100-102] [5, pp. 197-200]. However, to complete the story, instructors also might discuss the behavior of supply elasticity and total revenue *along* these linear functions. That is the purpose of this note.

This topic is a matter of some importance since linear supply curves, even though a special case, often are used in the classroom and have been employed in a multitude of empirical supply response studies. For instance, several recent studies used linear functions to estimate production response and supply elasticity for important commodities in less-developed nations [1] [4] [6] [7, pp. 65-82]. In each of these studies, policy implications might have been sharpened had price elasticity behavior along the estimated linear supply curves been considered in conjunction with the published elasticity estimates computed at mean values. It would be unusual if current prices and outputs, important for current policy decisions, coincided with mean values of historical data series. Thus we should be as aware of the analytical consequences in assuming linearity on the supply side as we are on the demand side.

To tackle the question, first consider the linear supply function

$$(1) \quad Q = a + bP \quad (b > 0)$$

where Q is the quantity supplied of some commodity, P is its price, and a and b are intercept and slope parameters respectively.¹ Figure 1 depicts this supply function at three arbitrary levels of a . For S_1 , a is less than zero; for S_2 , a is equal to zero; and for S_3 , a is greater than zero.

Now form the price elasticity of supply from equation (1)

$$(2) \quad Ep = \frac{\partial Q}{\partial P} \cdot \frac{P}{Q} = b \frac{P}{Q}$$

and using equation (1) to eliminate P

¹ The variable, P , may be some previous price, a price ratio, or a complex price expectation. In this context, it is regarded simply as the appropriate supply-inducing price variable. The intercept a can be viewed as including other supply "shifters" fixed at some level.

$$(3) \quad Ep = 1 - \frac{a}{Q}$$

Figure 2 shows how Ep varies along each of Figure 1's three supply curves: S_1^* shows elasticities along S_1 ; S_2^* is associated with S_2 ; and S_3^* is associated with S_3 . Along S_1^* , supply elasticity is infinitely large when Q is zero and then approaches $+1.0$ from above as Q increases. Along S_2^* , elasticity is always $+1.0$. Along S_3^* , elasticity is zero when Q equals a and then approaches $+1.0$ from below.

So, as others have shown, linear supply curves are unit elastic, elastic, or inelastic according to whether they intersect the origin, the price axis, or the

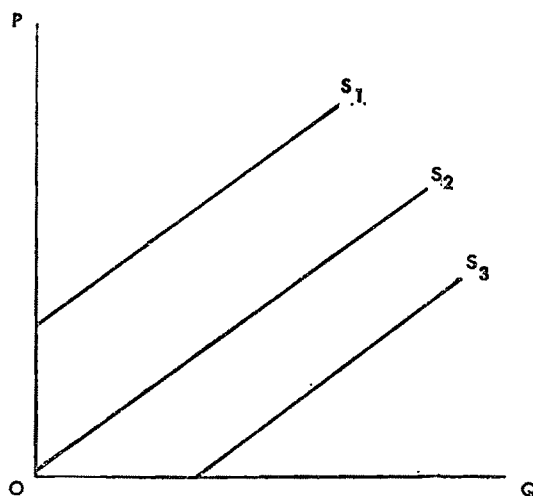


FIGURE 1

quantity axis. But all linear supply curves converge to an elasticity of $+1.0$ as Q (and P) increase. How fast $+1.0$ is approached depends upon a . The closer a is to zero, the faster the approach.

Along negatively sloped demand curves, total expenditures for a commodity increase, decrease, or remain constant as its price rises according to whether its demand elasticity is less than, greater than, or equal to -1.0 . This well-known relationship can be important in economic and business policy decisions. On the supply side, however, sellers' revenue always will increase as price rises along a positively sloped supply curve due to rightward shifts in the demand curve or to increases in guaranteed prices to sellers. Does the supply elasticity coefficient carry useful information other than whether quantities supplied change proportionately more or less than price? With linear supply curves, total revenue is given by the parabolic equation

$$(4) \quad R = P \cdot Q = \frac{Q^2 - aQ}{b}$$

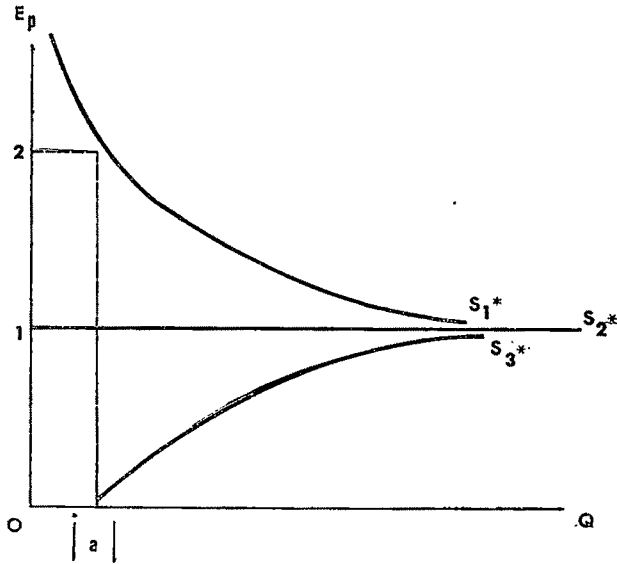


FIGURE 2

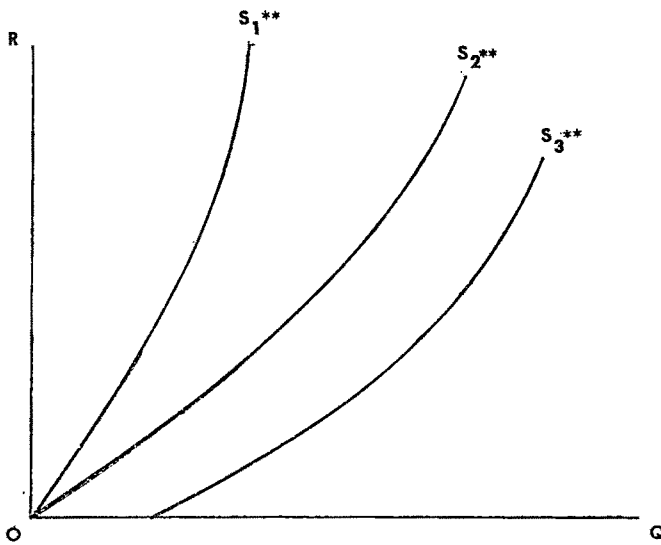


FIGURE 3

This equation measures the size of the total revenue rectangle under the supply curve at each Q . Figure 3 shows the behavior of R for Figure 1's three supply curves, each now indicated with a double asterisk.

Since R reaches no maximum as Q (and P) increase, it may be useful to know the proportionate change in R given a one per cent increase in price

when the supply curve remains stationary. Such an expression is

$$(5) \quad E_r = \frac{\partial R}{\partial P} \cdot \frac{P}{R} = 1 + E_p$$

where E_r is the "price elasticity of total revenue," and applies to any supply function, linear or not. As E_p approaches $+1.0$, E_r nears $+2.0$. That is, when supply is unit elastic, a one per cent increase in price, given through the market by a demand increase or by government support programs, will raise sellers' total revenue by two per cent. If supply is price elastic, total revenue will increase more than two per cent for each one per cent price increase. If supply is inelastic, a one per cent price increase will raise total revenue between one and two per cent. When supply is completely inelastic, a one per cent price increase leads to a one per cent revenue increase since there is no response in quantity supplied.²

In summary, supply elasticity varies along any linear supply function which does not intersect the origin. At higher prices and outputs, the linear function's elasticity approaches $+1.0$, the speed of approach depending upon the size of the price- or quantity-axis intercept. Sellers' total revenue always increases along any positively sloped supply curve as price rises. The percentage change in total revenue associated with a one per cent price change, whether or not the curve is linear, equals $+1.0$ plus the price elasticity of supply ($1 + E_p$). Thus, with any positive price elasticity, the proportionate response in sellers' total revenue always will be greater than any given percentage price change. These relationships should be kept in mind when interpreting and using elasticity information obtained from estimated or assumed supply curves.

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² Notice that this formulation of E_r applies equally well to demand curves where E_p is negative.

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Railroad Social Saving in Nineteenth Century America

The last ten years have witnessed rapid extension in the application of econometric technique to economic history, a development whose progress can be traced in past issues of this journal [3, pp. 86-98] [4, p. 128] [5, pp. 344-389]. The "new" economic history, it is claimed, unites economic history with economic theory and makes possible dramatic and precise revaluation of the significance of past events and institutions [3, pp. 92-93]. Perhaps the best known and most substantial "new" economic history project is Professor Fogel's reinterpretation of the contribution of railroads to U.S. economic development [1]. Fogel emphasizes the deficiencies in traditional, nonquantitative, railroad interpretations. These, he considers, represent "... the hero theory of history applied to things ..." [1, p. 236]. By using various econometric techniques Fogel calculated the value of railroad social saving in one year, 1890. The saving was equivalent to 6.3 per cent of gross national product [1, p. 220]. He regards this figure as a maximum measure of railroad contribution. A more realistic figure, making due allowance for economic adjustment to a nonrail situation, would be a saving of below 5 per cent of gross national product [1, p. 223].

How significant is a saving of 5 per cent of gross national product in 1890? Surprisingly, Fogel devotes scant attention to the significance of the figure he worked so hard to produce. He clearly regards 5 per cent as a good deal lower than the sort of figure that traditional nonquantitative interpretations would have led us to expect. Marc Nerlove has drawn attention to Fogel's failure to develop his conclusion. According to Nerlove, "Fogel's tacit acceptance of the triviality of the saving due to railroads is just the sort of half conscious quantification of which he accuses traditional economic historians" [2, p. 112].

Further analysis of Fogel's conclusion suggests that railroad social saving was far from trivial. The calculation represents railroad saving in one year, 1890. Fogel treats his conclusion as a measure of total railroad contribution—as if a saving of 5 per cent in 1890 implies that the gap between the actual U.S. economy and the hypothetical nonrail economy would have been 5 per cent twenty years earlier in 1870, and still 5 per cent in 1900. This assumption is unsound. It represents the extension of a snapshot analysis to a dynamic situation where the removal of any one economic variable, in this case railroads, would have a compound effect over time. Table 1 illustrates the compound effect of the loss of a railway social saving of 5 per cent, (a) for one year only, and (b) over five years. In both cases a GNP growth rate of 4 per cent per annum is assumed. Even if the economy lost its railroads for only a year, owing to a strike or similar cause, after five years the gap between the rail and nonrail economies would amount to 5.85 per cent. When the economy is deprived of its railroads permanently, the accumulated loss of railroad social saving would be much greater—in this case enough to more than offset an annual 4 per cent growth rate. After five years the margin between the rail and nonrail economies would amount to 27 per cent.

TABLE 1—INTERPRETATION OF A RAILROAD SAVING OF 5 PER CENT OF G.N.P. WHEN GROWTH RATE IS 4 PER CENT P.A.

(a) Effect of losing railroads for one year. i. Rail economy
 (b) Effect of permanent loss of railroads. ii. Nonrail economy

| | G.N.P. (a) | | (b) | |
|--------|------------|--------|--------|-------|
| | i | ii | i | ii |
| Year 1 | 100 | 95 | 100 | 95 |
| 2 | 104 | 98.8 | 104 | 93.8 |
| 3 | 108.16 | 102.75 | 108.16 | 92.55 |
| 4 | 112.48 | 106.86 | 112.48 | 91.25 |
| 5 | 116.98 | 111.13 | 116.98 | 89.9 |

The model exaggerates railroad contribution because it makes the dubious assumption that other things would remain the same. Society would have made some adjustment to the nonrail situation. The model is unrealistic to this extent, but it is much more realistic than a static interpretation. Even if we allow a generous discount for adjustment to a nonrail economy the indication is clearly that the long-term contribution of railroads was more important than Fogel implies. Traditional interpretations of railroad significance were not as far off the mark as Fogel suggests.

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Market Value and the Theory of Investment of the Firm

Recent econometric studies of investment behavior have found the market value of the firm to be an unsatisfactory variable in explaining changes in the firm's desired capital stock.¹ The purpose of this paper is to

¹ See [2], [6]. For some earlier studies in which the market value variable seems to perform more satisfactorily see [3], [4].

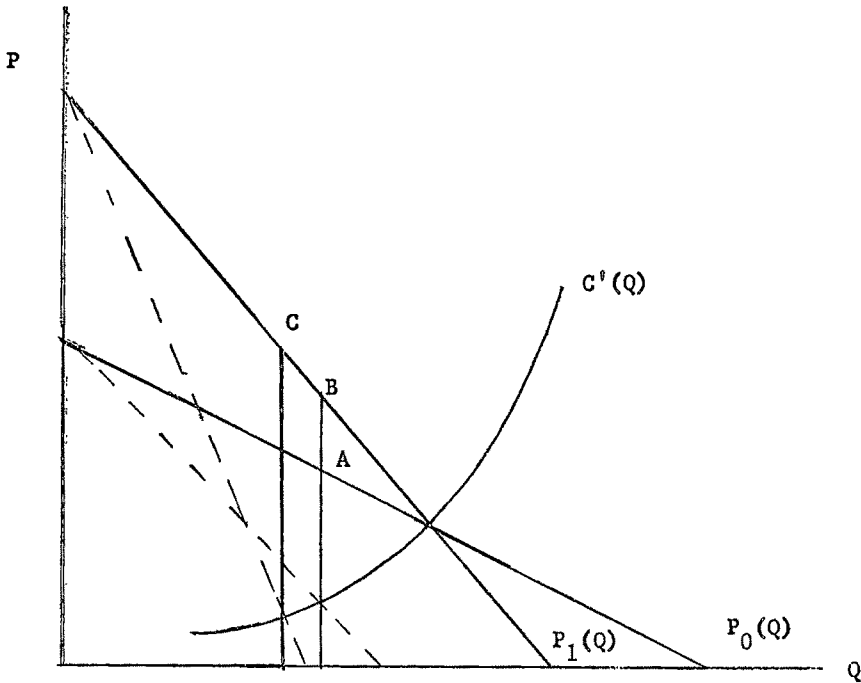


FIGURE 1. EQUILIBRIUM PRICE-OUTPUT COMBINATIONS

argue that erratic results are not really surprising since the theory of the firm indicates that desired capital stock can not reasonably be treated as a function of either market value or profits.

Consider the case of a rationally managed firm acting to maximize the present value of its net cash flows. This action, in a certainty world, is equivalent to maximizing the market value of the firm since its securities represent the total of all claims to the cash flows [5]. We can therefore write,

$$V = \int_0^{\infty} e^{-rt} [PQ - C(Q)] dt = \frac{1}{r} [PQ - C(Q)]$$

where V is market value, r is the rate of interest, P is output price, $C(Q)$ is the total cost of producing Q units of output, and Q is related to capital stock and other productive factors by the production function. Assume that the firm faces a demand curve of the form $P = P_i(Q)$ where the subscript i acts as a "shift parameter."² V is maximized when $PQ - C(Q)$ is maximized and this occurs, referring to Figure 1, at the price-output point A for which marginal revenue equals marginal cost.³

² The model considered here is of a monopolistic firm. Some comments on the competitive case follow.

³ The linearity of the demand curve is not essential to the argument as should be clear from the discussion.

Let there be an unanticipated shift in the demand function from $P_0(Q)$ to $P_1(Q)$. Clearly, V is greater at C , the new price-output equilibrium, than it is at B . In turn, B is a more profitable price-output combination than A because output is the same at A and B but price is greater at B . In this case, therefore, the demand shift permits the firm to decrease output but increase V . If the prices of productive factors are fixed, desired capital stock will be lower at the lower output.⁴ By similar reasoning it can be shown that there are demand shifts for which both V and desired capital stock increase. It follows that desired capital stock can not be treated as a function of market value since for any given change in V , capital stock may either increase or decrease depending on the shift in demand.

The foregoing analysis has been restricted to monopoly, dominant price-leader oligopoly, and other imperfectly competitive situations where the firm faces a downward sloping demand curve. In the case of pure competition with no externalities, the number of firms in the industry will change as demand fluctuates, but each firm will maintain the same output and capital stock so that changes in V , if any, will be unrelated to the desired capital stock of any firm.⁵ If externalities are introduced, equilibrium output and capital stock positions of the firm may be greater than, equal to, or less than their position before a demand shift according to the assumptions made about external technological and pecuniary economies. In the competitive industry case, moreover, interequilibrium profits or losses will affect V while not necessarily implying anything about the movement or equilibrium position of capital stock.

The discussion so far has been cast entirely in terms of the relationship between market value and desired capital stock but, insofar as V is a generalized profits variable, the same arguments apply almost directly to the similar use of current or past profits. Actually, the relationship will be even more tenuous to the extent that past profits are not related to future profits in any very simple way. Furthermore, the analysis has dealt only with the effects of demand shifts but it is clear that there are many other sources of slippage in the chain between capital stock and market value. Two of these sources of slippage are perhaps worth explicit mention. First, capital-saving technological change may occur which will affect the links between profits and output and between output and capital stock. Second, there is the possibility of changes in investor horizons about the length of time future investment opportunities will last, affecting the link between profits and market value. Ancillary to this last point, but nonetheless important, is the fact that a given change in market value may be consistent with a change in

⁴ Assuming, of course, that capital is not an inferior input in which case the lower output would be produced with a greater capital stock. Inferiority of capital does not change the basic point of this paper, although it does change the line of analysis slightly. For a discussion of inferior inputs see [1].

⁵ In a competitive industry with no externalities where output changes because firms with identical amounts of capital stock enter and exit, it may be that there is a linear relationship between the total capital stock held by the industry and aggregate market value of all firms in the industry. However, with less restrictive assumptions that allow for differing amounts of capital stock among firms, externalities, and changing factor prices a linear model does not seem plausible even on the industry level.

profit conditions at any point in the future—not necessarily the next period. Thus, V could increase in response to a highly profitable long-range condition despite current losses in output and reductions in capital stock.

To summarize, it seems doubtful that market value or any other profits variable is a good indicator of desired capital stock or even a good control variable since it reflects such a diverse set of phenomena. Even in those cases where a good fit is obtained, we do not know why it is good or how to interpret it.

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* The author, assistant professor of business economics at the University of Chicago, is indebted to Merton Miller and Arnold Zellner for their comments and especially to Peter Pashigan for suggesting the graphical analysis which has been used to illustrate the main point of this paper. All errors are, of course, my responsibility.

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Steel Imports and Vertical Oligopoly Power: Comment

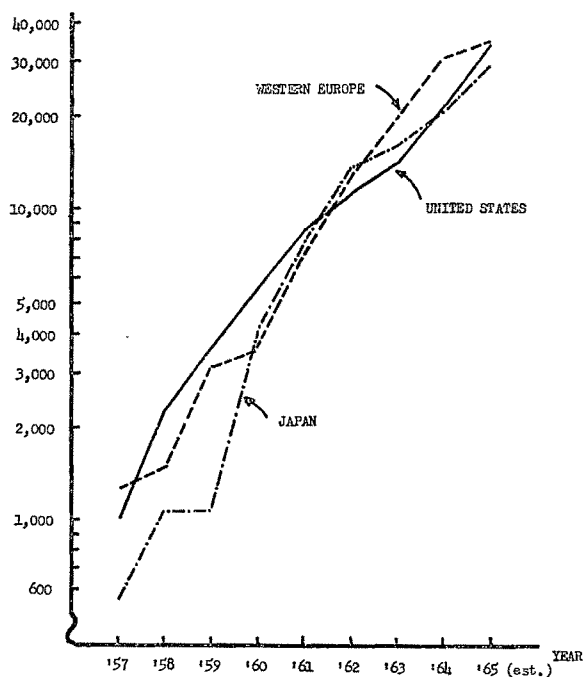
The article by W. Adams and J. B. Dirlam (hereafter referred to as A&D) in this *Review* [1] has provoked some expected comment [4] [5] to which A&D provide an interesting reply [2]. Much of this exchange concentrated upon the role of technological factors in the deterioration of the U. S. steel industry's competitive position.¹ It is the purpose of this note to show that in the case of the one major innovation A&D have chosen to support their argument, the LD process,² there is little evidence for their contention that the

¹ A&D have since elaborated their argument in a somewhat different direction, distinguishing between the propensities to innovate of large and small firms [3] and thus at least implicitly absolving U.S. industry from the wholesale condemnation pronounced in the paper here under discussion.

² The LD process, frequently referred to in this country as the BOF (basic oxygen furnace), was developed commercially in Austria in the late 'forties and early 'fifties. In this process, a charge of scrap and molten iron is placed in an open, pear-shaped, vessel. A jet of oxygen is then blown upon the surface of the charge at supersonic speed from a "lance" lowered into the vessel, bringing about a violent reaction. It is generally agreed that, except in unusual circumstances, the LD process is more economical, both in capital and operating costs per unit of output, than the open hearth method of steel-making.

United States lagged seriously behind the other major steel-manufacturing countries of the world. On the other hand, our results also cast doubt on the validity of R. E. Slesinger's statement that "by this measurement [production data for one year] U.S. producers were technologically ahead of—not behind—European producers" [5, p. 153]. In addition, our data show that A&D have little statistical support for their assertion that "... the oxygen process began to be introduced in U.S. steel mills at a significant rate only after the im-

FIGURE 1. INSTALLED LD CAPACITY, 1957-65
(thousands of net tons)^a



^a Where installation occurred over two or more years, full capacity was assigned to the year of plant completion.

Source: Kaiser Engineers, *L-D Process Newsletter*, October 1965.

port threat became serious" [1, p. 647; and again 2, p. 163] and that European mills were at least five years ahead in recognizing the economic advantages of the process. As a matter of fact, the introduction of the process occurred at an almost equal rate in Western Europe and the United States. While A&D are willing to make allowances for the technological difficulties some European countries had (revolving largely around the chemical composition of available iron ore resources), they see no extenuating circumstances, in terms of technological adaptation, in the case of U.S. industry.

G. A. Hone and D. S. Schoenbrod reach a position which appears most ten-

able on the basis of the available evidence: "Adams and Dirlam are misled in their view that oxygen conversion was adopted sufficiently more quickly by competing foreign steel firms so as to give them a significant price advantage" [4, p. 159]. But even they refer to Europe only and use rather sketchy capacity data which, by themselves, tell little about the competitive impact of the new technology.

Since the controversy centers largely upon the *rate* at which the LD process was adopted in the major steel-producing areas of the world, it is surprising that all the participants in the discussion restrict themselves to citing capacity or production figures for some selected years to support their positions. It would appear that only an examination of relevant data for the whole period could give some insights into the actual changes.

As a first step in assessing the rate of adoption of the new technology, Figure 1 shows the installed LD capacity in the United States, Western Europe, and Japan, from 1957 on. Before that year, there existed only two installations of LD converters in Austria (VOEST Linz and Donawitz), one in Canada (Dominion Foundries and Steel, Ltd.), one in France (Société des Aciéries de Pompey), and one in the United States (McLouth Steel Co.). Since the first Austrian oxygen furnace went into commercial operation in 1952, one can only conclude that all countries were equally slow in availing themselves of the new technology's benefits, if indeed one assumes that a five- to eight-year lag represents "slowness" in the case of an innovation requiring major pilot testing and large financial commitments. Professor Herbert Trenkler, one of the main contributors to the development of the process, commented on the early attitude of steel manufacturers in general very much in the terms which A&D now apply to U.S. firms only: "During that period the world's experts just sat back to watch the results obtained by the first big plants" [6, p. 6].

From 1957 on, there is little difference in the growth rate of LD capacity in any of the three areas under consideration. Of course one may still argue that U.S. industry *should have been* the leader in the adoption of the new process, but there is no evidence that it lagged behind Western Europe and Japan. This point is further emphasized by the data of Table 1, which show the share of the United States in world LD capacity. We see that it was actually in the early 'sixties that this country's firms failed to keep full pace with the overall spread of the new process.

TABLE 1—U.S. LD CAPACITY AS PERCENT OF WORLD INSTALLED LD CAPACITY

| 1957 | 1958 | 1959 | 1960 | 1961 | 1962 | 1963 | 1964 | 1965 |
|------|------|------|------|------|------|------|------|-------------|
| 31.0 | 32.8 | 35.0 | 32.1 | 30.8 | 26.0 | 24.1 | 25.0 | 27.7 (est.) |

Source: Kaiser Engineers, *L-D Process Newsletter*, No. 30, October 1965.

A valid argument can be made that the true competitive impact of the new technology does not occur just at the margin (i.e., absolute "new" capacity figures do not measure competitive position), but should be viewed in terms of

the share of total steel output of each area produced by the LD process.³ Table 2 shows the production of steel by the oxygen process as a percentage of total steel production, for the years for which comparable data were available.

From these figures it can be seen that there was no substantial difference in the growth of LD shares in total production between the United States and Western Europe. In the case of Japan we find, not too surprisingly, that new LD capacity played a major role in the rapid growth of that country's steel industry. Whether this is sufficient "to put the issue in focus," as A&D claim [2, p. 163], must remain a value judgment. They cite no further evidence to show that the LD process per se rather than the fact that it was part of entire-

TABLE 2—TOTAL STEEL PRODUCTION, PRODUCTION OF OXYGEN-BLOWN STEEL, AND PERCENTAGE SHARE OF OXYGEN-BLOWN STEEL IN TOTAL PRODUCTION, 1958-64 (thousands of metric tons)

| Year | Western Europe | | | United States | | | Japan | | |
|------|------------------|--------------|----------|------------------|--------------|----------|------------------|--------------|----------|
| | Total Production | Oxygen Steel | Per Cent | Total Production | Oxygen Steel | Per Cent | Total Production | Oxygen Steel | Per Cent |
| 1958 | 81,541 | 2,060 | 2.5 | 77,343 | 1,200 | 1.6 | 12,120 | n.a. | |
| 1959 | 88,212 | 2,497 | 2.8 | 84,773 | 1,691 | 1.9 | 16,632 | n.a. | |
| 1960 | 106,835 | 3,594 | 3.4 | 90,068 | 3,035 | 3.4 | 22,140 | n.a. | |
| 1961 | 106,073 | 4,578 | 4.3 | 88,918 | 3,599 | 4.0 | 28,268 | 5,357 | 19.0 |
| 1962 | 104,281 | 6,551 | 6.3 | 89,202 | 5,038 | 5.6 | 27,546 | 8,441 | 30.6 |
| 1963 | 107,028 | 9,900 | 9.2 | 99,120 | 7,752 | 7.8 | 31,501 | 12,045 | 38.2 |
| 1964 | 121,540 | 16,930 | 13.9 | 115,150 | 14,008 | 12.1 | 39,799 | 17,581 | 44.2 |

Source: Computed from United Nations, Economic Commission for Europe, *Quarterly Bulletin of Steel Statistics for Europe*, various issues.

ly new integrated plants, gave the Japanese steel industry a major competitive edge. A&D are correct when they criticize Slesinger for establishing a dichotomy between the *annual* capacities of "large" open hearths and "small" oxygen converters [2, p. 161]. On the other hand, the more frequent batches of output resulting from the much shorter heat times of the LD furnace do require major modifications in the supply of hot metal as well as in the production process following the ingot stage. This is but one example of the technological adjustments required for the successful introduction of the process. One may surmise that any industry that has more than doubled its output in a six-year span would be in a better position to take advantage of the new technology's inherent economies than one that has to build the new process into an existing physical production system.

³The use of percentage-of-capacity figures as a measure of competitive strength seems difficult to support. After all, what matters is not how much capacity an industry has available in a given year, but what proportion of its actual output was produced by the newer, less costly, process. Since A&D show that there existed unused capacity in all three areas during the period under discussion [1, p. 627], production data should clearly yield a more accurate measure. It seems safe to assume that the most economical capacity was kept on stream whenever there existed a choice of leaving some equipment idle.

To sum up: There is no statistical evidence of a major lag in the construction of LD capacity in the United States, compared with Western Europe and Japan. The European and U.S. steel industries also produce approximately the same share of their total output by the new process, while Japan has based the rapid expansion of its steel production largely upon new plants built around the requirements of the oxygen furnace. If one wishes to argue that a slow rate of technological progress had contributed to the competitive difficulties of the U.S. industry, one has to do so on some basis other than the history of the LD process.

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Steel Imports and Vertical Oligopoly Power: Reply

Rosegger comments on two of our recent articles—the first dealing with steel imports and vertical oligopoly power [1], the second with the inventive and innovative performance of the U.S. steel giants [3].

In the first article [1], we described the catatonic price policy of a vertical oligopoly in the face of burgeoning imports. We showed that the steel giants were so intent on preserving their vertical price structure that they preferred to abandon certain markets rather than meet the importers' price cuts. We noted that their nonprice reaction to the import threat consisted of a technological and political counterattack. Rosegger seems to forget that we treated technological innovation, i.e., the growth of oxygen steelmaking and continuous casting, as a *response* to, not as *cause* of, the competitive challenge of imports [1, pp. 646-47].

In the second article [3], we tested the "Schumpeterian" hypothesis and showed that, in introducing the oxygen converter, the U.S. steel giants lagged behind their smaller domestic rivals as well as their foreign competitors. The LD converter—the "only major breakthrough at the ingot level since before the turn of the century" [3, p. 175]—was invented by a miniscule Austrian firm in 1950, and first installed in Europe in 1952, Canada in 1953, and the

United States in 1954. The U.S. innovator was McLouth Steel, a firm which accounted for roughly one per cent of domestic steel capacity. The Big Three did not follow its lead until a decade later—U.S. Steel in December 1963, Bethlehem in 1964, and Republic in 1965. To the extent that the domestic steel industry kept pace with its foreign counterparts at all, this was largely attributable to the innovative propensity of the smaller firms. (For the distribution of oxygen steel capacity by size of U.S. producers, see [3, p. 183].) Rosegger is strangely silent on this point.

International comparisons are hardly more flattering to the giant-dominated domestic industry. ECSC-plus-Austria consistently *produced* larger absolute tonnages of oxygen steel than the United States till 1962, and larger relative tonnages until the end of 1965. Japan's oxygen steel production exceeded that of the United States in absolute terms starting in 1961, and in relative terms starting in 1958. As late as 1965, oxygen steel output in the United States was still a smaller proportion of total crude steel production than in such major producing nations as ECSC, Austria, Japan, Australia, and South Africa [4, esp. Table 1]. Evidently, the United States still had not closed the gap. As for the installation *rate* of LD *capacity*, purportedly portrayed in Rosegger's Figure 1, keep in mind that that the U.S. steel industry (in 1960) was 50 per cent larger than that of ECSC, 500 per cent larger than that of Japan, and 5,000 per cent larger than that of Austria!¹

The foregoing international comparisons, as we have repeatedly pointed out [2, p. 163; 3, p. 182; 4], are all the more remarkable because the new technology was immediately (1950) applicable to the low-phosphorus ores of North America, whereas most ECSC producers had to wait until 1957 before the LD process was modified to accommodate the high-phosphorus ores traditionally used in their steel industries.

Finally, as we have demonstrated elsewhere, [2] [4], it is doubtful whether technological feasibility of the new process was a significant bar to innovation by the steel giants, or whether it can explain their decision to install some 40 million tons of the "wrong" (i.e., open-hearth) capacity during the 1950's—capacity which was obsolete almost as soon as it was put in place [5]. That the U.S. steel giants lagged in innovation behind their smaller rivals at home and their Lilliputian competitors abroad is an indisputable fact. The explanation for this lag may be debated *ad infinitum*—unless, of course, one is prepared to accept our quite modest hypothesis that America's steel oligopolists are slothful innovators.

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WALTER ADAMS AND JOEL B. DIRLAM

¹ It is curious that Rosegger criticizes others for using "selected years to support their positions" but does not explain his selection of 1957 as a baseline date for his international comparisons. After all, the Austrians had *installed* their first converter fully *five* years earlier.

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A New Approach to International Liquidity: Comment

In a recent issue of this *Review* Professor Tibor Scitovsky proposes a "functional approach" to international finance [1]. According to this approach a country is to maintain "the kind of balance of payments that best serves its domestic goals" rather than an equilibrium in the balance of payments for its own sake. It is a credit to his ingenuity that once his scheme is adopted, the level of unemployment will decrease, international liquidity will increase, and at the same time developing countries of the world will receive unrequited assistance.

The gist of this scheme is to allow the deficit country to obtain exchange reserves from a reformed International Monetary Fund with goods and services produced in the country. The surplus country will eventually hold these newly created reserves, and developing countries will receive the additional products. A problem arises, however, as to the size of exchange reserves to be given to the deficit country in exchange for the products. Professor Scitovsky proposes that the new reserves created should be equal to the value of the products to be transferred to developing countries. It will be shown in this note that if this proposal is adopted it is most likely that the deficit country will be continuously in deficit, the creation of new reserves being always short of the deficit in the balance of payments.

Suppose a country has an initial deficit of D_t^* , where $D_t^* = M_t^* - X_t^*$, and M_t^* and X_t^* are the imports and exports of the country in period t . It is getting new reserves by producing additional output (T_t^*), which are equal to the value of this output. There is, however, an additional increase in the output of the country (ΔY_t) due to the multiplier effect of the autonomous increase in the aggregate demand by T_t^* . Then, given a positive value of the marginal propensity to import (m), the increase in imports,

$$\begin{aligned}\Delta M_t &= m \cdot \Delta Y_t, \\ &= (m \cdot \pi) \cdot T_t^*,\end{aligned}$$

where π is the income multiplier. Then, the post-transfer imports,

$$M_t = M_t^* + \Delta M_t.$$

Now, since the transfer of the output is equal to the pre-transfer deficit, D_t^* ,

there will still remain a deficit which is due to the increase in imports and which cannot be financed with the newly created reserves. That is, when the transfer has been made, the post-transfer deficit,

$$\begin{aligned} D_t &= M_t - X_t^*, \\ &= D_t^* + \Delta M_t. \end{aligned}$$

Obviously, given the value of T_t^* , the value of ΔM_t depends on $(m \cdot \pi)$.

For most countries the value of $(m \cdot \pi)$ would be greater than zero but less than one. They would, therefore, find themselves short of reserves even after the receipt of the new reserves, which are equal to D_t^* . One simple solution of this problem seems to be to give the deficit country new reserves which are greater than the transfer of the output and sufficient to cover the post-transfer deficit.

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REFERENCE

1. T. SCITOVSKY, "A New Approach to International Liquidity," *Am. Econ. Rev.*, Dec. 1966, 56, 1212-19.

Reply

I am glad that Professor Lee caught and pointed out my omission to take into account the multiplier effects created by the deficit country's producing the goods with which it pays for its new reserves. These effects are another important reason for including the payment for the new reserves in the recipient country's government budget. The simplest case is that where the new reserves are paid for out of taxes. Then the value of the multiplier is unity, the additional repercussions on the balance of payments Lee is concerned with are absent, and my original argument is unchanged.

TIBOR SCITOVSKY

National Debt in a Neoclassical Growth Model: Comment

In a recent article in this *Review*, P. A. Diamond produced a competitive model which concluded that the utilities of some generations are altered by converting part of the national debt from internally held debt to externally held debt [2]. In Diamond's model a flotation abroad (rather than at home) of a given amount of national debt means an equivalent importation of real capital. This extra capital increases wages and decreases interest rates, thereby currently benefiting the younger, working class at the expense of the retired capitalists.

Our critique of Diamond's model of externally held debt concerns the basic

premise above—namely, that a flotation of debt abroad means an importation of real capital. Before developing the critique, we shall attempt an economic explanation of Diamond's complete set of qualitative conclusions regarding the exportation of national debt. This should illustrate that Diamond's results flow quite naturally from his basic premise and should provide an indirect check on many of the technical manipulations contained in his paper.

1. *An Economic Explanation of Diamond's Conclusions*

Diamond assumes that an export of national debt is not merely a temporary, one-period movement but continues on into the next period; therefore, the members of the current younger generation, who gain a wage increase via the current capital import, will suffer an interest rate drop in the next period when they become retired capitalists. More specifically, Diamond, who analyzes only steady states, assumes that total externally held debt expands at the same rate as the total population. Consequently, the current younger generation, during its retirement, will lose interest equal in total value to the wage bill increase it receives via the current debt exports multiplied by one plus the rate of population growth (thereby keeping wage rates constant over time). This future loss to the members of the current younger generation must be discounted (by multiplying it by the reciprocal of one plus the rate of interest) to compute its present cost to this generation. Therefore, whenever the rate of interest exceeds the rate of population growth, the debt export provides a component of net gain to the current younger generation and all future generations. The current older generation loses.

If intramarginal debt exports exist, further effects of this debt-export policy emerge. Diamond assumes in the text of his paper that the world rate of interest always adjusts to the domestic rate of interest. So an additional debt export—and therefore capital import—benefits everyone in the country via the reduction in the interest rate and therefore the taxes necessary to service the intramarginal exports of national debt. Diamond adds these positive effects of a debt export to the effects described above, so that whenever intramarginal debt exports exist, even when the rate of interest is somewhat below the rate of population growth, the existing younger generation and all future generations will gain from a debt export policy. Diamond drops his assumption that the world rate of interest adjusts to the domestic rate of interest in the first paragraph of his Appendix B. In the plausible case in which the world interest rate increases with the exportation of debt, all generations in the country *suffer* from an additional debt export through the interest increase on the intramarginal debt exports. In this case, the existing younger generation and all future generations may be hurt by the debt export even when the rate of interest exceeds the rate of population growth.

The above discussion contains all of Diamond's qualitative conclusions regarding the effects of an exportation of existing national debt. Therefore, *given* Diamond's basic assumption that an exportation of existing national debt means an equivalent importation of real capital, all of his conclusions regarding the effects of an exportation of national debt are consonant with standard economic analysis.

2. *The Critique*

If an exportation of national debt from country A to country B were to mean an increase in the real capital used in country A, there would be an *increase* in the return to capital in country B as well as a decrease in the return to capital in country A. An importation of real capital would thus disequilibrate the international market for real capital, a market which is in equilibrium only when the rates of return in the different countries bear a given relation to one another. Ignoring transportation and transaction costs, this market would re-equilibrate by an exportation of originally imported real capital, leaving the exportation of national debt with no effect whatever upon the allocation of *real* resources.

Even if transportation and transaction costs did exist (they apparently do not in Diamond's model), they could be ignored as long as the disequilibrium which would result from a movement of real capital to country A is foreseen; for then this capital import would not have occurred in the first place. The exporters of domestic national debt in country A would have preferred to take a perfect substitute such as a debt instrument rather than an imperfect substitute such as real capital whose return decreases with its importation; and the importers of foreign national debt in country B would have preferred to give up a perfect substitute such as a debt instrument rather than an imperfect substitute such as real capital whose return increases with its exportation.¹

Hence, the government's exportation of its national debt, its flotation of national debt abroad rather than at home, has *no effect* upon any equilibrium interest or wage rates and *no effect* upon the utility of any generation.

The incompleteness of Diamond's international market for real capital is the basic source of his strange mathematical result that a change in the political allegiance or location of the holders of the national debt alters the allocation of real resources in a perfectly competitive model. Diamond's model does contain a supply curve of real capital imports in his Appendix B, but it completely ignores the demand side. So the model fails to equate the quantity supplied with a quantity demanded in the market for real capital imports. The only economically logical interpretation of Diamond's model is that the demand curve for capital imports coincides with the supply curve; but this would yield an indeterminate equilibrium (and also is inconsistent with his assumption of a diminishing marginal product of capital).

Therefore, Diamond's analysis of externally held debt, an analysis based upon an international flow of real capital, is inconsistent with the existence of an equilibrating international market for real capital.

This critique is by no means a denial of the politically important classical orthodoxy that internally held debt is a debt which a country owes to itself while externally held debt is a debt which one country owes to another. The differing distribution effects of unanticipated repudiations of debt (which may

¹ Of course, the government could succeed in importing real capital by incurring transportation or transaction costs which the free market deems to be overly expensive or, more generally, by subsidizing capital imports; but then the capital import would be attributable entirely to the government's expenditure or subsidy policy, and not at all to the exportation of the country's national debt.

arise in nonperfectly competitive worlds) are of obvious political importance to a country. Nor is it a denial of the relevance of the international capital market in helping to determine the final economic effects of a change in the *total* national debt. Clearly, the smaller the elasticity of supply of external capital, the larger the change in the domestic interest rate resulting from a given expansion in the *total* national debt. But it is a denial of that part of the "new orthodoxy" which claims that the *economic* effects of externally floated debt differ from the *economic* effects of internally floated debt.²

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² A critique of this part of the "new orthodoxy" for the special case in which the marginal product of the imported resources is constant has been produced by Buchanan [1].

REFERENCES

1. J. M. BUCHANAN, *The Public Finances*. Homewood, Ill., 1965, Ch. 28.
2. P. A. DIAMOND, "National Debt in a Neoclassical Growth Model," *Am. Econ. Rev.*, Dec. 1965, 55, 1126-50.

BOOK REVIEWS

General Economics; Methodology

New Dimensions of Political Economy. By WALTER W. HELLER. Cambridge: Harvard University Press, 1966. Pp. xii, 203. \$3.50.

National Economic Policy. By JAMES TOBIN. New Haven and London: Yale University Press, 1966. Pp. x, 211. \$5.00; paper, \$1.75.

The ideas grouped together under the label of the New Economics no doubt owe much to Keynes. But there is an essential difference, rightly emphasized by Walter W. Heller in his *New Dimensions of Political Economy*, between economic ideas that make convincing reading in a principles text and ideas that "work"—gain support, get adopted, bring about results. The forging of a durable link between the doctrines of the fiscal activists of the forties and fifties and the policies advocated and put into practice by the new economists of the sixties is the great achievement of the New Economics. This achievement, described by Heller as "translation, adaptation, and innovation," is the subject of these two books.

Heller's *New Dimensions*, an expanded form of his Godkin Lectures of 1966, contains three inter-related essays. The first, "Advice and Consensus in Economic Policy Making," is devoted to the new role of economists. Heller traces out the changes in the environment of public policy making and the progress in economic thinking that together have ushered in the Age of the Economist. It is no exaggeration to say, as Heller does, that "Economics has come of age in the 1960s. Two Presidents have recognized and drawn on modern economics as a source of national strength and Presidential power. Their willingness to use, for the first time, the full range of modern economic tools underlies the unbroken U.S. expansion since 1961" (p. 1). If anything, this may be an understatement, since it neglects the increasing use of economics or in Washington parlance, Hitchcraft, in the management of the vast enterprises of federal, state, and local governments. But the increasing role of economics in the management of government enterprise only serves to reinforce Heller's argument.

Heller provides a definitive inside account of the role of the Council of Economic Advisers in the Kennedy and Johnson administrations. In the wake of the vast success of the Council under Heller's chairmanship, many proposals have been made to expand the scope of its activities. In Heller's view the strength of the Council lies in its immediate access to the President and in the absence of conflicting responsibilities. He is skeptical of most of the proposed changes, favoring informality, flexibility, and fluidity in the Council's relationships within the Executive Office.

The second essay, "The Promise of Modern Economic Policy," presents the central doctrines of the New Economics. Omitting the qualifications supplied by Heller, the basic ideas are these: The goals of economic policy are 4 per cent unemployment, a rate of growth of real output of $4\frac{1}{2}$ per cent, price stability, and balance-of-payments equilibrium. The current state of

fiscal policy is measured by the full employment surplus, the federal budgetary surplus as measured in the national income accounts that would prevail at full employment of present labor force and capacity. If actual output falls short of potential output, the full employment surplus should be decreased; if pressure on capacity becomes too great, the full employment surplus should be increased.

Up to this point the New Economics is recognizable as orthodox Keynesianism, advocated by Hansen and Keynes and taught by Samuelson. However, the concept of full employment surplus provides an essential link between theoretical and practical analysis of fiscal policy. Even a federal budget substantially in deficit at less than full employment may not provide sufficient stimulus. The degree of stimulus is measured not by the size of the current deficit but by the size of the surplus that would prevail at full employment. A second level of sophistication is introduced through the concept of fiscal drag, defined by Heller as "the automatic growth in potential revenue arising out of growth in potential GNP" (p. 181). Since tax receipts expand more than in proportion to GNP, the political economist is fighting a continuing battle with fiscal drag in the form of a \$7 to \$8 billion dollar annual increase in revenues over automatic increases in transfer payments. In designing fiscal policy provision must be made for a fiscal dividend in the amount of \$7 or \$8 billions in greater expenditures and reduced taxes each year in addition to any reduction in taxes or increase in expenditures necessary to stimulate the economy to higher levels of performance.

The theory of fiscal policy just outlined may sound like Keynes, but it will be helpful to the less than omniscient student to learn that the dynamic character of the analysis adds a new dimension to the Keynesian discussion of fiscal policy. The conventional Keynesian prescription of balance over the cycle with surpluses in good years and deficits in bad as well as the objective of orthodox fiscal policy, a balanced budget at all times, is replaced by balance at full employment, a perpetually moving target. Intellectually, this change in perspective may be translation and adaptation; it represents a remarkable innovation from the point of view of practical economic policy.

Heller documents the course of the political and practical success of the New Economics, beginning with the fight against a tax increase in 1961 during the Berlin buildup, culminating in the great tax cut of 1964, and continuing with the excise tax cuts of 1965. He answers critics on the right by pointing to five years of rapid growth without inflation. He deals with critics on the left by demonstrating that two-thirds of the fiscal dividend of \$48 billions between 1961 and 1965 was employed for increases in expenditures while only one-third took the form of tax reductions. He also reviews other aspects of Council policy, including the wage-price guideposts, and analyzes the economic policy of 1966, the first post-Heller year of the Johnson administration.

Heller's third essay, "Strengthening the Fiscal Base of Our Federalism," is devoted to the much-discussed Heller Plan for providing federal tax revenues to the states in the form of direct grants not tied to specific governmental functions. The Plan is to distribute 2 per cent of the federal personal income tax base or about 10 per cent of receipts from the personal income tax to the

states on a per capita basis. The justification for the Heller Plan is that the federal government has largely pre-empted the only tax that is growth-responsive (that has receipts increasing more rapidly than GNP), leaving the states with less responsive sales and property taxes. States on the other hand have responsibility for items of public expenditure that should be highly growth responsive, especially expenditures for health, education, and welfare. The Plan commits a sizeable portion of the annual fiscal dividend in advance, reducing the burden of fiscal drag.

James Tobin's collected essays on national economic policy, covering writing over the past fifteen years, provide an interesting perspective on the development of ideas about economic policy. Three essays written in the waning years of the Eisenhower administration, taken together with Heller's 1966 characterization of the New Economics, yield a before-and-after picture of the viewpoint of the leading new economists. Despite the rapid development of a consensus around the New Economics, important differences of opinion remain between the new economists and proponents of traditional views. Tobin confronts two of the critics, Milton Friedman and William McChesney Martin, in his essays, "Barry's Economic Crusade" and "Lessons of Monetary History."

Whereas Heller would allocate a substantial or even predominant part of the recurrent fiscal dividend to expenditures on federal, state, and local levels, the Friedman-Goldwater program (taken from statements by then-Senator Barry Goldwater during the 1964 Presidential campaign) was to use all or part of it for tax reductions. Of course, it should be borne in mind that the use of the whole dividend or even an increase of taxes to provide for more expenditures is an alternative view not without its advocates. The broad agreement about the underlying economic mechanism and the effects of alternative fiscal policies as characterized by the New Economics is reassuring.

Another fundamental disagreement between Friedman-Goldwater and the new economists is about the use of discretionary fiscal policy. Both Heller and Tobin advocate additional flexibility through the enactment of stand-by authority for short-term manipulation of tax policy by the President. The Friedman-Goldwater alternative is an "automatic" policy geared to long-run growth trends in the economy and, presumably, to similar "automation" of other instruments of economic policy such as the supply of money. Another outstanding policy issue is the management of monetary policy by the Federal Reserve and, more generally, the design of policies for the achievement of balance-of-payments equilibrium and price stability along with full employment and rapid growth. Neither of these issues is close to settlement; the rapid evolution of views on the objectives of fiscal policy suggests that the ultimate consensus is unlikely to bear much resemblance to the positions advanced as alternatives at the current, essentially academic, phase of the debate.

The enhanced role of economists has raised the quality of public discussion of economic policy and expanded the range of government policies brought under professional scrutiny. Tobin's final essay, "Academic Economics in Washington," touches on the implications for university training and research in economics. Prior to 1961 the formulation of economic ideas and their ad-

vocacy as practical measures of policy were about the most one could expect of the most political of economists. The situation was eloquently summarized in another context by Samuelson: "The promise is always in the future; we are like highly trained athletes who never run a race, and in consequence grow stale."¹ In the Age of the Economist all the available athletes have a busy schedule of real races to run. The implications of this experience for university economics are summarized by Tobin as follows:

I think that any academician who has had this experience will come away from it with two feelings about his own profession: confidence and humility. He will have confidence that analytical economics can make a valuable and distinctive contribution to policy; it can raise significantly the level of the considerations of logic and of fact which go into policy discussions and decisions. He will have humility because he will have noticed many instances where decisions had to be based on incomplete information, inadequate research, and hasty analysis. He will return to his university, then, with a new sense of research priorities and an enriched agenda of problems for himself, his colleagues, and his students (p. 206).

It is unnecessary to recommend that these books be made required reading. They will be read avidly by economists and noneconomists alike.

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¹ P. A. Samuelson, *Foundations of Economic Analysis*, Cambridge 1947, p. 4.

Guidelines, Informal Controls, and the Market Place: Policy Choices in a Full Employment Economy. Edited by GEORGE P. SHULTZ AND ROBERT Z. ALIBER. Chicago: The University of Chicago Press, 1966. Pp. xi, 357. \$5.00; paper, \$1.95.

This conference volume reflects the rising tide of criticism of the President's wage and price guideposts in practice, if not in theory, as of April 1966. This tide led to the abandonment of the magic 3.2 per cent annual guidepost in the January 1967 *Economic Report of the President*. Yet the volume is still timely reading, since the principle behind the guideposts is far from dead. To quote from the 1967 *Annual Report of the Council of Economic Advisers* (p. 128): "The Council recognizes that the recent rise in living costs makes it unlikely that most collective bargaining settlements in 1967 will fully conform to the trend increase of productivity . . . (but) . . . The only valid and noninflationary standard for wage advances is the productivity principle. If price stability is eventually to be restored and maintained in a high-employment U.S. economy, wage settlements must once again conform to that standard."

The volume consists of eight papers presented at a conference held at the University of Chicago in late April, 1966, together with formal comments and summaries of four group discussions, on the subject of guidelines and informal controls. Although the bulk of the proceedings is devoted to wage-price guideposts, two papers, by Harry G. Johnson and Allan H. Meltzer, consider the guidelines for direct investment and bank credits abroad. There is a useful introduction and summary by George P. Schultz and Robert Z. Aliber.

In the initial essay, Milton Friedman argues that price inflation is always

and everywhere the result of increases in the supply of money relative to output. He notes that inflation can be suppressed to some extent, but this is harmful since suppression prevents the price system from working effectively. He views the wage-price guideposts as undesirable, since they divert attention from fundamental causes, and spotty compliance distorts relative prices of factors and products.

In the following passage (p. 21), Friedman conditionally admits the possibility of cost-push: "Unless the cost-push produces a monetary expansion that would otherwise not have occurred, its effects will be limited to at most a temporary general price rise, accompanied by unemployment, and followed by a tendency toward declining prices elsewhere." In his address to the conference, Gardner Ackley develops the rationale of the guideposts on the basis of "excessive income claims" (p. 68). That is, he observes the tendency of powerful unions to obtain wage increases in excess of productivity increases, and of oligopolistic firms to increase their markups as demand rises, beyond a point still short of full employment of resources. It is within this band of inflationary cost pressures, short of full employment, that guideposts are intended to be effective by appealing to the sense of social responsibility of business and union executives who have some discretion in price and wage determination. He rejects the notion that government should not validate excessive income claims by restraining aggregate money demand at the cost of at least temporary unemployment. While defending the guideposts and their application against some of the criticisms, Ackley admits "many undesirable aspects," and challenges his fellow economists to produce superior alternatives. At that time, he was not ready to relinquish guideposts until superior alternatives were found.

A defense of guideposts was also provided by Robert Solow, in a lower key as indicated by the title of his paper, "The Case Against the Case Against the Guideposts." Like Ackley, Solow rejects the anti-inflationary policies that produce unemployed resources as too costly. He sees the guideposts offering a little help at low cost in their influence on individuals and organizations with market power who, he believes, are sensitive to public opinion. In effect, the guideposts seek to educate such decision-makers to imitate the effects of competition. He adduces George Perry's relationship between wage changes and related variables for the period 1948-1960 to demonstrate that wages between late 1962 and 1965 rose significantly less than would be expected, presumably as a result of application of the guideposts. Yet Solow is disturbed by their unevenness in operation.

Much sharper criticism is expressed in the other papers. John Dunlop, while recognizing the concerns that gave rise to guidepost policies, scores them as not commanding wide assent in part because labor and management representatives had no real part in their drafting; for not being expressed in meaningful, operational terms from the viewpoint of private decision-makers and public mediators and arbitrators; and as having an inequitable impact, with no procedures for review "or provision for due process." He further raises a number of analytical questions, and maintains that the Council is ill-equipped

to serve as an administrative agency, even of an informal character.

Arthur Ross seems inclined to admit the validity of the cost-push factor in price inflation, and reviews the experience of several European countries as well as the United States in devising and administering wage-price, or "incomes," policies. He views the policies as still experimental, and unsatisfactory in many ways, as implied in a long list of questions he raises. His very general conclusion is (p. 130): "It may be that the problem is insoluble so long as wage and price policies are tied so closely to a single criterion, the trend productivity rate; and that the appropriate terms of wage and price restraint will have to be redefined from time to time on the basis of intensive study of the entire economic situation. Such an examination might provide a format for intimate consultation among leaders of government, industry, and labor."

This reviewer wonders what happens to the intended anti-inflationary impact of guideposts when criteria in addition to productivity are admitted into the formula. One also wonders what the other criteria should be, especially in view of Ross' statement (p. 130): "Attaching a cost-of-living escalator clause to the guideposts, under circumstances like those of the present, would not only contribute to a wage-price spiral but also yield a target figure beyond the reach of all but the most powerful unions."

Philip B. Kurland reviews relevant constitutional history, and concludes that "the present means of wage and price controls is constitutionally suspect" (p. 239), as well as ineffective. If effective controls are to be applied, he favors congressional legislation, but recognizes the political difficulties of this course. In his comments, another lawyer, Frederick R. Livingston agrees that the guideposts have not been effective at the collective bargaining table, and that if reformulated, they should be given a legislative base. He sets three standards (p. 260): "(1) There should be a procedure for public hearings to determine the appropriate guideline or the range of such guidelines; (2) there should be legal safeguards to assure equal standards for application and equal protection of the laws; and (3) there should be procedures for hearings with appropriate due process safeguards."

The conference was not notably strong in producing suggestions for practical, effective alternatives to the guideposts. Friedman's implied solution was to limit increases in money supply to probable increases in supply capacity, even if these should not validate possible excessive income claims resulting at least temporarily in reduced real demand, and increased unemployment. This does not seem politically feasible in the present environment, although it is clear that monetary and fiscal policy should be consistent with, and support, wage-price policy. Solow's plug for strengthening competitive forces was admittedly a long-range approach. Dunlop advocated a massive assault on bottleneck areas, in which prices are rising rapidly, in order to increase supply capabilities and possibly to restrict demand increases. While put forth as an alternative to guideposts, Dunlop's proposals also seem primarily long-range.

The type of wage-price policy which Kurland and others would legislate was not spelled out. The reviews of foreign approaches would suggest the pos-

sibility of creating a board which would receive advance notice of intended increases in wages and prices, with the power to investigate and possibly to disapprove those judged excessive. Ross raised the possibility of a potent consultative mechanism, involving major interest groups, to seek periodic agreement on basic wage-policy. Doubt was expressed that such agreements would necessarily be in the best public interest. Occasional temporary wage (or wage-price) "pauses" was another European device for slowing-down inflation, which has not been used here in peacetime.

The problems discussed at the Chicago conference remain a challenge to the creative thinking of economists. There must be a better means, or combination of means, of restraining inflationary tendencies in fully employed free economies than any suggested so far. The important thing is that any new method which is tried should preserve the great advantages inherent in market-direction of resource use.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Planning for Steady Growth. By GAUTAM MATHUR. New York: Augustus M. Kelly, 1965. Pp. xiv, 386. \$12.50.

The dust cover informs that this book presents "a fresh approach to development planning," laying down "the principles of constructing the framework of a long term plan of economic growth." This description can be misleading. There is hardly anything in the book in the way of practical aid to those concerned with framing development plans. It is, actually, an essay on a particular model of economic growth (the von Neumann model), written, unmistakably, in the spirit of the theoretical literature associated most often with the name of Joan Robinson. Indeed, to a large extent the book is an elaboration of various topics originating from previous works of Mrs. Robinson, in particular *The Accumulation of Capital* and *Essays in the Theory of Economic Growth*. Gautam Mathur provides in this book some careful thoughts and useful insights on a number of very complicated subjects, such as the choice of optimum techniques of production and the effects of technical progress thereupon.

The reader should be warned that working through this book is not easy. The discussion is usually too lengthy, quasi-formal, extremely abstract and intricate. It is doubtful, frankly, whether the numerical examples spread here and there are of much help. Mathur's proclivity to decorate his discussion with aphorisms such as "Bastard Golden Ages," "Forward Platinum Ages," "Reverse Wicksell-Effect Spectrum" and many other happy labels put afloat by Mrs. Robinson, makes it all the more difficult.

In spite of these faults, the book is a valuable contribution to capital and growth theory in that it focuses on and clarifies the kind of theory that has developed in Cambridge (England) in contrast to the so-called "neoclassical orthodoxy." Most rewarding, when you read the book carefully enough to penetrate the invective and terminology you realize that, despite differences of emphasis and description, there are many connecting links between these two schools. Obviously, in equilibrium long-run growth, the Robinson-Mathur, von Neumann or any of the various neoclassical models exhibit very similar characteristics. It is true that Mrs. Robinson and other neo-Keynesians have been mainly concerned with non-Golden Age situations, dominated by bottlenecks in production, unemployment, inflationary pressures and other disequilibrium indicators. But even here some convergence is in sight. For example, Mathur's discussion on the possibility of temporary labor unemployment if the capital stock lacks the capacity to employ the entire labor force is very similar to the results derived by Solow, Johansen, and others in the recent vintage models with *ex post* fixed proportions.

As stated at the outset, the book is devoted to the problem of the choice of the optimum path of accumulation which minimizes the time for achieving the full-employment Golden Age path of steady balanced growth (the von Neumann path). Since attainment of the von Neumann path usually takes time, the "*minimum time objective*" implies that society does not discriminate between various transitional consumption paths of equal duration. A social goal function of this kind is open to many objections, although various "Turnpike Theorems" suggest that even with more general criteria there is a long-run tendency for the efficient path of economic growth to approximate the von Neumann path. Thus, given certain institutional restrictions on consumption, the implied policy may well be appropriate, particularly for an underdeveloped country. It is regrettable, nevertheless, that there is no discussion in the book of the Ramsey and more general optimum accumulation under welfare criteria that attach values to transitional consumption. Economists are now busily concerned with the solution of this general problem which carries both theoretical interest and practical relevance.

In the first three parts of the book, Mathur outlines the various constraints imposed on a development plan, describes in detail the von Neumann type technology, and sets up the conditions for long-run Golden Age equilibrium. He then goes on either to relax various equilibrium conditions or to introduce rigidities (such as a floor wage rate which poses an "inflation barrier"), and analyzes the possible courses that the economy can pursue. The analysis of these situations follows pretty much that of Mrs. Robinson in her Model of Accumulation, and there is no need to review it here.

It is proposed to concentrate in what follows on the discussion of the choice of techniques of production contained in Parts IV and V of the book. The basic problem is as follows. Neoclassical aggregative models assume that there is a fundamental relation between the rate of profit and the capital-output (or capital-labor) ratio, such that when the rate of profit falls a more capital intensive method of production is chosen and vice versa. The assumption is, pre-

sumably, that a decrease in the rate of profit reflects the increased importance of a more capital intensive industry in its contribution to the aggregate output. The neoclassical one-sector model with a fixed saving ratio is therefore more or less self-contradictory (since the proportion of the consumption good to the investment good is constant by assumption). The problem of the choice of techniques can thus be appropriately analyzed only with a multisectoral model. It is the disaggregated aspects of growth equilibrium that now appear to cast doubt on the above neoclassical postulate.

Following Mathur, consider a closed von Neumann model, in which each sector has available a finite number of alternative processes. Thus, the economy can choose one technique (i.e., a set of processes adopted by the various sectors) among a whole feasible spectrum of techniques. Given a rate of profit, a vector of equilibrium prices (in terms of wage units) can be computed for each technique. The technique which gives the lowest prices for all goods (i.e., the highest real wage) is then termed optimal for the specified rate of profit. The dual to this problem states that, if the rate of growth is equal to the rate of profit (a Golden Rule condition), then the technique that minimizes prices also provides the highest consumption per capita. Having found the optimal technique corresponding to each rate of profit, one can then find the direction of the change in the capital-output ratio (in value terms) as the rate of profit varies. It turns out that no a priori hypothesis about the direction of change of this ratio can be justified: for some stretch of changes in rate of profit the ratio may fall, for some other stretch it may rise. The possibility of such non-monotonic behavior had been well known to Wicksell, and has since been proved by various writers such as Morishima (whose work, surprisingly, is not cited by Mathur). It got additional support in the recent discussion of the "reswitching problem" (*Quart. Jour. Econ.*, November 1966). The recurrence of optimal techniques at different rates of profit, if nothing else, is a definite indication that the assumption of opposite movement of the rate of profit and the capital-output ratio cannot, in general, be accepted.

Thus, for movements from one steady state to another, no pure technological ordering of techniques, as by the capital-output ratio, is possible. A much more interesting problem now comes up: starting at a nonsteady state situation, what is a typical optimum path for an economy in terms of the techniques chosen at each step in the growth process? For example, would it ever be optimal for an economy to readopt a technique which has once been used but has been discarded later in favor of a "more mechanized" (to use the term of Mrs. Robinson) technique? If some techniques are incapable, given the available resources, of employing all the labor force, is it optimal to adopt immediately a technique which provides employment to all, even if this occurs at the expense of a smaller output at the first stages of development? Or maybe the economy should adopt immediately the technique that will ultimately be optimal in Golden Age equilibrium and disregard any short-run considerations? These are very complicated problems and Mathur has made a brave effort to suggest possible solutions. This reviewer found his discussion of "strategy dams," "strategy corn" or "strategy tractors" very suggestive, but this method of analysis simply can not provide complete and rigorous answers.

A question such as under what conditions it is optimal to satisfy the terminal conditions with exact equalities rather than overfulfillment of the desired goals (whether capital can be costlessly discarded or not) can be solved only by dynamic programming methods of the kind used recently by T. N. Srinivasan, M. Kurz, D. Cass, and others.

Part VI of the book deals with the effects of neutral and biased technical progress on the choice of techniques. Economic obsolescence of capital equipment due to technical progress is introduced (The Upgrading Model) in a fashion reminiscent of the neoclassical vintage models. The book concludes with a disconnected mathematical appendix and some notes attacking neoclassicism "and all that."

I would like to comment finally that Mathur devotes little space to the price relations implicit in his model nor does he consider seriously the question of whether his planned economy could, partly at least, make use of a market system.

EYTAN SHESHINSKI

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The Wage-Price-Productivity Nexus. By RONALD G. BODKIN. Philadelphia: University of Pennsylvania Press, 1966. Pp. 302. \$8.00.

In the midst of affluence we often find great poverty. The publication in book form of Ph.D. theses is a good case in point. Certainly the quality of dissertations has risen greatly in recent years. But the plethora of immature books spawned by the publishing revolution and the "publish or perish" syndrome can only be lamented by a profession already hard-pressed by an ever increasing avalanche of "scholarly" journals. Good theses, like Bodkin's, might make good articles, but they are not necessarily good books.

Typically, the slightly expanded and revised Ph.D. thesis book begins with a more or less extensive, but disorganized, and nonpenetrating review of the literature. The cacogenesis then moves on to a scanty theoretical model which is manipulated to produce a few interesting artifacts. If the study is an exercise in theory, subsequent chapters torture the model even further. If it is empirically oriented, the model is simplified and modified under the duress of a barrage of data and statistical findings. In either case, the final product bears little relationship to the earlier rationale. And to complete the anamorphosis, few individuals, in today's policy oriented climate, can resist the temptation to draw some unsupported conclusions for government action.

So much for the generalities. Now let us turn to the particulars. Bodkin's *Nexus* begins with a jumbled survey of post-1950-pre-1963 literature on the wage-price-productivity subject. Chapter 2 is a reprint of his *Metroeconomica* (Volume XV, No. 2) article with R. J. Ball. It describes a static income and price determination model with much emphasis (later ignored) on the role of the money supply, real balance effects, and the degree of monopoly power existing in the economy. Money wage rates here are a function of the levels of *employment* and consumer prices.

In Chapters 3 and 4, Bodkin develops and estimates a wage adjustment equation. Initially, the annual change in money wage rates (dollars per hour)

over the preceding year at time t are hypothesized to depend linearly on the number of *unemployed* (thousands of men) in year t and the absolute difference in the consumer price index between year t and $t - 1$. Absolute levels are used because preliminary runs (estimated for 1900-57) in percentage form had lower explained variances. Lags on the independent variables were determined by cross correlogram type analyses (sometimes from plots of the series). Even in the absolute form, the coefficient on unemployment only became significantly negative when a time trend variable was added. Bodkin justifies the latter's inclusion on two grounds: (1) either as a statistical consequence arising from stating the dependent variable in absolute rather than percentage terms; or (2) as a representation of wage push "as could occur from a growth in the membership and power of trade unions."

Some other ideas were tried. The first difference in unemployment, the ratio of corporate profits to net worth, a split of the consumer price index into positive and negative components (a weak test of irreversibility), and the change in output per manhour all were introduced, singly or in combination, as additional explanatory variables. None was successful; either they were not statistically significant themselves or they were collinear with other terms (especially unemployment).

Price equations were then estimated (on annual data, 1914-57) in similar fashion (in Chapter 4). The level of the wholesale price index for finished goods, after a series of bivariate tests, was made a linear function of manufacturing wage costs per constant dollar of output, the wholesale price index of crude materials for further processing, a time trend (which "may reflect savings in raw materials"), the percentage deviation of real output from its moving average (a proxy for "excess demand") and two dummies to correct for 1943-46 and 1951-53 wartime conditions—all variables were for the current year. An attempt was made to test irreversibility of price changes by splitting the demand term and including a previous peak unit labor cost or wage rate variable. Only the last was statistically significant after the equations were re-estimated using an Orcutt transformation (with assumed alternative values of ρ) to reduce serial correlation of residuals. Bodkin interprets his results as an affirmation of "the view that demand factors are of secondary importance in the determination of the level of final prices [which] has also been supported in studies by Dow, Yordon, Yance, Dicks-Mireaux, and Kuh."

Chapter 6 of the volume goes on to a shallow analysis of the average productivity of labor, (A), defined as the real gross output of the private domestic economy divided by total manhours of labor input. The annual rate of growth of this quantity (i.e., $\log A$) is related to time, the square of time (t^2) and the current rate of unemployment for 1900-57 data. The t^2 term is found not to be statistically significant after adjustment for autocorrelation of estimated residuals, thereby denying an acceleration of productivity growth. The unemployment variable is to represent cyclical productivity influences; several forms of an average weekly hours term were inserted as alternatives, but they gave inferior results. The chapter closes with a presentation of two-stage least squares estimates of the earlier equations, treating them as an endogenous system, with time and the war dummies as exogenous. The first

stage regressions use 10 additional exogenous variables. Not surprisingly, the standard errors of a number of the coefficients rise sufficiently to cast doubt on their statistical significance.

Some further reservations are given by Bodkin in the succeeding chapter where he draws heavily on the arguments of other writers who aver that inflation is an aggregation phenomenon. The principal strand is that of Charles Schultze's U.S. Joint Economic Committee study paper (1959), wherein inflation is caused primarily by price ratchets and demand asymmetries. Following Lipsey, a similar rationale is applied to labor markets. But this is immediately rejected by Bodkin on the grounds that his own wage-unemployment function is linear and Lipsey's nonlinear model is relatively untested. Bodkin goes on to hypothesize that the apparent nonlinearities are due to expectations of future tightness or looseness in labor market conditions. This and the sectoral phenomena then lead him to propose a "guideposts" policy very nearly identical to that expounded by the Council of Economic Advisers in its 1962 report (Bodkin says he first wrote this conclusion in 1961).

The final chapter contains a summary of earlier conclusions and a linear tradeoff relationship between the rate of change of prices and the level of unemployment. Bodkin finds, depending upon the wage function and method of estimation selected (single equation or two-stage least squares), that a 3 per cent "frictional" rate of unemployment in 1960 would lead to a predicted rate of increase of the price level of between 1 and 2 per cent. Alternatively, the rate of unemployment "required" for price level stability (i.e., $\Delta p = 0$) is 18.8 per cent.

In contrast, George L. Perry's recent study (*Unemployment, Money Wage Rates and Inflation*, Cambridge, 1966), with equations fitted to U.S. postwar quarterly data (1949: 1-1960:3), estimates that with 1960 $\Delta p = 0$ and a profit rate on stockholders' equity of 10 per cent, the rate of unemployment would be about 5 per cent. Perry's analysis is cast in percentage form, includes level and change in rate of profits variables, and has the *rate* of unemployment entering in nonlinear fashion.

Bodkin's study must be faulted on several grounds. First, it cannot be presumed that the structure of the economy remained unchanged from 1900 through 1957. While the included time trend variables also may capture the evolving structural change, they should not cavalierly be accepted and rationalized on other a priori grounds. At a minimum, an analysis of covariance (Chow test) splitting the period into subintervals would have been in order. Second, a sounder basis should have been given for the forms of the equations and the inclusion and exclusion of variables. What was done smacks too much of a "fishing" expedition to maximize explained variance. One would like an a priori profit and utility maximization model, constraints, and expected coefficient magnitudes more soundly grounded in fundamental theory. Third, since Bodkin himself raises the issue of disaggregation, at a minimum, wage and price behavior should have been analyzed for a number of industry divisions.

Bodkin's own modest view is that "The present author is satisfied if this work serves to advance, in some small degree, our understanding of wage and price

level relationships." It does that—but the contribution is small. Nevertheless, it should be surveyed by specialists working in the area and those with a general interest in the subject.

GARY FROMM

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Theories of Economic Development and Growth. By Y. S. BRENNER. New York: Frederick A. Praeger, 1966. Pp. viii, 285. \$7.50.

In this volume the author purports to summarize all theories of development and growth at an elementary level of discussion, the topics ranging from the ancient Mercantilists and Physiocrats to modern growth theory. In addition he discusses some aspects of Soviet planning, as well as the "stage" theory of economic development. The volume is divided into broad, chronologically ordered sections dealing with the early and classical theories, the Marxian and neoclassical theories, modern and stage theories, Soviet planning, and development theories of underdeveloped economies.

In the first half of the volume, the author examines and summarizes the contributions of the following economists to the theory of growth: Adam Smith, Ricardo, Malthus, John Stuart Mill, Marx, Engels, Jevons, Menger, Edgeworth, Marshall, J. Clark, Walras, Pareto, Wicksteed, Irving Fisher, Wicksell and Pigou, in that order. The presentation of material is so organized that a good portion consists of direct quotations from the past economic classics. The reviewer feels that the volume is a brief summary of a course in the History of Economic Thought related to theories of development and growth, rather than *Theories of Economic Development and Growth*. This is particularly true in the discussion of neoclassical theories where a good portion of material is not "directly" related to the theory of development and growth. Nevertheless, these sections may prove to be useful to some economists whose primary interest is to acquaint themselves with the history of thought as related to economic growth.

The chapter devoted to contemporary theory is rather brief, considering the voluminous literature written in this field since the contributions of Harrod and Domar. The author discusses at great length the rudiments of the Harrod-Domar model. But the presentation is neither simpler nor clearer than the original articles of Harrod and Domar. The discussion of the neoclassical growth model is incredibly brief and ambiguous. For example, in discussing Solow's model, the author states, "Professor Solow developed a neoclassical system which may well be called an input adjustment model. To the familiar Keynesian accelerator he added two further assumptions which give the system a greater flexibility. First he assumed that at each point of time all labour and capital was [sic] readily available 'on the market' so to say, and secondly, that profits and wage rates are adjusting themselves automatically and without time interval. In other words he introduced a 'cost mechanism' which automatically adjusts labour and capital costs in a way which produced a capital output ratio to suit equilibrium growth requirement" (p. 190). The above quotation is the entire specification of Solow's model. Obviously, given a sufficiently fuzzy specification of the model any conclusions are possible. Aside

from its general ambiguity, another fault of the book is that nowhere can one find a reference to the problem of stability, nor is there any discussion of steady state and golden-rule paths.

Under the heading of "The Soviet Theories" the author gives a history of Soviet planning. Though interesting, no theoretical discussion of Soviet planning is made.

In the final section the book deals with growth problems of underdeveloped economies. The author points out numerous difficulties characterizing the growth problems of underdeveloped countries, and also suggests a few factors conducive to their development. But essentially no new idea is proposed.

In sum, the strength of the book lies in the first half of the volume, where the author presents a well-organized (though brief) summary of the history of economic thought related to economic growth.

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Economic History; Economic Development; National Economies

Planning Without Facts. By WOLFGANG F. STOLPER. Cambridge: Harvard University Press, 1966. Pp. xx, 348. \$7.95.

Development Planning in East Africa. By PAUL G. CLARK. Nairobi: East African Publishing House, 1965. Pp. vi, 154. \$3.00; 16s. East Africa.

The keynote of both these studies is operational feasibility. Each author is a clear eyed economic theorist, applying modern tools of analysis to economic development problems. Each worked in a country going through the early stages of reorganizing its economic activity to produce higher incomes. Both studies show how rough estimates for aggregate economic variables can be used to improve the complex decisions involved in managing scarce resources wisely.

One basic issue in economic development is whether the scanty data available are meaningful, and, *a fortiori*, whether the data appearing in a development plan can be anything more than wild guesses. Skepticism here reflects deeper questions concerning the sources of development initiative, the factors that alter economic performance, and the most effective ways to promote development-inducing uses of scarce resources. As a skeptical realist, Stolper makes a major contribution to informed reflection on these matters. In more compact form, the statistics and discussion in Clark's monograph provide further practical evidence of what careful analysis can achieve. Both studies should be on every planner's bookshelf.

Stolper uses his experience during 1960-1962 in Nigeria, where he helped prepare the 1962-68 national plan, as a basis for wide-ranging reflections. The facts that are denied a planner, in countries like Nigeria, are technological and social, relating to the speed and extent of changes actually or potentially under way around him. Stolper argues, first of all, that the planner should study intensively his unique surroundings, being wary of applying foreign

analogies or empty universals. He argues, secondly, that development planning should be shaped from the present a short way into the future, with frequent revisions, rather than being shaped from a future vision backward to the current scene. He calls this approach "optimizing as one goes along" (p. 4). A third precept of "planning without facts" is that the very lack of adequate technological and social knowledge means that decision-making should be decentralized, since only those in close touch with fragmentary evidence can evaluate it accurately.

Another major theme in Stolper's analysis is the great importance of choosing profitable investments and avoiding unprofitable ones. Profitable investments made now will generate new output from which added investments can be made later. If aggregate current realizable "social profitability" falls far short of "economic profitability," there will be a large current drain on the recurring budget. Similarly, Stolper argues that market-clearing prices for the use of scarce resources should be charged now, in order to ration their use. "The application of shadow prices leads to the substitution of one problem, the budget, for another one, an imperfect market" (p. 195).

The eight chapters of Stolper's book are preceded by Chief S. O. Adebó's thoughtful foreword and a modest author's preface. Chapter 1 outlines a conceptual framework and Chapter 2 sets forth the Nigerian background. The "general view of planning" presented in Chapter 3 shows how the need for operational feasibility constrains a planner's efforts. In further detail, Chapters 4 through 7 discuss national accounting, investment criteria, monetary problems, and government-business relations "from a planning standpoint." The 13-page concluding chapter is an eloquent plea that the tensions of rapid change not be made worse by plans that misuse resources and coerce the people. A 15-page appendix sets forth an input-output table for Nigeria in 1959-60, together with a few derived structural tables, the work of Nicholas G. Carter.

Clark's monograph is directed to government officials, private leaders, and university students in East Africa, all of whom are affected by (and many of whom will help to guide) the economize development programs of their countries. Because his study summarizes valuable empirical evidence for three economies, organized within a quantitative model, it will also provide valuable comparative evidence for development planners and advanced students everywhere. Chapter 1 is a brief essay describing the elements of comprehensive planning. Chapters 2 and 3 describe economic trends in Uganda, Tanzania, and Kenya, and outline their earlier economic plans. In Chapter 4, Clark presents a macroeconomic model covering six producing sectors, seven kinds of imports, two classes of exports, four forms of capital formation, four kinds of government taxes, and certain other variables appropriate to these three economies. In its structural relationships, the model contains five autonomous variables: the quantity of agricultural exports, the prices of those exports, the value of manufactured exports, import substitution in manufactured products, and central government current expenditures. The parameters of the model have base-period values which are pragmatically reconsidered when projections to a future period are made. In Chapter 5, the author uses the model to

show the implications of two projections for Uganda in 1981, one "moderate" and the other "ambitious." The same quantitative framework, fitted with different values, is used to explore the implications of 1970 targets for Kenya and Tanzania. Clark's concluding chapter offers a number of suggestions on plan implementation and observations on economic coordination among the three East African economies.

Both studies deserve careful scrutiny, even by those who may not agree with the details of Clark's model or the judgments behind Stolper's reflections. Neither author is dogmatic; their professional craftsmanship deserves respect whether or not it compels assent. Stolper's study is especially valuable for its thoughtful reflections, Clark's monograph for its operational data.

Stolper is convinced that plans which impose a high degree of austerity will be self-defeating, since they will erode morale, undermine productivity gains, distort resource use, and create political instability. He agrees with W. Arthur Lewis (see his *Development Planning: The Essentials of Economic Policy*, New York, 1966), that the absolute level of living should not be depressed, even during a "big push." This is a humane, Western view, responding to the situation of countries in West Africa. It also stems from a belief that there are at any one moment of time only a limited number of sensible outlets for additional savings.

However, a reviewer familiar with the Soviet and Chinese approach to economic development, and aware of the grimmer dimensions of development problems in India, cannot help wondering whether this sensible advice will fit the desperate urgencies that loom over many countries today. We can all agree that belts should not be tightened unnecessarily, as a result of wasteful resource use, but strenuous efforts may still be required to fend off economic and political disaster. "Strenuous, but sensible"—perhaps that should be the development adviser's slogan.

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The Economics of Underdeveloped Countries. By JAGDISH BHAGWATI. New York: McGraw-Hill, 1966. Pp. 254. \$4.95; paper, \$2.45.

This book by Jagdish Bhagwati is one of a specially commissioned, international series of books designed to present a broad contemporary treatment of particular topics which will serve as authoritative introductions for both student and general reader. With this goal Bhagwati has had outstanding success. Written simply and with astonishing clarity the book will furnish the new student an excellent foundation for further study in development and will give the general reader a sound understanding of this most pressing international issue.

Bhagwati uses a straightforward ordering of the book. Part I (The Incidence of Squalor) defines the problem of underdevelopment. Part II (The Economic and Social Structure) contains an incisive survey of both economic and noneconomic obstacles to development. In Part III (The Process of Transformation) Bhagwati outlines alternative development policies and pre-

sents an outstanding treatment of the planning framework. (If no other part of the book makes an assigned-reading list, this should.) Here the author treats planning from the inception of goals and targets through balancing and maintenance of consistency to plan completion. The final section, Part IV (The International Framework), incorporates the problem of development into a discussion of the international economy with its implications for aid, investment, and trade policies.

So much for the book as a general introduction to the field of economic development. There is another, more penetrating, and much more exciting side to this book which warrants the attention of everyone in the area of development. This is its searching appraisal of many ideas and theses which too often have tended to become part of the "conventional wisdom" of the developmental economist. If this is too extreme, then the economist bears responsibility for allowing these notions to become accepted as truths by the general public.

Here are some of the theses which are forced to run Bhagwati's gauntlet: (1) Wide differences in income inequality exist between countries. (2) The terms at which underdeveloped countries exchange their products for imports have been declining secularly. (3) Countries relying heavily on export earnings from a few primary commodities suffer consequently from sharp instability in their capacity to import their requirements. (4) The social institution of the joint-family is a significant barrier to economic development. (5) The caste system seriously impedes the growth of a modern industrial labor force.

Perhaps because Bhagwati represents a fortuitous blend of leading international economist and knowledgeable observer of Asian life, he has been able accurately to reappraise ideas too often taken for granted in the literature. Moreover, this rare blend also makes his analysis of such institutions as the joint-family, the caste system, and motivational aspects of eastern religions refreshingly original.

Another strong point of his work is its nondoctrinaire tone. Bhagwati attempts to draw from conflicting economic systems those aspects holding the most promise for the developing nations. Thus he sees an important role for public enterprise in a development program, yet urges strongly that detailed bureaucratic planning be minimized because of its tendency to lead to inefficiency and corruption.

In the final section of the book, "The International Framework," Bhagwati perceptively analyzes the standard questions on aid-giving: tied vs. un-tied aid, multilateral vs. bilateral aid (including an interesting account of consortia agreements) and the role of private foreign investment. With respect to the trade problems of the developing economies Bhagwati calls for a restructured GATT with major alterations in its policies toward: subsidization of exports, reciprocity, state trading, and preferential entry. Also included here are a discussion of the effective tariff-rate argument and a call to the industrial nations to remove nontariff barriers to the entry of the manufactured exports of the underdeveloped countries. An unfortunate omission in this section is failure to consider in any detail the vehicle of economic integration as an approach to development.

It is most fortunate that there seems to be a trend emerging in which economists working at the theoretical frontiers of their respective fields occasionally pause to condense, integrate, and simplify the body of contemporary literature. This is what Bhagwati has so effectively done. The same could be said about Harry Johnson's latest books. This keeps the lagging sector close enough to be still relevant and useful in policy formulation.

DONALD R. SHERK

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Agricultural Production and the Economic Development of Japan 1873-1922.

By JAMES I. NAKAMURA. Princeton: Princeton University Press, 1966. Pp. xxiii, 257. \$7.50.

This is an imaginative and stimulating book. It says something radically new about the tired theme of Japan's rapid economic growth. The book argues that Japan's growth rate during the period under consideration was very much lower than what has been accepted. It also argues that Japan's standard of living at the beginning of industrialization was very much higher than conventionally supposed. Nakamura arrives at these conclusions by examining the flaws and biases in the official statistics on agricultural production in Meiji Japan. The agricultural output of early Meiji years is adjusted upward by 80 per cent above the widely accepted Ohkawa estimate. The subsequent rate of growth of the agricultural output comes down to about one per cent per annum in contrast to 2.4 per cent in the Ohkawa estimate. If Nakamura were right, all the merry variations on the theme of Japan's *wirtschaftswunder* would amount to a gigantic hoax. One would suddenly be precipitated into a gloomy task of explaining why Japanese development was so slow. This new task, giving relief to iconoclastic impulses, would not be without its *schadenfreude*, but before indulging let us examine Nakamura's case critically.

Nakamura regards agricultural output as consisting of two components: paddy rice and dryland crops. For either component, output is an arithmetic product of arable land area and yield per unit of area. The underestimate of output can be traced to the understatement of area or the underreporting of yield. The former arises from concealment, misclassification, or undermeasurement of arable land. Both the understatement of arable land area and the underreporting of yield derived from the landowners' desire to minimize the land tax burden and the public authorities' difficulty in obtaining full cooperation from the landowners.

On the whole, Nakamura does a convincing job in the adjustment of arable land area for concealment and misclassification (Ch. 2). This adjustment affects land statistics prior to 1890. His adjustment for undermeasurement is less convincing but its effect is not too serious (Ch. 3). He applies uniform undermeasurement indices to paddy and dryland areas for the entire period. His method of adjusting for underreporting of yield is least convincing and may even be regarded as a little too audacious (Ch. 4). This is all the more serious, because the adjustment for underreporting of yield accounts for a pre-

dominant part of the difference between the Nakamura and Ohkawa estimates of agricultural production.

Nakamura rejects the whole series of paddy rice yield obtained from the official statistics except for the average yield for 1918-22. He infers a range of yields from 1.5 to 1.7 *koku* per *tan* (1 *koku* = 4.96 bushels; 1 *tan* 0.245 acres) for the 1870s from historical findings on agricultural progress during the Tokugawa period and a common belief among the officials about the level of yield during early Meiji years. He then calculates a range of rates of increase in yield between the assumed range of yields for the 1870s and a range of yields from 1.9 to 2.0 *koku* per *tan* for 1918-22 obtained from averaging the official figures for these years.

Among the various considerations that have led Nakamura to adopt that particular range of yields for the 1870s, the following two seem to be most important: (1) the land survey of 1592-96 placed the average paddy rice yield at 1.18 *koku* per *tan* and (2) land productivity during the Tokugawa period must have increased slowly but steadily (pp. 75-77). At the annual rate of increase equal to one-tenth of one per cent during the Tokugawa period, paddy rice yield could easily have risen to 1.6 *koku* per *tan* by 1875 (Table 4-6, p. 103). The official statistics put the yield for 1878-82 at 1.166 *koku* per *tan* which was lower than the yield of 300 years before (pp. 79-80). Clearly there was something drastically wrong with the official figures, claims Nakamura.

He may well be right. But anyone who has used the Japanese official statistics in one way or another would find it extremely difficult to cast them aside as easily as Nakamura does. Nakamura pitches the full weight of Tokugawa history as it has recently emerged from the works of historians like E. S. Crawcour and T. C. Smith, against the canons of modern empirical research based on mass data applied to Japan by economists like B. Johnston, K. Ohkawa, and H. Rosovsky. Nakamura thus corners his readers into a *Shigemori*-dilemma of divided loyalties: e.g., to honor history is to challenge economics. It may perhaps be Tokugawa versus Meiji, for preferences for one would surely affect the evaluation of the other.

One way to resolve this dilemma is to hunt up masses of unofficial, but hopefully unbiased, data from a wide range of households, communities and prefectures, compare them with the official statistics having to do with these areas, and construct an "underreporting index" to be used for the adjustment of the official statistics. The task is no doubt arduous and costly. But its payoff may be worth the cost. In fact, Saburo Yamada, whose agricultural output estimates are called in for scrutiny by Nakamura, may well have adopted this kind of approach (pp. 119-21). It is a pity that Yamada has not published his method in sufficient detail. Interestingly enough, Yamada's results come up with an intermediate position between the Ohkawa and Nakamura estimates. Yamada's belief that the official-yield reports can be adjusted upward is sensible enough. Surprisingly, Nakamura takes a dim view of this kind of approach (p. 121), denying in effect the possibility of adjusting the official data. Could it be that Nakamura for once was carried away by his enthusiasm over Tokugawa history and his delight in the elusive ingenuity of Meiji farmers?

Given the uncertainties that shroud the yield estimates and the whole series of agricultural output built upon them, little need be said at the moment about Nakamura's re-interpretation of Japan's modern economic growth (Ch. 7), except for registering an admiration and amazement as to the variety of problems and promises that arise on the heels of so fundamental a revision of agricultural output, and the efficiency of skill and the elegance of exposition with which Nakamura has conquered problems and fulfilled promises. In conclusion, there is no gainsaying that Nakamura has written a very important book which raises studies on Japan to a new level of sophistication.

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KOJI TAIRA

U.S. Aid to Taiwan: A Study of Foreign Aid, Self-Help, and Development.

By NEIL H. JACOBY. New York and London: Praeger, 1966. Pp. xviii, 364. \$8.50.

In the voluminous literature on economic development, systematic and rigorous study of the effects of U.S. economic assistance upon developing nations is only a recent phenomenon. Jacoby's book is certainly an urgently needed empirical study and is the most comprehensive yet published on this subject.

During the period 1951-65, Taiwan received a total of \$1,465 million in U.S. aid, amounting to about \$10 per capita per year and to 6.4 per cent of Taiwan's GNP over the entire aid period. Jacoby's major purpose is to examine the effects of such aid on the Taiwan economy. After presenting the scope and method of his study, he discusses the U.S. aid program in terms of its objectives, character, organization, and institutions; reviews Taiwan's economic, social, and political development and its military burden; and then devotes about one-third of the book to analyzing the relationships between aid and development. Appendices occupying about another third of the book provide abundant statistical data and three growth models of Taiwan's economy. It should be noted that Jacoby's study accepted, without evaluation, both the U.S. and the Republic of China's political and military policies and relied primarily upon their official statistics. Those "who must read as they run" are advised by Jacoby to turn immediately to the last chapter which summarizes the flaws and errors in the Taiwan aid program, reasons for success in Taiwan, and other lessons.

To evaluate the effects of aid on development, Jacoby developed seven criteria, e.g., target attainment criterion (actual growth versus Chinese government targets); and three simple models of the Harrod-Domar type, i.e., No-Aid, No-Military Burden, and Light-Aid Growth models. He admitted that each of his criteria and models has certain theoretical limitations and peculiar difficulties in application. Fortunately his seven criteria are more or less independent and all pointed toward the same conclusions.

It was found that "Aid more than doubled the annual rate of growth of Taiwan's GNP, quadrupled the annual growth of per capita GNP, and cut thirty years from the time needed to attain 1964 living standards" (p. 152). With aid, the Chinese government achieved economic independence in 1965,

three years ahead of the original goal set by the Agency for International Development (AID). Jacoby estimated that with aid but no-military burden, averaging 9 to 11 per cent of its GNP during 1951-65, "Taiwan's GNP might have expanded about 9.7 per cent a year—2.1 percentage points about the actual growth rate" (p. 124). The example of Taiwan shows, however, Jacoby believes, that a powerful military force and rapid economic growth in a semi-developed economy can be achieved simultaneously, provided that major military hardware is supplied by an external force.

One understanding achievement of Taiwan's development, Jacoby noted, is that the fruits of its progress were widely distributed among farmers, villagers, industrial workers, professional and civil service personnel, and business enterprisers. However, it is common knowledge that, despite some recent improvement, civil service salaries are still considered very low.

To Jacoby, it is a fallacious view that aid should have "no strings." He describes quite vividly how aid has been used to influence Chinese national policies (Ch. 10). The most important economic consequence was the creation of a booming private enterprise, which, by 1965, became the mainspring of Taiwan's economy.

U.S. aid also shaped political and social development in the areas of education, health, and public administration, admittedly with much less success. His discussion of policy influence in these areas, however, is disappointingly brief. More quantitative evidence is needed to substantiate some of his conclusions about aid allocation and selection of aid projects in Chapters 13-14. A typical conclusion reads, "Although the allocation of funds among sectors of the Republic of China's economy was close to optimal, choices of individual projects departed from the optimum by a material margin" (pp. 203-4). Neither "optimal" nor "material margin" is clearly defined. Such inadequacies are undoubtedly in part due to the lack of relevant data and to the difficulty of measuring the effects attributable to aid. But one's suspicion is also aroused that Jacoby is perhaps unaware of or reluctant to cite certain relevant literature. Several important studies, for example, A. F. Raper and E. S. Kirby's studies on Taiwan's social and rural development were not cited.

Throughout the book, he makes candid and penetrating observations on a number of controversial and vital issues. He gives more credit to the Japanese contributions to Taiwan's early development during her occupation from 1895-1945 than most Chinese are willing to admit (Ch. 6). He notes that political development has lagged behind the rapid economic and social development. "There was a measure of democracy—more than a facade, yet less than a genuine and full structure" (p. 113). He is much concerned about the problem of "brain drain"—an exodus of talented young people to foreign countries (Ch. 8 and p. 185).

In my judgment, Jacoby is quite successful in analyzing the effects of aid upon Taiwan's development. But the book is more than a case study. He is equally successful in drawing lessons of value for future aid programs. He also attempts to develop methods for aid evaluation, but with less success. His criteria and models are enlightening, but are built upon restrictive and static assumptions. These can be applied elsewhere only with great care and consider-

able modification. But his efforts are nevertheless an important step toward the development of a much needed proper theoretical framework for evaluating aid programs.

Considering the lack of such well-established theory and the many hazards in such pioneering analysis, Jacoby has, I believe, written an interesting book characterized by his skillful interpretations of factual materials.

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Ceylon: An Export Economy in Transition. By DONALD R. SNODGRASS. A publication of the Economic Growth Center, Yale University. Homewood, Ill.: Richard D. Irwin, 1966. Pp. xvii, 416. \$9.50.

In this volume, which is sponsored by the Country Analysis Program of the Yale Economic Growth Center, Dr. Snodgrass attempts to analyze the process of growth in Ceylon since her opening to the world trade system during the 1840s. His study is purported to show that economic changes of Ceylon are consistent with the "enclave" thesis *à la* Hans Singer, Raul Prebisch, and Jonathan Levin. "The growth of the estate sector and its continued prosperity, then, increased dividend levels for foreign shareholders and increased consumption levels within a growing enclave in Ceylon itself, but it did not materially raise levels of expenditure on Ceylon-produced goods and services, nor did it provide a surplus to be invested in other parts of Ceylon economy" (p. 70).

This pattern of growth (or non-growth) has prevailed, according to the author, during "period 1" (1840-1946), which is designated as the "classical or colonial period." Chapters 2, 3, and 4 are devoted to studying the growth pattern in this period. Here the author describes how British capital was introduced and Indian laborers were imported to combine with land in Ceylon. This resulted in the creation of a plantation type of "enclave system," growing coffee (now defunct), tea, rubber, and coconut to export. With qualified success, efforts are made to explain the reason for a weak intersectoral flow between the growing enclave sector and the nonexport sector (or the subsistence sector).

On the other hand, "period 2" (1946-1961) is characterized by the "revolt"—the independent government's effort to change the structure of the economy and thus to increase per capita income. Chapters 5, 6, 7, and 8 describe this period of "transitional, development-oriented or independent economy." The story is an unhappy one, however. The government effort has been frustrated because of "the high consumption and import propensities in the private sector, the sluggishness of public revenues, the shortcomings of public investment allocation, the failure to stimulate an adequate volume of private investment . . ." (p. 214). As a hindsight, the author diagnoses that "A higher growth rate could have been produced by converting more current expenditure to investment" (p. 214).

A few questions may be raised regarding the claim that Ceylon's experience is consistent with the enclave thesis. Other studies show the magnitude of the

export multiplier estimated at 2 to 2.5 for the period covering the 1920s to 1950s.¹ This is contrary to the implication of the enclave thesis wherein the marginal propensity to consume for domestically produced goods and services out of the export income is near zero. Further, Snodgrass' own words contradict the thesis: "Many of the services that were provided—electricity, roads, railways, and others—yielded substantial benefits to other parts of the economy and society as well as to the estates" (p. 63). To this list, education and health services must also be added. This evidence among others does not seem to support his claim.

According to the enclave thesis, imports consisted primarily of the "luxury imports." However, one finds that the "traditional goods" such as rice, fish, cotton goods, etc. composed as much as 51 per cent of total imports for 1929 (p. 61, Table 3-3). "Western style consumption goods" composed only 16.8 per cent. Thus, contrary to the enclave thesis, the size of the domestic market seems to have expanded rapidly. Obviously the domestic industries failed, in spite of the tariff protection, to increase their output to capture the rapidly increasing domestic market. The suggested explanation is: "the peasant economy created its own demand or, rather, its subsistence production was relatively unresponsive to market stimuli" (p. 67). But, Snodgrass himself found puzzling exceptions which indicate the farmers' "over-responsiveness."

In spite of the questions raised, this volume is a welcomed addition to the scanty stock of scholarly studies on the Ceylon economy. I concur with the author in hoping that this book will be interesting to "all three types of readers; those interested respectively in Ceylonese economic development, in the growth of export economies, and in the development process in general" (p. viii). The Appendix (160 pages) is impressive and useful. It contains a wealth of materials ranging from national income estimates of various types (1938-1960) to numbers of livestock (1871-1959). Reliability and the method of estimation are discussed in informative ways. The discussion of the development problems facing Ceylon today is illuminating.

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¹ W. Rasaputram, *Influence of Foreign Trade on the Level and Growth of National Income of Ceylon 1926-57*, Colombo: Central Bank of Ceylon, 1964, pp. 76 and 150; K. Tharmaratnam, "Propensity to Consume and the Multiplier Effect in Ceylon," *Ceylon Economist*, March 1956, pp. 84-90; and Youngil Lim, "Export Industries and Pattern of Economic Growth in Ceylon," unpublished Ph.D. dissertation, UCLA 1965, pp. 38-42.

Statistical Methods; Econometrics; Social Accounting

Input-Output Economics. By WASSILY LEONTIEF. New York: Oxford University Press, 1966. Pp. xii, 257. \$8.50.

At the beginning of his second essay in this volume, Professor Leontief claims that James Clerk Maxwell would have difficulty in keeping track of the discussion in modern meetings of physical societies, while John Stuart Mill would easily pick up the thread of argument at corresponding economics meetings. This is surely not so, largely because of the technical nature of economic

analysis stimulated by Leontief and others in modern quantitative economics. John Stuart Mill would have difficulty in following, without advance preparation, much of the material on input-output economics in the present volume. He would be completely at sea in modern discussions of more general econometric methods or applications and the advanced mathematical theories of programming, balanced growth, existence of equilibrium, stability of equilibrium, social accounting and other subjects with their own terminology, advanced equipment, and technical methodology.

Leontief has been a true leader in the transformation of economics into a quantitative science and gives good display of his accomplishments in the present volume of eleven previously published essays. Several come from *Scientific American*, others from *Harvard Business Review*, *Dun's Review*, *Proceedings of the American Philosophical Society*, and the *Review of Economics and Statistics*. The reader who expects to find new results in I-O analysis will be disappointed, but the now familiar material that is reprinted here is interesting and contains many surprises on a rereading.

The disadvantage of placing these collected essays together in a single volume is that much of the introductory material becomes repetitious. The reader must be taken, over and over again, through an elementary discussion of what constitutes I-O economics and the structure of a typical table.

Among the more well-known essays in this volume are the stimulating pieces on factor proportions in foreign trade and the economic effects of disarmament. On the re-reading as well as in the earlier reading, the famous paper on the American capital position has a sensational appearance. It is especially good to have this paper reprinted because it is not easily available in its original form. This is an important paper, but it distorts the picture by making calculations in U.S. technology alone. The succeeding chapter goes far towards giving some theoretical justification for the argument in terms of U.S. technology alone, but it is doubtful that Leontief has effectively dealt with the critique offered in 1954 by S. Valavanis and others.

The disarmament paper is also important, but the lack of a real and firm forecast about the possibility of absorbing slack demand generated in the armaments sector by a government-supported private sector gives the results an altogether too bold and optimistic tone. This points up the basic weakness of I-O methods of analysis; they are not adequate by themselves for making forecasts. I-O relationships deal with only part of the economy and do not give us a closed system of relationships. The title for Chapter 1, "Proposal for Better Economic Forecasting," seems in this respect to be misleading, for I-O methods by themselves are not suitable for use in forecasting. They are suitable for giving partial information on economic structure, especially technological structure, but they tell us little about final demand, price formation, monetary affairs or many other economic processes that would have to be considered in a more complete system that would be usable in forecasting. In the case of disarmament, the crucial question is the maintenance of overall demand. I-O methods throw hardly any light on this issue, and the secondary marginal effect of shifting people from military to civilian activities is of limited significance.

In the seventh essay, Leontief states that, "The Input-Output method is an adaptation of the neo-classical theory of general equilibrium to the empirical study of the quantitative interdependence between interrelated economic activities." This is a frequent overstatement found in the literature of I-O analysis. The I-O system and the Walrasian general equilibrium system simply are not the same. The I-O system could be used for a particular representation of the technology of a general equilibrium system, but so much of the functioning of the economy is left out of the I-O system that it is surely wrong to identify it with the general equilibrium system. It is just a partial system.

I-O methods of analysis are becoming important in development planning, both in centrally planned and market type economies. The chapter on the structure of development is especially useful as a way of making international comparisons among developing economies and in providing an expository aid to understanding the whole process of development.

The final chapter (written together with Alan Strout) makes a similar application at the regional level, for even in countries that are developed in an overall sense, there are important problems of regional development. This chapter shows how models of regional analysis might be constructed. It gives a clear indication of the immense data requirements that would be needed beyond the already large requirements of conventional I-O analysis.

For teaching purposes and for referring people with lay interests to an expository treatment of I-O methods, Leontief's new volume will be found to be extremely convenient. It is not a collection of new contributions to the subject, but, as ever, shows the inspirational abilities of the founder and leader in this area of research.

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A Statistical Model for the Greek Economy 1949-1959. By P. PAVLOPOULOS. Amsterdam: North Holland Publishing Co., 1966. Pp. xi, 333. \$12.50.

This book presents a macro model for the Greek economy based on data for the years 1949-1959. Four spheres of economic activity are described—those of private consumption, private fixed investment, imports, and changes in stocks of agricultural products—in terms of seventeen linear equations. Eight of the equations of the model are behavioral (consumption, investment in plant and equipment, investment in housing, imports, agricultural and nonagricultural income determination, and agricultural and consumer price formation); two are institutional (direct and indirect tax equations); and seven are definitional. The stated purposes of the model are to analyze the structure of the economy during the sample period, to forecast events, to prescribe economic policy—and, this reviewer would also add, to demonstrate in detail how one goes about constructing an econometric model (despite severe data availability and reliability constraints), and using it. The pedagogical goal is perfectly satisfied; the rest are satisfied to a degree that varies directly with their above stated rank.

Obvious data limitations account for most of the shortcomings of the model. There is lack of wage cost data, therefore a price-wage interaction relationship

is omitted. Employment data are not available and as a result the model incorporates neither production functions nor demand for labor equations. Only in the agricultural submodel appears a supply equation which involves agricultural capacity, prices (both proxied in a rough way), and the weather and which is formulated on the basis of a modification of Nerlove's model of adaptive expectations. Monetary policy, exchange rate policy and credit policy variables are omitted from the model, although in some instances only after experimentation with proxies for these variables gave poor results. Finally, the degree of aggregation of the model seriously circumscribes its usefulness as a tool for policymaking. Despite its impressive record of growth (5.5 per cent per year during the period of the model and higher thereafter) the Greek economy faces serious long-term structural problems, in contradistinction to short-term problems arising from fluctuations of economic activity due to pressures or inadequacies of effective demand. Yet, the supply side only indirectly enters the model. And detailed structural analysis at the sectoral level is not attempted. From this point of view, the Greek specialist might want to compare the Pavlopoulos model with the Suits model¹ and the Adelman-Chenery model²—both less sophisticated but more disaggregated models. (A highly sophisticated and disaggregated eight-sector model of the Greek economy has been constructed at the Center of Planning and Economic Research in Athens as a basis for the formulation of the *Draft for the Economic Development Plan, 1966-1970*.)

Pavlopoulos is keenly aware of the limitations of his data and of the resulting constraints to his model—and he is ingenious in trying to circumvent them. In Chapter 2, the specification of the model, he carefully discusses each equation presenting its theoretical underpinning and also the alternatives considered. One cannot help but be impressed (and also amused at times!) at the inventiveness that the author displays in his use of proxy variables in an attempt to make the most out of poor, incomplete, or dubious data! One may also suspect that an aftertaste of this use of proxy variables appears in the estimation of the model in terms of the covariance among the error terms of the structural equations. Chapter 3, which deals with estimation, is as methodical as Chapter 2. The author presents side-by-side the ordinary least squares and the two-stage least squares estimates; he presents the results for the alternative forms considered (he bases his choice of preferred forms on a priori notions about the derived estimates, on autocorrelation of residuals, on the significance of the regression coefficients and, lastly, on the power of otherwise similar equations to explain observations beyond the sample period); and he presents the time graphs for all the variables and the residuals. The results of the estimation of the structural equations are in general highly satisfactory.

In Chapter 4 the author presents the reduced form estimates and the impact multipliers. Although less satisfactory than the estimates of the structural parameters, these are generally acceptable—despite the absurdity of a negative

¹ D. B. Suits, *An Econometric Model of the Greek Economy*. Athens: Center of Economic Research, 1964.

² Irma Adelman and H. B. Chenery, "Foreign Aid and Economic Development: The Case of Greece," *Rev. Econ. Stat.*, Feb. 1966, 48, 1-19.

impact multiplier of exports on income and the qualms one might have about accepting at face value a multiplier of autonomous government expenditures on gross national product of 1.50, especially in view of a lower multiplier that is usually estimated for the U.S. economy, e.g., by the Klein-Goldberger model. Still, the model survives the battery of tests it is submitted to, including a test for structural stability (Ch. 5 and 7).

Pavlopoulos' main contribution to econometric theory comes in Chapter 6, where very methodically he tries to detect the weak links of his model. The best known approach to the sources of distortion of an econometric model is due to Goldberger³ who analyzed the correlations between the reduced form residuals of GNP and the reduced form residuals of various endogenous variables. Pavlopoulos convincingly demonstrates that a better criterion of choosing among alternative forms consists of the combination of the quantitative measure of the error terms of the structural equations, of their variance, and of the covariance between the error terms of the different equations. Application of this criterion to the model leads to the result that all structural equations, with the exception of the one for indirect taxes and the one for agricultural price formation, share the guilt, although the main contributors to the model's distortions are the agricultural supply and demand equations and the equation for imports. Finally, Chapter 7 presents the truncated multipliers derived from the model, and Chapter 8, as an overview, discusses the use of econometric models in the policymaking process.

Pavlopoulos' work may not have provided the answers to the problems that smite Greek policymakers and planners. Yet, it provides a systematic, skillful, and comprehensive step-by-step account of how an econometric model is constructed, tested, and put to use. This didactic feature alone is sufficient to merit the attention of the practitioners of the art. The author's contribution on the methodology of locating the weak links of an econometric model remains as a consumer's surplus for the reader—and might even find a niche for Chapter 6 in the reading lists of courses in Econometrics.

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³ A. S. Goldberger, *Impact Multipliers and Dynamic Properties of the Klein-Goldberger Model*. Amsterdam: North Holland Publishing Co., 1959, p. 70.

Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Banking in the Early Stages of Industrialization: A Study in Comparative Economic History. By RONDO CAMERON, with the collaboration of OLGA CRISP, HUGH T. PATRICK, AND RICHARD TILLY. New York: Oxford University Press, 1967. Pp. xv, 349. \$6.50.

Rondo Cameron, the impresario of this volume and the author of four of its seven country studies, concludes that private entrepreneurs have been (and still are) the driving force behind economic development, that instead of gran-

diose and monumental state-sponsored development projects "in general the state would do better to devote its own limited resources to creating the conditions in which private entrepreneurs flourish" (p. 320). And the same truth applies to the banking system: when freedom and competition are allowed, the banking system is quite likely to contribute greatly to economic development; when the state restricts banking, everyone usually suffers.

These conclusions come from Cameron's studies of banking and early industrialization in England (1750-1844), Scotland (1750-1845), France (1800-1870), and Belgium (1800-1875); and from similar studies by Richard Tilly for Germany (1815-1870), Olga Crisp for Russia (1860-1914), and Hugh T. Patrick for Japan (1868-1914). They are all interesting and competent studies, and apparently they were all done from a common outline, which compelled the authors to discuss similar topics and which now enables the reader to make some meaningful comparisons of the various countries' experiences. This outline contains a background discussion of the country's financial structure and evolution, a quantitative analysis of the role of banks in credit extension, especially in industrial finance, and in note issue and deposit creation. Each study concludes with an assessment of the impact made by the banking system on the country's economic development.

Banking seems to have played an important role in the early industrialization of Scotland, Belgium, and Japan; it played a modest role in England, perhaps even less in Germany and Russia; and it may have held back growth in France. According to Cameron, Scotland had a superior banking system because there was freedom of note issue, early development and widespread use of bank deposits, and the early establishment of branch banking, all within the context of intense banking competition and free enterprise. Cameron thinks that France had the worst banking system, brought on by too many artificial and unnecessary restrictions. "The root cause of these deficiencies lay in the monopolistic position of the Bank of France and in the restricted vision and inflexible attitudes of the men who controlled its destinies" (p. 127). Cameron's low opinion of officials is not confined to the French, for in the final chapter we find him saying: "The kinds of men to be found in the service of the state—and, more importantly, the kinds of incentives that impel them—are not necessarily those most conducive to innovation and economic growth" (p. 320). So, Scotland and her private entrepreneurs are at the top, France and her government officials are at the bottom. England lies somewhere in between. Her growth, as Cameron relates it, was held back at almost every point by wrong decisions of the authorities, but these very obstacles "stimulated the private sector to introduce the financial innovations necessary for realization of the full benefits of the technical innovations in industry" (p. 59). Hence, in this case, private entrepreneurs were clever enough to circumvent ill-considered and foolish government policies. However, Belgium seems to present a puzzle to Cameron, because her banking system, geared as it was to industrial banking, apparently contributed importantly to growth, despite strong government interference and a lack of banking competition. What's the answer? "The Belgian experience would seem to indicate that different institutional arrangements can produce equally favorable results. . ." (pp. 149-50).

In Germany, as Tilly shows, the banking system did not contribute much to growth before 1840. After that, bankers became more active in organizing new enterprises and in obtaining short- and long-term funds for them. They had to carry on this activity, however, in the face of heavy restrictions—imposed especially by the Prussian government on note issue, credit creation, etc. The Prussian Bank's monopoly of note issue and its other restrictive practices "did not meet the range of needs that a competitive, private controlled system of banks of issue might have" (p. 180). But, once again as in England, astute private entrepreneurs got around these barriers, this time by creating many types of money substitutes related to their promotional and investment banking activities. "One might therefore conclude that the Prussian government's policies contributed—albeit unwittingly—to an institutional development which was on other grounds congenial to German development needs" (p. 182).

Russian banking was under the strong influence of government—paternalistic supervision of joint-stock commercial banks, monopoly of note issue, direct participation in lending, etc. The poor performance of the economy up to 1890, according to Cameron, was owing partly to such official intervention, which in this case was not circumvented by the private businessmen because of their "backwardness and sluggishness . . . itself in part a product of a long tradition of paternalism . . ." (p. 294). The banking system assumed a more active role after 1890, as it increasingly provided long-term capital to industry and engaged in investment banking. Nevertheless, as Olga Crisp suggests, a great deal of the saving for industrial investment came from the state and from foreigners; the banking system apparently did not supply a major portion of the saving.

In Japanese banking development, the role of government was also important; according to Patrick: "it was vigorous in establishing a new and modern financial system" (p. 241). However, at the same time Patrick warns us that "The role of the government should not be overemphasized . . ." because private enterprise after all was mainly responsible for the development of banking and of the economy. He stresses that "an extremely important feature dominating governmental policy throughout this period was its explicit advocacy of a private, competitive, laissez-faire banking system" (p. 249). Cameron takes up this theme in his concluding chapter when he says that, while both the governments of Japan and Russia intervened strongly in the economy, the Japanese entrepreneurs "responded far more vigorously to their opportunities to establish privately owned banking companies" (p. 295). Whether the impetus came from the government or from profit-seeking individuals, there seems little question but that the Japanese banking system contributed a great deal to the industrialization of the country, for it seems to have attracted a large amount of individual saving and to have loaned these funds for industrial purposes. But, as Patrick makes clear, the banking system did not do everything; for at least part of the period, foreign investment was very important, and a great amount of the banking funds came from government deposits through taxes.

With the exception of Japan, the output growth rates of these countries

during their industrialization drives are not at all impressive; they range from $1\frac{1}{2}$ to 3 per cent per annum. And even Japan's average progress over this period of 4 per cent is nothing to write home about. The fact is that even these successful countries did not produce high levels of saving and investment, either through their banking systems as intermediaries or by any other means.

The underdeveloped countries in the present-day world that have moved ahead most rapidly have generated saving and investment levels (relative to national incomes) two to four times higher than those recorded by Cameron and his associates; and today's successful countries (e.g. Taiwan, Israel, Venezuela, and most of the Communist countries) have produced output growth rates many times higher than those established by their predecessors. By and large, they have not accomplished these records by heavy dependence on their banking systems as financial intermediaries between savers and investors, but rather by the utilization of other saving-investment techniques—taxation, foreign aid, self-finance by private business firms, and profits of state enterprises. Many socialist countries, for example, in recent decades have generated very high rates of saving and investment through taxation and state-enterprise profits, assigning only insignificant roles to their banking systems as intermediaries. Other underdeveloped countries have shot ahead, despite weak financial systems, through heavy doses of foreign aid.

Thus, recent experience strongly suggests that banking systems as intermediaries are not highly essential to the growth process. The evidence presented by Cameron and his associates suggests that banking, whether it developed competitively or monopolistically, did not generate high levels of saving and investment in "the old days" and in fact produced rather unimpressive results. While there is as yet no definite proof that banking systems as intermediaries could not perform well if properly utilized, the evidence against them is mounting up. The Japanese experience, both in the early and later periods, may offer counter evidence—but, then, it always does.

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Public Finance

Federal Estate and Gift Taxes. By CARL S. SHOUP. Washington: The Brookings Institution, 1966. Pp. xvi, 253. \$6.75; paper, \$2.50.

Trusts and Estate Taxation. By GERALD R. JANTSCHER. Washington: The Brookings Institution, 1967. Pp. xv, 211. \$6.75; paper, \$2.50.

Economic Behavior of the Affluent. By ROBIN BARLOW, HARVEY E. BRAZER, AND JAMES N. MORGAN. Washington: The Brookings Institution, 1966. Pp. xiv, 285. \$6.75; paper, \$2.50.

These three monographs provide information about the behavior of wealthy people, how they respond to taxation, especially the federal estate tax, and about methods used to insulate property by the use of trusts and other legal devices, both from the tax collector and from its unrestricted enjoyment by living people. For public finance scholars, parts of each monograph are re-

quired reading; for other economists, the Barlow, *et al.* study, based on the Michigan Survey interview technique, is likely to be of greatest interest. The chapters in the Shoup and the Jantscher studies, based on a Treasury sample of decedents, provide a variety of illuminating information useful to anyone interested in the facts of life.

C. S. Shoup, a practiced hand at tax questions, analyzes the federal estate and gift taxes, giving much attention to gifts prior to death, so-called "generation-skipping devices," transfers between relatives, such as between spouses, parents to children, etc., and sketches a plan for revision of the federal estate tax. Appendices are contributed by Robert Anthoine, C. Lowell Harriss, Seymour Fiekowsky, and by Richard Bird, G. R. Jantscher, and Shoup himself.

The federal estate tax had a yield in 1964 of \$2.4 billion, an amount equal to about 2.3 per cent of federal tax receipts. As a revenue device, the estate tax is a failure. Why, then, continue federal death taxes? Shoup does not explore this question. The frame of reference of his study, and of the Brookings Conference on death taxation as reported by him, is the continuation of these taxes in substantially their present form with, hopefully, integration of the gift and estate taxes—the supposed great reform to be supported by all right-thinking people. This stance merely assures that a type of tax with no sensible rationale shall be continued.

Estate taxation generates a tax liability that depends on the size of the net estate and, apart from the marital deduction, the liability is independent of distribution of the estate. If wealthy decedents generally followed the religious doctrine that their estates were to be distributed to the poor, would tax experts advise governments to impose estate taxes? The simple, straightforward tax treatment of transfers at death or before death is inclusion of such gains in the income tax base of recipients, accompanied by effective averaging. If this were done, the tax revenue generated would become substantial. For example, if the total of such transfers amount to \$50 billion per year, and unfortunately Shoup does not provide an estimate of such transfers, the yield would be \$12.5 billion at the modest tax rate of 25 per cent. Shoup's own sketchy, proposed "reform" may be an improvement over the present law, but remains without clear justification in any known canons of taxation.

The Jantscher study is about half legal and half statistical and both parts are carried through with meticulous concern for accuracy. The social consequences of the use of trusts are found to be serious; the dead hand can and does place substantial amounts of property in legal arrangements that incidentally reduce tax liabilities and, what is more important, restrict the freedom of the living in the enjoyment of property.

Professor Jantscher combines a firm knowledge of the law, statistical sophistication, and clear mathematical and literary exposition to produce a first-rate work on a difficult subject. Thanks to a Treasury sample of the tax returns in 1957 and 1959, the actual practices of decedents can be studied in detail. Among the highlights is the perhaps comforting fact that more property is left outright than in trust; less comforting is the fact that trusts are used more frequently and for longer "life-expectancies" by very wealthy decedents than by others.

Jantscher explores various methods of treating transfers of interests in trusts, finding the British practice too easily evaded. His own proposal, described as a "backward-looking" approach, calls for computing the interest on a trust when it reaches its maximum present value. Assuming that the House Ways and Means Committee members could be instructed in the subtleties of the plan, and this one may doubt, its chief effect would be to discourage the formation of trusts.

If, as a matter of social policy, trusts and similar legal devices are to be discouraged, an alternative is a regulatory tax to be imposed on a trust device at a rate on the positive difference between the market value of the assets placed in trust and the present value of the interests of *living* beneficiaries in the trust. Where the trust devices "destroy" no present value to living beneficiaries, illustrated by a trust formed for legally incompetent persons, the tax base is zero. Where, however, the trust effectively limits the enjoyment of property by living beneficiaries, the dead hand taxes the beneficiaries. The government, in this event, could place a genuine tax on the dead-hand tax at a high rate, say 80 per cent, to discourage such antisocial practices. The gains of beneficiaries would be treated as income and taxed under the income tax. New trusts could be expected to dwindle fast if this plan were installed.

The Barlow-Brazer-Morgan monograph is based on data obtained in 1964 from interviewing a sample of 957 persons, whose incomes were \$10,000 or more in 1961. The sampling was roughly proportional to income and the questions asked were mainly designed to elicit information about asset management and work habits. Respondents give the impression of being hard-working, conservative people who let tax considerations influence their behavior slightly or not at all. Estate planners do not get much of their business; like other mortals, they vacillate—a fact that incidentally accounts for much of that \$2.4 billion yield of the federal estate taxes. On the work-leisure choice, they work on the average of 48 hours per week, 50 weeks per year. The interviewers did not ask about the three-martini lunch, and one wonders who are the customers of summer cruises and round-the-world golfing vacations. Rather mysteriously, those whose marginal tax bracket was in the neighborhood of 50-60 per cent, but not at higher percentages, reported that taxes reduced the hours they worked. Quantitatively, the "disincentive" effect of marginal income-tax rates appears to be trivial—the usual conclusion of empirical work in this area.

Subject to the inherent limitations of the interview method, this study adds substantially to our knowledge of the behavior of the affluent. It serves as a corrective to the idea that wealthy people spend much of their waking hours arranging their affairs to avoid taxation.

There is a lesson for economic theory in these studies. The time-preference doctrine continues to dominate what is called "capital theory" and theoretical pieces starting from the premise that people maximize consumption over their lifetimes or some successor's lifetime continue to use up much valuable journal space.

This view does not fit the behavior of wealthy Americans. As Seymour Fiekowsky in Appendix F of the Shoup minograph aptly states: ". . . these

wealth holders have a persistent affinity for assets. Their estates do not 'peak' at some age between 50 and 60 and then decline. Rather their estates continue to grow even beyond the age of 80!" (p. 229). The time-preference theory may be appropriate for aboriginals and even, perhaps, for British working classes of the 19th Century. Americans, however, like assets and this desire, unlike the desire for consumption, seems to be insatiable.

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Intergovernmental Fiscal Relations in the United States. By GEORGE F. BREAK. Washington: The Brookings Institution, 1967. Pp. xv, 273. \$6.75; paper \$2.50.

The fiscal crisis of state and local governments in the United States constitutes one of the major challenges to policy makers in this country. Rapid increases in the school-age population and growing congestion and pollution problems in urban areas have placed great pressures on state and local governments to expand both the range and levels of expenditures on public programs. To meet this challenge, the states and localities must have increased access to sources of funds. And this is the heart of the problem—state and local governments are subject to serious constraints in tax policy, constraints which suggest that it is unlikely that the necessary revenues will be forthcoming from state-local tax structures. The response of policy makers to this dilemma will in large part determine the future of federalism in this country.

Break's study of this problem begins with a survey of the projections of state-local spending and revenues for 1970. Projections of this kind are a tricky business; they are highly sensitive to the assumptions adopted in the study. Thus, Break settles for interval estimates of 1970 expenditures and tax receipts of \$100-120 billion and \$64-70 billion respectively. Much of this projected gap may be filled by a growth in receipts from user fees, increases in existing federal grant programs, and by a rise in state-local indebtedness. However, there is "... at least an equal possibility that the pressures will soon be too great to be met in conventional ways; it is this prospect that brought about this study."

In the next three chapters, Break treats the problems of tax coordination, functional grants-in-aid, and unconditional grants to states and localities. Following traditional theory, Break argues that the federal government should limit functional grants to those programs where substantial spillover benefits and costs exist. In principle, such grants should be open-end, matching grants, where the share of the federal government equals the ratio of spillover benefits to total benefits. Furthermore, as the grantor, the federal government should exercise some control over the character of the programs involving these funds. The federal government can then supplement these tied grants with unconditional grants. The latter would serve two basic purposes. First, they would provide added funds for all state and local governments. Because of the pressures resulting from the high intranational mobility of economic units, state and local governments are reluctant to increase tax rates or to adopt new taxes which might adversely affect new investment or migration into the state

or locality. Thus, the cost of a dollar of taxes to a state or municipality may be more than just the loss of a dollar of disposable income to its residents. As a consequence, states and localities may fail to provide important public services where the benefits exceed the real opportunity costs of resources. Unconditional grants could thus make available funds for genuinely desirable programs. Second, the federal government could employ unconditional grants for equalizing purposes by making relatively larger grants to the poorer states (and possibly localities). On this point, Break discusses a number of alternative grant formulas and presents calculations indicating the degree of equalization implied by each.

On the grant problem, I am a bit uneasy about Break's unqualified application of the Pigovian theory of functional grants to the question of intergovernmental fiscal problems. Economists have used the theory of optimizing grants to tackle the problem of spillover costs and benefits generated by individuals and firms. But it is somewhat tenuous, as Anthony Downs and others have argued, to assume that governments act to maximize net social benefits in the economic sense. A group of vote-maximizing politicians, as Downs shows, may behave in a way which diverges significantly from optimality in economic terms. Thus, we should, it seems to me, be developing a theory of intergovernmental grants which is more closely tied with what we know or can infer about social decision-making processes.

Break devotes his fifth chapter to metropolitan fiscal problems. It is here that the pressures of growing needs and limitations on revenues are most serious. Per capita expenditures in 1962 were \$234 within Standard Metropolitan Statistical Areas (SMSA's) as compared to \$180 outside the SMSA's. The gap would in all likelihood be even more pronounced if the urban areas could generate the funds they require to meet the bewildering array of problems which confront them. However, the mobility of resources at urban levels places even greater constraints on taxing powers than at the state level. In this chapter, Break presses for three types of reform: the reorganization of metropolitan governmental systems, the adoption of systems analysis for program evaluation, and the restructuring of metropolitan revenue systems. The organization of urban government is a problem of incredible complexity. Break's brief discussion is of some help, but the problem is clearly at least as much a political as an economic one. On the tax issue, Break's proposals for a rationalization of assessment procedures and a reduction in exempted property would definitely strengthen municipal tax systems. However, I cannot share his enthusiasm for "... the conversion of the property tax into a tax on all wealth holders levied annually on the market value of their equity interest in assets of all kinds" (p. 213). In spite of its desirable equity characteristics, a tax on mobile forms of wealth is precisely the kind of tax urban areas cannot adopt—it serves simply to chase much of the tax base away.

The great merit of this book is that it treats in a comprehensive and systematic way the whole area of federal-state-local fiscal relations in the United States. Break states the problems and examines the various proposals which have been advanced to deal with them. For this reason, it is undoubtedly the best available introduction to the subject. The theorist in public finance will,

however, find little new in this book. But it is nevertheless a useful contribution to the discussion of one of the major policy issues of the decade.

WALLACE E. OATES

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International Economics

Discrimination in International Trade, The Policy Issues 1945-65. By GARDNER PATTERSON. Princeton: Princeton University Press, 1966. Pp. xiv, 414. \$10.00.

A coherent approach to the chronicling and analysis of the complex evolution of international trading relationships since World War II can be, and has been, attempted on the basis of a variety of organizing principles: dollar shortage, European integration, Britain's efforts to enter Europe, the new system of multilateral commercial diplomacy embodied in the General Agreement on Tariffs and Trade, the problem of promoting development through trade. In choosing the purposes and results of policies of trade discrimination among supplying nations as his point of reference for this study, Gardner Patterson has selected an organizing principle of surprising potency in rendering a mass of heterogeneous detail amenable to logical and comprehensible exposition. The detail itself comes largely from the invaluable records of GATT, supplemented by personal discussion with responsible officials of the various national and international organizations concerned. Patterson's skillful ordering and review of it provides a book that will be useful at more than one level: the reader may either skim it to obtain a perspective on contemporary trade policy issues, or consult it as a reference on the major policy issues raised in particular negotiating incidents, or mine it for comparative information on particular policy issues as they have arisen in various contexts (the book is carefully indexed).

The first chapter of the book defines the territory, and explains the reasons for the primacy of the principle of nondiscrimination in the planning of the postwar international trading system. Chapter 2 then examines immediate postwar experience with the main exception allowed by the planners, discrimination for balance of payments reasons. The central theme is that while such discrimination yielded immediate benefits, given national commitments to full employment and fixed exchange rates, the benefits eroded as the European economies recovered and dollar shortage disappeared, while the costs in terms of "incidental protection" and other effects mounted. The author gives considerable credit to the new international institutions for the rapid unravelling of this discrimination, once the desirability of ending it had become apparent. Chapter 3, on the temporary regional arrangements of the dollar shortage period—the European Payments Union and the OEEC Trade Liberalization Scheme—develops the same theme of mounting costs and falling benefits, and a swing of opinion and policy back towards the virtues of nondiscrimination.

The developments recounted in Chapters 2 and 3 could be regarded as a laggard return to the wisdom accumulated by interwar experience and em-

bodied in the planning of postwar economic reconstruction. Since the mid-1950s, however, the trend has been strongly the other way, with the exception of the Kennedy Round, itself an effort to stop the trend, or at least contain it. Chapters 4 and 5 deal with the establishment of and the trade policy issues raised by the European Coal and Steel Community, the European Economic Community, and the European Free Trade Association (and, briefly, the Latin American Free Trade Area). The author provides particularly valuable discussions of the unresolved issue of how balance of payments restrictions on imports should be applied when a member of such arrangements gets into difficulties, and of the serious problems raised by the association of overseas territories with the European Common Market, problems which excited little public attention at the time but planted the seeds of future difficulties in relations between developed and less developed countries. In evaluating the implications of these developments, Patterson makes the interesting point that since they evoked the Trade Expansion Act, their net effect may prove to have been favorable rather than unfavorable to nondiscriminatory liberalization of world trade if—as has actually happened—the Kennedy Round succeeds in achieving a substantial reduction of trade barriers on GATT principles.

Despite their discriminatory aspects, the EEC and EFTA can be argued to fall within the rules of nondiscrimination. More serious tendencies towards discrimination, in terms of positive espousal of discrimination as a principle, are evident in the use of discrimination as an overt tool of protection against low-wage competition, and in the demands of the less developed countries for preferential treatment of their exports of manufactures in the markets of the developed countries, which demands have been accepted as reasonable by a number of developed countries. The former trend is evident in the special treatment of Japan in GATT—"one of the more unsavory chapters in postwar international commercial policy" (p. 318)—and in the development of the "Long Term Arrangements Regarding International Trade in Cotton Textiles," discussed in Chapter 5. Patterson fears that this legitimation of the cheap foreign labor argument for protection could seriously hurt the less developed countries' export interests in the future. The latter trend is discussed in Chapter 6, which is particularly valuable for its marshalling of the precedents and policy discussions of the period before the World Trade Conference of 1964 publicized the proposal, and for its recognition of the subsequent weakening of the position on the issue the United States adopted at that Conference, by its support of the Ankara Agreement associating Turkey with the EEC, and the signing of the United States-Canada accord on trade in automotive products.

The concluding evaluation of the case for preferences substantially modifies the position Patterson adopted in his *Lloyds Bank Review* article of 1965, in the light of forth-coming work on the theory of preferences based on the concept of effective protection; while he continues to stress the political and administrative costs and difficulties of giving aid through preferences, he no longer denigrates their potential economic effects on a priori grounds, but instead argues the need for a major study of these effects to be undertaken.

There can be no doubt that the past ten years have witnessed a significant

reversal of the trend towards a return to the principle of nondiscrimination evident in the postwar decade. But it is questionable whether as yet this reversal can be called "a headlong flight toward a massive recrudescence of discrimination" (p. 395), threatening to undermine the whole process of negotiation of reciprocal tariff reductions. To take this view is to accept too readily recent words as evidence for imminent deeds; in particular, much depends on whether the 1968 meeting of United Nations Conference on Trade and Development proves a forum for world statesmanship or a wailing wall for stale complaints and recrimination. Nor does it seem very helpful to attempt to stem the tide by preaching the virtues of nondiscrimination, and warning of the costs of discrimination, the note on which the book concludes. One could instead argue that the frequent hypocrisy of the United States, in preaching nondiscrimination while practising massive continental protectionism, has induced first the Europeans and then the less developed countries to seek in their separate ways to obtain for themselves what they conceive to be the real source of U.S. economic power—a large protected market—and that what is needed to stem the tide of discrimination is not more sage preaching of nondiscrimination but more adventurous practising of free trade policies by the United States.

HARRY G. JOHNSON

*The London School of Economics
and The University of Chicago*

Economic Policies Toward Less Developed Countries. By HARRY G. JOHNSON. Washington: Brookings Institution, 1967. Pp. xvi, 279. \$6.75.

Despite the limited effect of the 1964 United Nations Conference on Trade and Development on the policies of advanced countries, it established a framework for continuing debate between the developed and underdeveloped countries that promises to endure. By juxtaposing the whole set of economic relations between the two worlds, UNCTAD called into question the fundamental principles which permitted a compartmentalization of trade, aid, and monetary policy in advanced countries. In each area, the claim was advanced that the existing international rules discriminate against the less developed countries and that taken together they seriously hamper the possibilities of development.

Harry Johnson's survey is one of the most stimulating and provocative contributions to the post-UNCTAD discussion. He brings out with great clarity and insight the essential features and interrelations among the leading proposals for improving international economic relations. Particular attention is given to three central questions: commodity agreements, trade preferences for manufactured goods, and the trade-aid option.

Johnson is at his best in exposing logical fallacies in arguments for various forms of interference with the free market. Protectionist approaches by both rich and poor countries are contrasted unfavorably with an optimal system which would combine free trade, realistic exchange rates, and the provision of aid on the basis of net resource requirements. Since unfortunately nothing approximating the optimal system appears politically feasible, the policy implications of the analysis hinge on comparisons among second-best, third-best, and *n*th best solutions.

One of Johnson's main contributions is to show the weakness of the U.S. argument against preferences for industrial goods, which is based on the fact that average nominal tariffs are already low. However, effective tariffs on value added are typically twice as high and those on products of interest to the less developed countries are higher still. Although preferences would create many administrative difficulties, these must be weighed against the drawbacks of other ways of stimulating manufactured exports.

My only serious criticism concerns Johnson's one-sided treatment of the trade-aid option. Here the choice of political assumptions is crucial to a realistic comparison of alternatives, and Johnson's assessments are more open to question. He implies that the administration of trade policy can be made enlightened and sophisticated, but that aid policy will inevitably be blundering and politically motivated. His implicit assumptions about the developing countries seem biased in the opposite direction: although they typically have inefficient trade policies and over-valued exchange rates, they are assumed to be better judges of resource allocation than the lending agencies of the advanced countries. While tariff negotiations are described with insight and realism, the process by which aid loans are negotiated and development policies influenced is ignored. A more balanced comparison would reveal some favorable aspects of aid as a means to better resource allocation, including pressure for better trade policies. Even Johnson is forced to conclude that "for the short run an increase in the supply of foreign aid would be more effective in supplying external resources for development than would the opening of trade opportunities" (p. 245).

The central facts which the United States refused to face at UNCTAD are (1) that most of the poorer countries must industrialize in order to transform their economies and raise their incomes, and (2) that the trade policies of the advanced countries have a dominant effect on the possibility of their carrying out this transformation. I would strongly support Johnson's advocacy of the free trade option over the protectionist approach favored by most of the underdeveloped countries if the aid part of the package could also be supplied. The problem is not disposed of if this effort fails, however, and the search for second-best solutions must be pursued. "What the United States cannot continue to do is to follow protectionist policies for itself, while using the language and concepts of the free-trade position to deny that these policies injure the less developed countries whose economic development it has pledged itself to help promote, and to reject their proposals for improvement of their trading opportunities" (p. 241).

HOLLIS B. CHENERY

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Forward Exchange Speculation, and the International Flow of Capital. By HERBERT G. GRUBEL. Stanford: Stanford University Press, 1966. Pp. xiii, 192. \$6.50.

Forward exchange has been somewhat neglected in international economics because of the difficulty and narrow application of the concepts involved. Recently, however, it has acquired importance because of the possibility that it can be used to freeze the growing stock of short-term funds which can move

internationally in search of maximum interest yields and which thereby threaten the survival of independent national policies on interest rates.

Professor Grubel's book will serve the interest in forward exchange at three levels. It begins with a careful and thorough exposition of forward exchange that will be useful for those insufficiently familiar with international trade to absorb the subject from the concise expositions found in the journal literature. Just one slip was found in the development of the basic model, but it will cause confusion and should be eliminated: because the price of the forward exchange "insurance" demanded by those who place funds abroad for a higher short-term yield will be negative, the demand for it will *not* (contrary to pp. 42-44) become abnormally small and very insensitive to reductions in price when the value of the foreign currency held is close to its minimum value (the minimum set by the lower support point).

The author provides next some partial but worthwhile statistical tests of the validity of conventional forward exchange analysis. Some of the tests parallel those made prewar by Einzig and Bloomfield, but are of course more rigorous; they use weekly data from several countries and cover a variety of conditions which are of interest in forward exchange policy: the disturbed conditions of 1955-58 with partial inconvertibility of major European currencies, the stable, "normal" period of 1959-60, and periods of speculation against the dollar in 1960-61.

One of the tests confirmed the theory that in normal, nonspeculative periods the movement of short-term funds in search of higher interest rates would continue to the point where the net yield advantage (after payment of the cost of forward exchange cover) was very small, under one-half per cent. Unfortunately, the author did not have suitable data for testing whether it was only a small or a large movement of funds which caused the price of forward exchange "insurance" to move by enough to absorb most of the advantage from higher short-term interest rates abroad. If the price elasticity of supply of forward exchange actually was low—the prevalent view until the 1960s—then both the importance of the forward exchanges in international economics and the need for international harmonization of interest rates would be greatly reduced. (An approach to the necessary kind of information through the use of econometric models has been made by Jerome Stein, *Am. Econ. Rev.*, March 1965, and *Rev. Econ. Stud.*, April 1965.)

A test yielding indirect evidence that the elasticity of supply of forward exchange should—under certain conditions—be high enough to meet the demands arising even from very large transfers of short-term funds was made by determining that stabilizing speculators not having any advance information could have earned high rates of profit on funds invested in the forward sterling market during 1955-61. However, the demonstration is not convincing, even given continuation of the earlier conditions, not only because certain risks, costs, and enlargements of the necessary investment base were explicitly excluded from consideration, but also because income taxes on the resulting 16 per cent to 30 per cent annual rates of profit as well as the need for an additional amount of investment where the higher rates were claimed were neglected; and the fact that dealing at the "wholesale" exchange rates necessary for

earning the assumed profits required a capital stock of many millions per speculator was ignored.

A test which provided some ground for worry about the validity of the accepted analysis concerns equality in the costs of a foreign currency bought direct and bought by first acquiring a different currency. This test was successfully passed by spot exchange rates, but where Canadian dollars were concerned the nearly cost-free spatial arbitrage of the exchange dealers failed, for reasons unknown, to eliminate cost discrepancies of as much (according to a check of the author's basic data appended to the book) as one per cent in periods of exchange rate speculation.

The final part of Grubel's work provides an exhaustive and quite useful theoretical analysis of the possibilities for protecting a country's exchange reserves under a variety of conditions through official intervention in the forward exchange market. It should be noted, however, that the readers who benefited from the initial, elementary exposition may not survive these chapters. The inevitable unwieldiness of the multiple relationships which must be analyzed, complemented by a sprinkling of typographical errors and expository slips, will deter most nonspecialists from making the effort necessary for negotiating the terrain. (At the extreme, despite having presented similar conclusions through use of related concepts, this reviewer found the analysis of the infrequently examined triangular arbitrage problem practically impenetrable because it combined garbled text within mislabeled diagrams.)

Despite some concern about triangular problems, Grubel remains an advocate of official forward intervention. His own presentation of the case seems to provide somewhat weak support, however, for he states that official intervention can be undertaken only when the authorities are sure of success in defending the exchange rate (i.e., when the intervention is least needed); the capital losses in case of an ultimate forced devaluation are too great to risk. This policy would make forward intervention, when it was used, doubly successful, for bear speculators would know from the fact of official intervention that their case was hopeless. But the mirror-image consequence is that refraining from intervention because success is not a certainty would encourage the bears and convert a merely possible forced devaluation into a likely one. This implication of Grubel's analysis weighs against any use of official intervention. However, its importance is diminished when allowance is made for those possible enlargements of official exchange holdings (and hence reductions of the amount of net capital loss from devaluation) which official intervention can produce but which the author did not include: the diversion of traders and destabilizing speculators from buying spot exchange to buying the bargain-priced forward exchange being offered by the authorities.

WILLIAM H. WHITE

International Monetary Fund

Trade Policy and the Price System. By L. B. YEAGER AND D. G. TUERCK. Scranton, Penn.: International Textbook Co., 1966. Pp. vii, 294. \$3.95.

"The trouble with most economists," a colleague of mine recently remarked, "is their damn fair-mindedness. They see every side of a question." The first

virtue of this textbook is its refreshing, almost brazen, advocacy of unilateral free trade for the United States. The opening three chapters state the positive case for unfettered commerce, the next eleven catalogue and ably expose protectionist arguments, and the final two discuss strategy questions.

A second virtue of *Trade Policy* is its lively style. The title might suggest a geometrician's paradise of box diagrams, preference curves, and marginal cost schedules, but Yeager and Tuerck spend little time expounding these mysteries of the "Price System." Instead, relying on plain English, they give the student a readable, closely reasoned, analysis of policy arguments. One novel expository technique: the authors liberally quote protectionist testimony taken from Congressional annals. This testimony not only provides comic relief, it also illuminates "practical" thinking on trade matters.

A third virtue of *Trade Policy* is its use of fresh institutional material. The theory of free trade has changed little in the past half-century. But each generation of students deserves to see important policy issues framed in familiar surroundings. Yeager and Tuerck are better landscape architects than most textbook writers.

If the authors were to add another section, they might explain why, given its academic credentials, free trade enjoys so little political support. Certainly, the weakness of consumers, the strength of protectionists, the supposed sanctity of \$35 gold, and plain economic innocence all play a part. But it puzzles me why the 150 largest supercorporations care so little for free trade. These firms would surely benefit from complete elimination of trade restrictions, thanks both to their commanding grasp of technological and managerial skills, and to their present concentration in exportable goods. Do these powerful natural allies so poorly understand economic phenomena that they only see those tariffs which protect their *own* products? I hope that the explanation runs deeper than that.

The authors emphasize certain drawbacks in the free trade case. The static gains accruing to the United States from unilateral free trade would not be enormous, surely less than \$8 billion. This figure is exceeded by *quarterly* GNP increments during a good year. Moreover, devaluation of the dollar, either explicitly or through fluctuating rates, is recognized as a concomitant of unilateral free trade.

Unfortunately, Yeager and Tuerck are not altogether candid on certain other aspects of the free trade case. Labor must speedily relocate if gains are to be fully realized. The relocation burden would fall most heavily on labor-intensive, low-wage, "backward" industries. There are plenty of instances where the price system has accomplished such adjustment only slowly and painfully.¹ To be sure, many economic changes besides tariff reduction contribute to human distress in these least adaptive sectors. But Yeager and

¹ R. Vernon makes the point that free trade in advanced nations, like technological progress, would sacrifice weak industries for the immediate benefit of strong, better earning, activities. In view of this probable outcome, it would be interesting to estimate roughly the kind of welfare function and adjustment speeds required to *negate* the apparent money gains of free trade. Incidentally, free trade in developing nations would probably most penalize the newly ascendant and relatively prosperous manufacturing centers.

Tuerck see widespread misery as an argument *against*, not *for*, broad policies of adjustment assistance.

When the authors discuss infant industry and diversification tariffs, they neglect to emphasize the relevance of these arguments for emerging nations. Thus, while the thrust of *Trade Policy* is unfettered commerce for the United States, the student might finish this book believing that all nations should abolish trade restrictions.

I am sorry the authors throw cold water on those who advocate American tariff reduction and quota liberalization as a way of promoting economic growth among developing nations. But Yeager and Tuerck prefer to see free trade grounded entirely on self-interest. They fail to appreciate that "... self-interest . . . may serve as [a] powerful source of social energy, but not as [a] powerful avatar of the social imagination."² Gains of \$8 billion are marginal to the American economy. Gains of half that amount would have, for the poorer nations of Asia, Latin America, and Africa, very substantial consequences.

These omissions, policy non-recommendations, and nuances of emphasis, betoken more than simple neglect. Yeager and Tuerck are both associated with the economics department at the University of Virginia, which is ideologically close to the University of Chicago's department. As they state in the preface, "... we use [the free trade versus protection issue] for definiteness in comparing two broad lines of policy, one that welcomes and one that mistrusts the working of a free price system." Starting with this objective, *Trade Policy* belittles the need for *any* government interference in the marketplace.

The case for free trade is a strong case. It has survived since Hume despite concentrated and persistent opposition. Whether it can survive the company of Professor Friedman's policy nostrums remains to be seen.

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University of New Mexico

² R. L. Heilbroner, *The Limits of American Capitalism*, New York 1965, p. 127.

The Future of the Dollar as an International Currency. By ROBERT Z. ALIBER. New York: Frederick A. Praeger, 1966. Pp. 169. \$6.00.

There is a continuing need for informative studies which deal with the complex issues of international currency. The topic is highly controversial, although not all can join in the controversy objectively because the issues are burdened with technicalities and prejudices. To the average reader, the topic of international money seems to have a decided mystique. For long periods of history, gold has served an important function as international currency. More recently, however, its function has declined relatively as the currency of the United States has assumed new and important international dimensions.

Professor Aliber's book deals with these new functions of the U.S. dollar in its expanding global activities. In brief, the most significant point which emerges from this study is that the international status of the dollar has been determined not by the U.S. authorities but by the preferences of other countries. Domestic as well as international economic objectives of the United

States therefore, have had to be modified in order to deal with international dimensions of the currency.

In order to deal with the future role of the dollar, Aliber has found it necessary to provide much interesting although not provocative background material. Included are discussions on international financial cooperation; the attempts to establish international financial policy; the special difficulties of reserve currencies and the conflicts which can arise between national interests and international financial arrangements. Only the final chapter deals directly with the topic suggested by the title of the book.

It is pointed out that the future of the dollar, as an international currency, will be conditioned largely by the institutions which emerge as payments mechanisms or as credit creating bodies. Adjustments by the United States to payments imbalances cannot be arrived at without affecting the volume of foreign reserves required by expanding world trade.

Aliber considers it extremely unlikely that institutional arrangements can be established that would obviate the need for reacting on an *ad hoc* basis to payments imbalances or that would dismiss the need for providing finance for payments deficits. This arises, he thinks, because countries cannot decide among themselves who should initiate adjustment. Aliber feels that priority should be given to the further negotiation of formal and informal multilateral arrangements so that adjustment mechanisms are more adequately provided for.

The author seems to weigh rather lightly the disadvantages of an increase in the world gold price, or resort to a system of flexible exchange. He finds, on balance, economic as well as political advantages accruing to the United States from either of these measures. Aliber sees some advantages in a system of flexible exchange rates in which the U.S. authorities cease to buy and sell gold at a fixed price. He claims that in a world of flexible exchanges the dollar would possibly be the best store of value given the long term record of price stability in the United States. None of the devices widely broached as solutions to contemporary difficulties would seriously demote the dollar as a prime international currency. At present, it is the dollar which gives gold its value and not the other way around.

Aliber attempts to show the value of having options for policy making. However, the chapter that deals with the decision making process is rather disappointing. Instead of being taken behind the formal conference tables of the international bodies, we are presented with a quick tabulation of formal authority for several of the international financial institutions with a hasty analysis of power in the executive branch of the U.S. government. The policy options are not precisely outlined by the author. Many sections of the book are repetitive and the use of quantitative material is neither original nor deft.

There is an attempt in this book to produce a balanced view of the alternatives open to the United States and other countries in dealing with serious problems of payments deficits and reserve currencies. This book does not argue one consistent point of view. Rather it is for all those desiring to know the outstanding issues in a multicurrency world.

KENNETH J. ROTHWELL

University of New Hampshire

American Investment in Australian Industry. By DONALD T. BRASH. Cambridge: Harvard University Press, 1966. Pp. xv, 366. \$7.50.

Dr. Brash's book not only is about American investment in Australia, it is itself a part of it: the name of Harvard University Press turns out to cover a book which was "printed and manufactured in Australia." The study represents the author's doctoral thesis instigated and written under the auspices of Professor Sir John Crawford at the Australian National University. The subject matter is essentially a report on the results of a survey of 208 manufacturing companies operating in Australia in 1962 that were at least partially American-owned. The criterion for inclusion was a minimum of 25 per cent American ownership, "American" being defined to include United States and Canada. The purpose of the book is "to add to the meagre stock of information currently available on the subject of foreign investment and to attempt answers to at least some of the questions being asked in the field" (p. 7).

The author starts with a discussion on Australian attitudes toward foreign investment—an issue of some concern especially since the spectacular success of General Motors-Holden in the 1950s. As would be expected, there is a considerable degree of ambivalence about this matter, perhaps best illustrated by the contrast between the negative attitude of the Federal Labor Party and the encouraging actions of the party's State and local dignitaries. Following this introduction, the author proceeds to examine the importance of foreign capital to see whether it justifies the public concern which it provokes. The evidence presented by the author indicates that American-affiliated companies in Australian manufacturing industry made up a little less than 7 per cent of total employment, with British and other foreign affiliated companies raising the foreign share to no more than 20 per cent. If value of production rather than employment is used as a yardstick, the respective percentages are somewhat higher. Thus the magnitude of the foreign share is far from negligible and may justify interest if not concern.

Most of the remaining chapters deal with the results of the survey conducted in 1962. These results are grouped into topics such as reasons for investment, ownership patterns, management characteristics, transfer of technology, and so on. With minor exceptions the method is always the same: the author introduces the topic, presents answers in tabular form, gives a verbal description of the main features of each table, quotes some respondents who gave interesting or illuminating answers, and adds his own comments and observations. No formal hypotheses are set up and no statistical tests are attempted. Only two chapters depart substantially from this pattern, Chapter 8 which contains a discussion on "some effects on Australians" and Chapter 11 called "Conclusions." Both chapters deal with foreign investment from a theoretical standpoint.

In general, the approach of the author is purely empirical. This is rarely rewarding, but in the case under review the approach leads to particularly serious drawbacks. The first of these is the obvious lack of guidance by economic theory in the choice of questions asked in the survey. This neglect of economic theory is noticeable throughout, but it is most striking in the chapter on reasons for investing in Australia. The respondents were given a choice of twelve reasons none of which included profitability; it took one of the responding ex-

ecutives to spell out that "the big reason a company decides to invest abroad is because it believes it can get a better return on its investment abroad than at home" (p. 40). The second drawback arises when we come to assessing the answers: while we are told here and there that such and such result is "significant," we are never told for what it is significant. Another difficulty of neglecting theory is that it leads the author to an implied surprise (if not moral condemnation) when he finds evidence of profit-maximizing behavior. Thus, for example, in the chapter on management the author takes a long journey to come to the conclusion that "there is little doubt that most American subsidiaries, like most Australian companies, pay wages which are no higher than necessary" (p. 135). Finally, there are answers which have little or no informational content—for example, statements about demand without mentioning price, or comparisons of salaries without mentioning productivity differentials. The frustrations of the reader on this account are not alleviated when reaching the final chapter where the conclusions presented are not related to the results of the survey but are based on an almost independent theoretical discussion. In any case, at least one conclusion, viz. "as long as private direct investment benefits Australia, it should be encouraged" (p. 286), is not very profound.

But it would be uncharitable and unfair to Brash to finish this review on an entirely negative note. The author has done a great amount of work in collecting the survey results and in gathering other statistical information from a variety of sources. All this material is well organized and interestingly presented. The style of writing is lively and well balanced: the one joke in the book is offset by one equation. But the most important part is that the author's vision is in the right place. He sees quite clearly that any criticism of the behavior of American-affiliated companies should be directed at the Australian institutions which permit restrictive trade practices and give generous tariff protection to local manufacturers.

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Business Finance; Investment and Security Markets; Insurance

Corporate Dividend Policy. By JOHN A. BRITTAİN. Washington: Brookings Institution, 1966. Pp. xvii, 254. Paper, \$2.50.

The purpose of this book is to estimate the determinants of corporate dividends and to isolate the influence of tax laws on dividend payments. Starting with the standard Lintner hypothesis, Brittain tests the importance of various alternative measures of profits and cash flow in the dividend function. He then examines the effect of various other variables on the explanation of dividends. Next several alternative regression equations found to be successful are estimated at the industry and firm level. He then attempts to pool time series and cross section data as a further method of testing the results. Finally the findings are summarized and the implications of the results for policy purposes are analyzed.

The early chapters of this book are well done and present little cause for disagreement, following as they do the familiar work of Darling, Kuh, and others. It is shown that cash flow—profits after taxes plus depreciation—explains dividend behavior much better than balance-sheet profits after taxes or other series of profits adjusted by more realistic depreciation allowances. For the aggregate function, the fit (adjusted for degrees of freedom) is better when cash flow is used alone than when profits and depreciation are both included explicitly in the equation.

Brittain tests the two halves of the sample period (1920-41 and 1942-60) separately and finds that the corresponding coefficients of the two subperiods are significantly different. This suggests that increases in income tax rates result in lower dividend payout ratios. Several "tax shelter" variables are proposed to test this hypothesis. The one chosen is the marginal tax rate for that income level which cuts off recipients of 25 per cent of all dividends at the top of the income scale. This rate of course varies from year to year but has averaged about 20 per cent for the interwar period and 60 per cent for the postwar period. Such a variable is highly significant and improves the fit of the equation substantially. However, with the inclusion of this tax shelter variable Brittain finds that the fit of the equation is better if profits and depreciation are treated separately instead of being combined in a single cash flow term. Further experimentation leads to the choice of the ratio of balance sheet depreciation to "realistic" depreciation (figures of the Machine and Allied Products Institute are used) for use in additional regression analysis.

In his chapter on other factors influencing corporate dividend policy, the author tests the effect of the interest rate, growth rates (measured by the change in sales), corporate tax rates, investment demand, corporate liquidity, inflation, and changes in stock prices in the dividend function. None of these variables is successful except the interest rate. These results may partially be due to the way in which the dividend function is estimated. Brittain uses the form

$$(1) \quad \Delta D = a_0 + a_1 r P - a_2 D_{-1}$$

where D = dividends, P = profits, and the short-run payout ratio $r = \Sigma d_i X_i$ (X_i are all the other variables being tested in the equation). Thus in actual estimation all of the X_i are multiplied by profits, which leads to serious problems of multicollinearity. Brittain comments on this at one point and calculates the matrix of simple correlation coefficients, most of which are well above 0.9. This problem could have been mitigated by dividing the equation by profits and estimating the dividend yield as the dependent variable, i.e.

$$(2) \quad \frac{\Delta D}{P} = b_0 + b_1 \Sigma d_i X_i - b_2 \frac{D_{-1}}{P}.$$

Writing the equation in this way would also have made it easier to see that the significantly negative interest rate coefficient measures the general inverse cyclical relationship between dividend yields and interest rates. It is not clear

whether this relationship can correctly be transformed into the causal relation that high interest rates "may discourage dividends by making internal finance relatively more attractive" (p. 104). If such a direction of causality does exist, it would then seem that other sources and uses of corporate funds would also be important determinants of dividends; yet this is found not to be the case.

The results obtained from the industry and firm time series regressions are broadly consistent with the aggregative results. Of the 17 industries subjected to examination, depreciation or cash flow is significant in 12, the tax-shelter variable in 13, and the interest rate in 7. For 40 individual firms the findings, as might be expected, are not so good. Depreciation or cash flow is significant in 12 regressions, the tax-shelter variable in 16 and the interest rate in 11. It is not clear whether Brittain considers these latter conclusions satisfactory. At first he writes "... the frequent significance of these variables is impressive in light of the relative stability of the aggregate payment ratios for this group of firms" (p. 159) but later decides that "the earlier findings of this study concerning the influence of depreciation, tax rates and interest rates were only partially supported by time series analysis of individual firms, and were given no support by the firm sample total" (pp. 187-88).

In his last chapter of empirical results "Pooling of Time Series and Cross-Section Data," Brittain considers cross-section analysis for the first time. This could have been the most interesting chapter, but in this reviewer's opinion his method contains some defects. The cross-section regressions consist of estimating the simple function $D = b_0 + b_1P$ for all 40 firms and obtaining a given value of b_1 for each year. After spending a good part of the book showing that dividends depend more on cash flow rather than net profits, it is a little surprising that Brittain reverts to the latter measure at this point in the analysis. Furthermore, no adjustment is made for heteroscedasticity so that the estimates are dominated by the larger firms. While this may not be inappropriate, which is the position taken by Brittain (p. 174), the resulting parameters are not cross-section estimates in the usually accepted sense of the word. More serious, this lack of adjustment virtually forces the equation through the origin, so that the estimate of b_1 is simply the average dividend/profit ratio. The time series of b_1 obtained in this manner is then regressed on the depreciation ratio A/A_2 , the tax shelter variable t_{25} , and the interest rate, or

$$(3) \quad \frac{D}{P} = c_0 + c_1 \frac{A}{A_2} - c_2 t_{25} - c_3 i.$$

This equation is identical to equation (2) above, with the important exception that lagged dividends are now excluded. Not surprisingly, the fit is much lower and significant positive autocorrelation of the residuals appears for almost the first time in this study. This suggests the inclusion of additional variables in the equation, and investment and corporate liquidity (both relative to sales) are again tried. This time both are significant. If these results are to be

taken at face value, investment affects dividend policy in the long run but not the short run. Brittain states, "the investment coefficients have the expected negative sign, supporting for the first time in this study the proposition that a high investment demand will tend to restrain dividend policy *in the long run*" (p. 183, italics added). Since investment is measured relative to sales, this implies that more capital-intensive firms have lower long-run dividend payout ratios. Since highly capital-intensive firms such as public utilities have relatively high payout ratios, this view does not seem to agree with the facts.

In Brittain's concluding chapter, he summarizes the results of tax rates on dividend policy and finds that "... federal fiscal policy exercises substantial influence over corporate dividend-saving policies, even without resort to special devices such as an undistributed profits tax" (p. 207). This comes about through the importance of depreciation allowance and individual income tax rate variables in the dividend function as well as the obvious relationship that a change in corporate income tax rates will modify the percentage of profits available for dividend payments. He also discusses the pros and cons of corporate income retention, but does not take sides on the issue. The analysis in this section is a bit sketchy; probably the claim that high corporate saving promotes the concentration of economic power deserves more than one sentence.

In summary, the effects of depreciation allowances and tax rates on corporate dividend policy have been shown by Brittain to be quite substantial at the aggregate and industry level. While this section is thoroughly and competently done, it does not offer a great deal of new information. Since dividends are merely one use of corporate funds and as such must compete with alternative uses, particularly investment, it is somewhat disappointing that this important relationship was not studied in greater detail.

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Insider Trading and the Stock Market. By HENRY G. MANNE. New York: The Free Press, 1966. Pp. xiii, 274. \$6.95.

Professor-lawyer Manne suggests that we take another look at the assumption that insider trading is harmful to the welfare of stockholders and the workings of our economic system. His book contains no empirical evidence on that point, but rather a lawyer's defense, well documented with legal, economic and financial references.

The argument goes something like this: (a) no one has established proof that insider trading is harmful, barring fraud and manipulation which are illegal regardless of insider rules; (b) existing laws prohibiting insider trading are difficult to administer and there are many ways to avoid detection; (c) insiders can provide an important economic service, that of innovation in the firm, which is not adequately compensated by present methods of salaries, bonuses, and options for management; (d) there is a market for valuable information where an innovator could take inside information and receive compensation for innovative changes in a company; (e) most long-term investors are not harmed by insider trading; therefore (f) insiders should be allowed free

trading in their company's stock as a means of encouraging and compensating innovation.

To complete his brief, Manne concludes with a discussion of the exceptions, indicating that: (g) most objections against corporate insider trading are not persuasive and (h) government employees should not be allowed to trade on inside information, as they have no innovative contributions to make toward corporate activity (administration of rules is again difficult, but necessary in this case, according to Manne).

Manne has prepared his brief well, although it is not without logical flaws. As he notes after one argument, "Some of the evidence to establish this point is a bit dated to be sure, but recent studies indicate no fundamental changes. And, if empirical evidence is lacking, we can fall back on logical inference" (p. 75). The book is offered as a "theoretical treatment" of the subject; and Manne hopes that it will be convincing without the much-needed empirical research which he calls for from time to time. Perhaps the greatest value of the book, however, is that it does emphasize the need for research to establish some background for what is otherwise only a debate in logic.

Let us concede that it has not been proven that insider trading is always harmful, that detection and administration under present rules are difficult, and that innovation is important but difficult to compensate. We might even concede that stock market trading is one method of compensating inside innovators, although it is difficult to imagine a private information distribution system within a corporation where only the innovator—or those he wishes to favor—would be compensated. Still, we cannot agree that Manne's conclusion (point f) follows.

The insider will have limited financial means with which to take advantage of his inside information, will have to act fast, and may, himself, affect the price of the stock so that he receives only a small profit compared to the actual value of the news. He can nonetheless make up this loss by the barter of stock information, according to Manne.

To establish the presence of a market for such information, Manne names four essentials for an information clearinghouse: (1) it is a repository of valuable information, (2) it can distribute valuable information to those who make deposits of information, (3) it can maintain a priority or preferred list indicating those entitled to information, and (4) it has some method (such as discretionary accounts in brokerage firms) to assure that individuals do not withdraw more information value than they are entitled to. Manne then shows that investment banking houses, including large brokerage houses, could perform each of these functions separately. Therefore, they must be available to provide the entire package, orderly trading of valuable inside information. The conclusion: "Until convincing evidence to the contrary is available, we should assume that this market exists and is efficiently maintained by important functionaries in the financial community" (p. 75).

Although informal bartering of information is undoubtedly present in investment banking and elsewhere among those of sophistication and position, this does not imply that these channels are open for the random innovator pictured by Manne. This innovator, or entrepreneur, is not top management but,

rather, anyone with an idea up or down the line. Is there, indeed, an organized market for such innovators to trade information? They may have a bit of information to trade but once in a lifetime and may not be privy to these high-class bartering techniques. Certainly there is no evidence of investment banking firms including the casual stranger in their trading circles. Will the non-management innovator be able to convince anyone of the worth of his information? Will he have the funds to take advantage of the information by independent trading?

But insider rules are directed primarily at those in control—officers, directors, or 10 per cent shareholders of a corporation. The officer-innovator among the insider group, in fact, can be compensated by salaries, bonuses, and options. “But what incentive does he have to perform the unusual or to revolutionize a company or an industry?” (p. 133). Surely our present corporate system, although not perfect, has found some method of evaluating the services of officers and rewarding those of greatest value. Furthermore, from what we know of entrepreneurs, it must be assumed that they are motivated by more than monetary gains.

A fundamental point of the book is that no significant injury to corporate investors can result from insider trading; and here we must take issue. Manne’s point is simple: the effect of insider trading is to raise the price of a stock to where it represents the new value after the inside information is made public. Instead of random short-term traders or speculators benefiting, the insider benefits. The long-term investor merely realizes an increase in stock value and is otherwise unconcerned. (Although Manne contends that the effects of good news are more important than the effects of bad news, it is difficult to agree with him on that point. The long-term investor argument is simply not appropriate when bad news is concerned.)

Even long-term investors sell at some time; and many of them may be caught in the change between one value and a higher one, while insiders profit. Furthermore, the long-term investor may be influenced specifically by a rapid increase in a stock price, without apparent reason, and may sell before the stock reaches its “new true” value.

We cannot be as unconcerned about short-term traders, speculators, and specialists as Manne is. Manne is aware of the need for a continuous market in the shares of a corporation, but he would substitute trading by discouraged speculators with trading by the deserving insider. Presumably the marketability of a stock would be maintained. However, it is difficult to see how occasional insider trading in large volume can replace the daily activity of short-term traders and speculators in creating a continuous market.

But the most important omission in the book is the institutional investor. These long-term investors enter the market at frequent intervals because of the size of funds they have to invest. They sell frequently also, although we would hesitate to consider them short-term traders or speculators. Is this large share of the market also to watch out for itself? Or shall we put institutions in the market for insider information? What effect would such power have on the stock market?

Obviously we need much more empirical research before we can argue that

few worthwhile investors are hurt by insider trading. Even if Manne is right about entrepreneurship and compensation, we would like to see some evidence regarding the many different types of investors and investment decisions likely to be affected by free trading by insiders.

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Business Organization; Managerial Economics; Marketing; Accounting

Accounting: A Programmed Text. By JAMES DON EDWARDS, ROGER H. HERMANSON, AND R. F. SALMONSON. Homewood, Ill.: Richard D. Irwin, Inc., 1967. Pp. xxi, 1613, Two Vols., \$8.95 each.

The techniques of programmed instruction have gained increasing attention in recent years, and a programmed instruction format has been utilized in business and economics texts, including accounting principles. However, this new two-volume text is the first *comprehensive* treatment of basic accounting employing programmed instruction methods.

As the authors concisely point out in their preface, "the primary objectives, indeed, the advantages, of a programmed text are three-fold: (1) to permit a more active participation by the student in the learning process; (2) to permit immediate reinforcement of correct understanding and immediate correction of misinterpretations; and (3) to permit a possible reduction in the contact time between each student and instructor with no loss of comprehension or coverage of the materials studied." The advantages of programmed instruction are well established and the utility of the method is unquestioned. Nevertheless, possibly because the technique is relatively new, it has not been applied extensively in business and economics texts.

The scope of materials covered in this new accounting principles text resembles that of others, although the arrangement of subjects is significantly different. Topics included are: financial accounting, financial management, and managerial accounting. Managerial accounting has been given more emphasis here than in some books. This may enhance the value of the text for nonaccounting majors interested in developing an understanding of the application of accounting data to decision-making. Nevertheless, the nuts and bolts techniques of data recording, classifying, and summarizing have not been ignored.

The text is divided into two volumes, each suitable for a one-quarter or one-semester course. Volume I is devoted to consideration of accounting functions, data accumulation, the concept of the accounting cycle, and recording procedures. The arrangement of materials represents a significant departure from traditional texts; i.e., certain subjects often deferred to the end of a one-year program are presented early in the first volume, while other principles usually stressed relatively early in most texts have been relegated to the latter part of the second volume.

The revised organization should be very useful in the early development of

students' awareness of the accountant's role in a modern business enterprise. Further, economics students may find the arrangement of topics valuable because the utility of accounting data can be appreciated without becoming deeply involved in recording and data accumulation techniques.

Volume II is divided into three sections: earnings determination and asset and liability valuation; internal accounting; and special topics, which include discussion of subsidiary ledgers and journals, the voucher system, and single proprietorships and partnerships. The development of special topics at the end of Volume II represents an important departure from the customary presentation. No major weakness appears to have resulted from the new arrangement. A basic understanding of some of the problems of record-keeping may be adequate for students, many of whom will not become professional accountants. Placement of detailed recording procedures in the latter part of Volume II enables an instructor to omit more than an introduction of the subject early in the course, but include the material for comprehensive coverage if desired. The authors' decision to de-emphasize recording procedures and partnership accounting is a logical departure from traditional approaches, and one which more adequately fulfills the requirements of contemporary students.

Each chapter in the text includes a summary of important concepts, a glossary of terms, and a review of principles through questions, exercises and problems. Further, supplementary review questions, exercises and problems form an appendix at the end of each volume.

A note about certain structural techniques in the text is necessary. This programmed text employs the scrambled method of materials presentation, and does not follow the page-by-page progression found in standard texts. A concise explanation of the programmed method is included in the preface to each volume; and a few moments spent in understanding the scheme of presentation is vital to effective utilization of the text.

In summary, this two-volume text is well written, concisely organized, and it represents a substantial departure from the format of traditional accounting texts. Educators faced with constantly increasing enrollments and rising costs have been offered an alternative to present methods of accounting principles instruction. With publication of this text, the programmed instruction approach finally has moved toward maturity in the presentation of business subjects. Its success may presage application of the technique to other business disciplines and to economics subjects as well.

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Industrial Organization; Government and Business; Industry Studies

Competition and Oligopsony in the Douglas Fir Lumber Industry. By WALTER J. MEAD. Berkeley and Los Angeles: University of California Press, 1966. Pp. x, 276. \$6.00.

While the title of this book refers to the Douglas Fir lumber industry, shown to be a subset of the national market for softwood lumber, the most

original and useful analysis deals with the local oligopsonies in the purchase of timber from federal lands. Little timber is sold by other owners. Within the two-state region of Oregon and Washington the author identifies 43 "working circles," or local timber markets, each delimited by the cost of hauling logs to established sawmills. The intensive study of these local markets is delayed by an overly meticulous development for the national lumber market of the familiar structure-practices-performance procedural steps from timber ownership through sawmill operations to selling and distribution of lumber. Not surprisingly, this national market is found to be competitive. With the expected prices for lumber in that vast market as data, the few mills located so that they can economically cut and saw particular stands of timber are cast in the oligopsonists' role. Added features influencing timber buyers' behavior are the government's practices in selling timber and the fact that some mills have the alternative of cutting timber they own.

Because this local market oligopsony must reflect the efficient size of sawmills, the survivor test is applied to changes over a decade in shares of regional output by mills of different sizes. While the author notes some of the difficulties in applying this test he does not recognize that location affects costs and feasible mill size. The only available measure of mill size is board feet of output. This groups together the quite unlike "dimension" mills turning out only lumber of two inches or more in thickness and "grade recovery" mills producing a wide variety of thicknesses and widths. Fortunately what these faulty data show—a marked increase in the share of regional volume obtained by the middle-size mills—is reinforced by the engineers' estimates of minimum optimal size of sawmills built *de novo*.

While that scale is not so large absolutely as to pose a serious capital supply problem for a new mill, the volume of an optimal plant would add such a large percentage to the local demand for timber that Bain's "percentage effect" barrier to entry is high. A potential entrant could anticipate that the price of timber would be increased substantially by his addition to the demand. The likelihood of this outcome would be enhanced by the fact that the amount of timber offered by the government is unaffected by the price of timber.

Buying behavior by existing mills is conditioned by several unique factors. A floor is placed on bid prices by the government's appraisal price. Local mills face the possibility, however, that a mill outside the working circle may bid, but only over the hurdle of substantially higher costs for hauling the logs. The government's policy is to sell timber stands when they are deemed to be mature regardless of whether it expects a high or low price. Consequently, the amount of timber put up for sale in a particular call for bids is perfectly inelastic, and the same condition holds for a succession of timber sales. (The supply curve in the chart on page 166 is confusing.) However, mills that own timber may elect to cut more of these trees when lumber prices (and presumably the competitive bid prices for government timber) are high. This alternative tends to put a ceiling on what is bid for timber. Furthermore, various mills' demand for timber from a particular government offer must reflect their differing backlogs of purchased but uncut timber. If this timber is not cut on schedule it goes up for bid again. A lucid summary of these facts and of their

significance would have helped the reader to follow through the detailed study of bidding behavior.

Against this factual background and by use of bargaining and duopoly theory, the author makes an illuminating and skillful exposition of the bidding process which can be only highlighted here. There is more than a suggestion of collusion on occasion, with evidence also of frequent breakdown of tacit and what may be formal agreements. There is much attention to behavior of individual bidders whose situation is well known by rivals. At least some outcomes are predictable in light of the number and characteristics and the current operating situation of various mills. Sometimes longer-run considerations enter, both in the actions of a particular buyer and in a united front against bidders from outside the working circle.

There emerges a fascinating and instructive moving picture of an oligopsonistic market in operation, the results of which are tested by regression analysis. A statistically significant negative relationship is found between the *ratio* of the sale price to the government's appraisal price and each of the following independent variables: the absolute size of bidders, the number of bidders, and the size of the block of timber up for sale. The latter is positively correlated with the sum required to build the access roads. These findings mean that smaller buyers are generally at a disadvantage in bidding.

Questions follow about the government's practices in timber resource management and in setting the conditions for bidding for timber. Some of these handicap bidding by small firms. Particularly where bidders are numerous enough and independent in their interests for the price to be competitive, the large margin by which the government's appraisal price is set below what proves to be the sales price suggests deficient analysis by those setting the appraisal price. Consequently, this study not merely tests theories of behavior of a few-buyer market but also points out defects in public policy and in the decisions made in its application.

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Labor Economics

Married Women in the Labor Force: An Economic Analysis. By GLEN G. CAIN. Chicago and London: University of Chicago Press, 1966. Pp. xiii, 159. \$6.50.

To the growing stock of empirical studies on labor force participation, this compact monograph is a welcomed and highly important addition. The immediate focus of Professor Cain's study is on the determinants of participation decisions by married women in the United States at a point in time—a subject of great interest in its own right. But his main objective is a better understanding of why Negro married women have higher participation rates than their white counterparts, and why the participation of both groups has increased over time.

The book consists of four chapters and nine appendices. Following a short introduction that sets forth the plan of the book, Chapter 2 develops a the-

oretical model of participation decisions, including its application to aggregated and disaggregated cross-sectional data, and reviews previous studies of participation. The third chapter contains Cain's very extensive findings, and the fourth summarizes his main conclusions.

While the book is clearly written, the sheer volume of regressions summarized in Chapter 3 makes that chapter rather hard to navigate, and so does the fine print in some of the tables. (Table 20, for example, manages to compress all of the coefficients from eight multiple regressions into 3.4 square inches of space—a typographical tour de force that may deserve a prize, but not from the reader.)

Cain's model, constructed on theoretical foundations developed by Jacob Mincer and Marvin Kosters, relates the wife's participation decision to her market and nonmarket earnings opportunities, family income (minus her earnings), and tastes. The problems of finding satisfactory empirical measures of these variables and estimating their effects on participation by means of single-equation, least-squares, multiple regression techniques are carefully explained.

The author's review of the literature does not give the reader much overall perspective on what previous studies have unearthed, but it does provide an excellent critical assessment of their individual achievements and shortcomings. Empirical research in this area—particularly the selection of explanatory variables—is beset with many pitfalls, and Cain points out a number of instructive examples.

Cain's main contribution, of course, lies in his own research findings, and it is a large contribution indeed. On the basis of his analysis of data for metropolitan areas (from the last three censuses of population) and for individual wives (from the Scripps Population Foundation's Growth of American Families Survey and from the One-in-a-Thousand Sample of the 1960 Census), a number of interesting conclusions emerge, only a few of which can be mentioned here.

First, in each of the last three census weeks (1940, 1950, and 1960), the net effect of higher unemployment in a metropolitan area has been to reduce the overall participation rate of married women, even though the disaggregated data show that wives with unemployed husbands are considerably more likely to be in the labor force than are wives whose husbands are employed. There is, of course, no contradiction in these results: the discouragement effect of higher unemployment (operating on all wives) simply overpowers the additional-worker effect (which influences only those wives with jobless husbands).

Second, Cain constructs a large number of estimates of the elasticity of wives' participation rates with respect to wives' earnings and family income minus wives' earnings. Both sets of estimates span a rather wide range (0.4 to 1.0 for the former, -0.3 to -0.7 for the latter), so that Mincer's earlier finding that the wage elasticity substantially exceeds the income elasticity is, in Cain's judgment, "weakened . . . but not overturned" (p. 117). The size of these elasticities has special relevance to the secular uptrend in the participation rate of married women: since both male and female earnings have been

rising at about the same percentage rate, a (positive) wage elasticity that is larger than the (negative) income elasticity would help to explain this trend. Cain concludes, however, that less of this trend can be accounted for by these variables than had previously been believed. Undoubtedly, many other forces have been at work, and Cain emphasizes the need for additional research to find out what they are.

In this connection, it is interesting that Cain finds larger income-elasticity estimates (relative to wage-elasticity estimates) for nonwhite wives than for white wives, a result which is consistent with the slower growth of Negro wives' participation in recent decades. Cain attributes part of this differential rate of growth to the concentration of nonwhite wives in domestic service—a declining trade. But he has no explanation for the white-Negro income-elasticity differential itself.

Cain's work also sheds considerable light on why Negro wives have higher participation rates than white wives—even after adjustments for differences in earnings, family income, husband's employment status, the presence of small children, and the wife's schooling have all been taken into account. He finds evidence that this persistent differential is largely attributable to (1) the greater prevalence of part-time work by Negro wives; (2) the greater incidence of "poorer housing conditions, smaller dwelling units, and more doubling up" among nonwhite families (p. 119)—all leading nonwhite wives to seek more work in the labor market since there is less for them to do at home; and (3) the greater chance that a Negro wife has of losing her husband—and having to support her family on her own wages—before she grows old.

One conclusion that appears to be in error concerns the deterring effect of small children in the home on the participation of white and Negro wives. On the basis of his regressions using data from the 1/1000 Sample, Cain concludes that "only when children aged 7 to 11 were present did Negro wives work more readily than white wives" (p. 120), a conclusion that seems to have stemmed from an unfortunate choice of dummy variables and a misreading of their coefficients. The results of independent research by William G. Bowen and myself lead to the opposite inference—that the tendency for Negro married women to have a higher participation rate than their white counterparts is more pronounced in the case of women with children *under six* years of age than for women with older children only or with no children in the home at all. (On the basis of correspondence with Cain, it should be added that he has now reached the same conclusion after ingeniously reworking his data. He will be happy to send a revised section of Table 34 to any interested reader.)

To sum up: the quality of Cain's work is first rate, and he has made a major contribution by his painstaking and incisive analysis of the labor force behavior of married women. More generally, this piece of research displays the praiseworthy qualities of ingenuity, scholarly objectivity, and a willingness to present all of the relevant evidence.

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The Technical Elite. By JAY M. GOULD. New York: Augustus M. Kelley, 1966. Pp. 178. \$5.00.

Thorstein B. Veblen anticipated a continuing and irreconcilable conflict between entrepreneurs and technicians. In Veblen's scheme of things, technicians were engaged in rationalizing production, optimizing use of resources, and maximizing output while entrepreneurs were working to curb production and stifle innovation. The motives of the technicians were attributed to a scientific spirit of inquiry while the motives of the entrepreneurs were attributed to self-interest, i.e. protection of markets by preventing oversupply and distress selling, and protection of invested capital by preventing new ideas from being used to undermine the value of such invested capital. Among the conclusions that Veblen drew from observing this conflict was that production and profits would be maximized if entrepreneurs relinquished their leadership role to technicians, as the technicians were better equipped to manage business enterprises.

In a compact book that was originally planned as an introduction to a new edition of Veblen's "The Engineers and the Price System," Gould reviews the chain of reasoning that led Veblen to his conclusions. Veblen argued that the demands of the marketplace required a continuing "withdrawal" of efficiency by entrepreneurs to offset the productivity advances introduced by technicians. At the same time, Veblen recognized that entrepreneurs were becoming increasingly dependent upon technicians to advance production. What Veblen apparently did not realize, and what Gould demonstrates, is that the technicians are increasingly becoming the entrepreneurs and captains of industry, and in this way, the Veblenian conflict is being reconciled.

In his study of the penetration of business ranks by technicians (persons with a college degree or higher in natural sciences or engineering, or those without such formal education discharging technical functions), Gould shows that technicians dominate the ranks of middle management today, and thus will be the major manpower source for the ranks of top management in the future. Original research on the education and social background of top managers and technicians was drawn upon to demonstrate that the need for technically educated people in business was overcoming barriers to their entrance into the ranks of top management, even when drawn from social classes that traditionally were barred from top management positions. This research was particularly enlightening because it exposed the educational and technical weaknesses of top management as well as their strengths.

A section on productivity gains has a breakdown on employment of technicians in specific industries. The data show a high correlation between rates of gain in value added per employee and employment of technical and scientific personnel. As part of the study of productivity trends, Gould demonstrates that an exponential growth curve for the technical labor force had brought about a steady increase in numbers of technicians for two centuries. Commencing when Benjamin Franklin was apparently the only scientist in the labor force to the present day when there are 1.5 million technical members, the ratio of growth of the technical labor force has been very stable. But contrary to the expectations of many that the technical labor force will grow at

an accelerating pace in the future, Gould assumes that the rate of growth of the technical labor force will taper off in the next few decades because of limitations on the supply of persons capable of absorbing a technical education.

Veblen's antagonism towards the entrepreneurs did not carry so far as to attack the profit mechanism which he saw as a reward for efficiency. In a chapter on the controversies in the U.S.S.R. about the use of the profit mechanism as a measure of economic efficiency, Gould notes ". . . that Russian economists are only beginning to grapple with the true nature of the paradoxical tie that binds the truly efficient management of enterprise . . . to the unmanageable and disruptive forces of scientific creativity, which cannot be planned." Pointing out that "Veblen too saw in the profit margin a device to harness the energies of both to the task of advancing economic development," the author then speculates that ". . . the Russians too will come to accept the use of profit for this purpose, perhaps within some more or less flexible framework provided by a central plan."

In sum, Gould contends that Veblen has won a "quiet victory" because the demands of a highly technical economy have changed the role of technicians from servants to partners of the captains of industry, and that in the future, the technicians will actually be the captains of industry.

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Related Disciplines

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NOTES

EIGHTIETH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Sheraton Park Hotel, Washington, D.C.—December 27-30, 1967

Preliminary Announcement of the Program

Wednesday, December 27, 1967

12:30 P.M. EXECUTIVE COMMITTEE, American Economic Association

9:00 P.M. A HAPPENING, ECONOMICS AND THE ARTS

Thursday, December 28, 1967

8:30 A.M. TRANSPORT FOR ECONOMIC AND SOCIAL DEVELOPMENT (Sponsored jointly with the Transportation and Utilities Group)

(Arranged by WILFRED OWEN, The Brookings Institution)

Chairman: GEORGE W. WILSON, Indiana University

Papers: Simulation of Transport Policy Alternatives for Colombia

PAUL O. ROBERTS, Harvard University

DAVID KRESGE, Council of Economic Advisers

A Transport Financing Model for Development

ALAN A. WALTERS, Massachusetts Institute of Technology and
the University of Birmingham, England

Transport Policies for European Economic Integration

JAMES R. NELSON, Amherst College

Discussants: GUSTAV RANIS, Agency for International Development

BENJAMIN CHINITZ, Brown University

ERIC SCHENKER, University of Wisconsin

GRADUATE STUDENT SESSION (Sponsored jointly by *The American Economist*,
Journal of the Honor Society in Economics and Omicron Delta Epsilon)
(Arranged by DANIEL E. DIAMOND, New York University)

Chairman: EGON NEUBERGER, State University of New York at Stony
Brook

Papers: Increasing Returns to Scale and Market Mechanism

MASAHIKO AOKI, University of Minnesota

An Aircraft Routing Model for the Airline Firm

JAMES C. MILLER III, University of Virginia

A Theory of Economic Growth and Inflation

RICHARD W. MONTGOMERY, Oklahoma State University

Discussants: To be announced

THE EFFECTS OF SHIFTS IN AGGREGATE DEMAND UPON LABOR MIGRATION
AND INCOME DISTRIBUTION (Sponsored jointly with the American Farm
Economic Association)

Chairman: C. E. BISHOP, University of North Carolina

Papers: Farm-Nonfarm Labor Mobility

DALE E. HATHAWAY, Michigan State University

BRIAN B. PERKINS, University of Guelph

Effects of Shifts in Aggregate Demand Upon Income Distribution

HYMAN P. MINSKY, Washington University

Discussant: LOWELL E. GALLAWAY, Ohio University

10:30 A.M. ROUND TABLE ON THE ECONOMICS OF HIGHER EDUCATION

Moderator: CHARLES C. KILLINGSWORTH, Michigan State University

Panel: CLARK KERR, University of California, Berkeley

WILBUR J. COHEN, U.S. Department of Health, Education and Welfare

ALLAN M. CARTTER, New York University

GERARD BRANNON, U.S. Treasury

ROUND TABLE ON THE ECONOMICS OF ENVIRONMENTAL MANAGEMENT

Moderator: JOSEPH L. FISHER, Resources for the Future, Inc.

Panel: JACK W. CARLSON, Council of Economic Advisers

M. MASON GAFFNEY, University of Wisconsin (Milwaukee)

MORRIS E. GARNSEY, University of Colorado

LESTER B. LAVE, Carnegie-Mellon University

INTERNAL AND EXTERNAL LABOR MARKETS (Sponsored jointly with the Industrial Relations Research Association)

Chairman: ARNOLD R. WEBER, University of Chicago

Papers: Voluntary Labor Mobility in the U.S. Manufacturing Sector

JOHN F. BURTON, JR., University of Chicago

JOHN E. PARKER, University of Michigan

A Computerized Labor Market Information System

FRAZIER KELLOGG, Massachusetts Institute of Technology

Negro Employment Practices in the Internal Labor Market

HERBERT R. NORTHRUP, University of Pennsylvania

Discussants: To be arranged

2:30 P.M. INTERNATIONAL LIQUIDITY

Chairman: JOHN M. LETICHE, University of California, Berkeley

Papers: The Present State of International Liquidity Theory

RICHARD G. LIPSEY, Essex University, England

The Current Relevance of International Liquidity to Developed Countries

RICHARD N. COOPER, Yale University

The Current Relevance of International Liquidity to Developing Countries

ALEXANDRE KAFKA, University of Virginia

Discussion: The Case of the United Kingdom

ALEXANDER CAIRNCROSS, Adviser United Kingdom Treasury (Invited)

The Case of the Common Market

EMIL VAN LENNEP, Chairman of the Monetary Committee, EEC (Invited)

The Case of the United States

FRANCIS M. BATOR, Harvard University

Synthesis and Appraisal

JOHN M. LETICHE, University of California, Berkeley

2:30 P.M. BEHAVIORAL AND ECOLOGICAL ECONOMICS

Chairman: KERMIT GORDON, The Brookings Institution

Papers: Consumer Behavior: Theory and Findings on Expectations and Aspirations

GEORGE KATONA, University of Michigan

The Supply of Effort and the Interpretation of Inequality

JAMES N. MORGAN, University of Michigan

Ecological Processes in Economic Change: Models, Measurement and Meaning

RUTH P. MACK, Institute of Public Administration

THE AMERICAN ECONOMIC REVIEW

Discussants: PETER E. DE JANOSI, Ford Foundation

HAROLD W. WATTS, University of Wisconsin

ECONOMICS OF ARMS CONTROL AND DISARMAMENT

Chairman: ARCHIBALD S. ALEXANDER, U.S. Arms Control and Disarmament Agency

Papers: The Monetary and Real Costs of National Defense

EMILE BENOIT, Columbia University

Economic Sanctions and Rewards in Support of Disarmament Treaties

MORRIS BORNSTEIN, University of Michigan

Arms and the American Economy: A Domestic Convergence Hypothesis

MURRAY WEIDENBAUM, Washington University

Discussants: WILLIAM CAPRON, The Brookings Institution

HOLLAND HUNTER, Haverford College

8:00 P.M. RICHARD T. ELY LECTURE

Chairman: KENNETH E. BOULDING, University of Michigan and University of Colorado

The Economics of Inquiring, Communicating and Deciding

JACOB MARSCHAK, University of California, Los Angeles

Friday, December 29, 1967

8:30 A.M. MEASUREMENT FOR PUBLIC POLICY (Sponsored jointly with American Statistical Association)

Chairman: WILLIAM CAPRON, The Brookings Institution

Papers: Analysis for Health Planning

ROBERT N. GROSSE, U. S. Department of Health, Education and Welfare

Supply and Demand Analysis of Public Timber

HOWARD W. HJORT, U.S. Department of Agriculture.

Policy Objectives and Aggregative Measurements

NESTOR E. TERLECKYJ, U.S. Bureau of the Budget

Discussants: OTTO ECKSTEIN, Harvard University

EDWARD FOSTER, University of Minnesota

8:30 A.M. EXPERIMENTS IN TEACHING ECONOMICS (Arranged by the American Economic Association Committee on Education, G. L. BACH, Stanford University, Chairman)

Chairman: MARSHALL R. COLBERG, Florida State University

Papers: An Experiment with TIPS, a Computer-Aided Instructional System for Undergraduate Education

ALLEN C. KELLEY, University of Wisconsin

A Simulation Policy Game for Teaching Macroeconomics

RICHARD ATTIEYEH, University of California, San Diego

WILLIAM C. BRAINARD, Yale University

F. T. DOLBEAR, The Brookings Institution

An Experiment with Television in the Elementary Course

CAMPBELL R. MCCONNELL, University of Nebraska

Discussants: MYRON L. JOSEPH, Carnegie-Mellon University

RICHARD S. BOWER, Amos Tuck School, Dartmouth College

HENRY H. VILLARD, City College of New York

THE ENTREPRENEUR

Chairman: ARTHUR H. COLE, Harvard University

Papers: The Entrepreneur in Economic Theory

WILLIAM J. BAUMOL, Princeton University

The Entrepreneur in Economic Development

HARVEY LEIBENSTEIN, Harvard University

The Entrepreneur in Economic History

JAMES H. SOLTOW, Michigan State University

Discussants: EVSEY D. DOMAR, Massachusetts Institute of Technology

EVERETT E. HAGEN, Massachusetts Institute of Technology

ALEXANDER GERSCHENKRON, Harvard University

10:30 A.M. ROUND TABLE ON THE GRANTS ECONOMY

Chairman: MARSHALL A. ROBINSON, The Ford Foundation*Panel:* JOSEPH A. PECHMAN, The Brookings Institution

JAMES M. BUCHANAN, University of Virginia

KENNETH E. BOULDING, University of Michigan and University of Colorado

ROUND TABLE ON THE SCIENTIFIC-MILITARY-INDUSTRIAL COMPLEX AND THE NEW INDUSTRIAL STATE

Chairman: EDWARD S. MASON, Harvard University*Panel:* WALTER ADAMS, Michigan State University

GEORGE J. STIGLER, University of Chicago

H. L. NIEBURG, University of Wisconsin, Milwaukee

SEYMOUR MELMAN, Columbia University

12:30 P.M. JOINT AMERICAN ECONOMIC ASSOCIATION—AMERICAN FINANCE ASSOCIATION LUNCHEON

The Economic Outlook: An International View

MILTON GILBERT, Bank for International Settlements

2:30 P.M. THE ECONOMIC IMPLICATIONS OF 3 PERCENT GROWTH (Sponsored jointly with the Society for the History of Technology)

Chairman: ROBERT L. HEILBRONER, New School for Social Research*Paper:* Technological Possibilities for 3 Percent Growth

MICHAEL MICHAELIS, Arthur D. Little Inc.

Discussant: JOSEPH L. FISHER, Resources for the Future, Inc.*Paper:* Economic Implications for Consumption of 3 Percent Growth

HENRY H. VILLARD, City College of New York

Discussants: VICTOR FUCHS, National Bureau of Economic Research

BEN G. SELIGMAN, University of Massachusetts

ECONOMETRIC MODELS, THEIR PROBLEMS AND USEFULNESS (Sponsored jointly with the Econometric Society)

Session in honor of the 100th Anniversary of the Birth of Irving Fisher

(Arranged by ALBERT ANDO, Massachusetts Institute of Technology)

Chairman: JACOB MARSCHAK, University of California, Los Angeles*Papers:* Pitfalls in Financial Model Building

JAMES TOBIN, Yale University

WILLIAM C. BRAINARD, Yale University

FRB-MIT Econometric Model: Its Special Features and Implications for Stabilization Policies

HAROLD SHAPIRO, University of Michigan

Discussant: JOHN LINTNER, Harvard University

8:00 P.M. PRESIDENTIAL ADDRESS

Chairman: ARTHUR F. BURNS, Columbia University

Can Full Employment Be a Criterion of Monetary Policy?

MILTON FRIEDMAN, University of Chicago

Saturday, December 30, 1967

8:30 A.M. REGIONAL ECONOMIC MODELS

Chairman: RICHARD RUGGLES, Yale University*Papers:* An Open Growth Model for a Regional Economy

GEORGE H. BORTS, Brown University

Regional Impact Models

WERNER Z. HIRSCH, University of California, Los Angeles

An Intra-Regional Simulation Model

JOHN MEYER, Harvard University

JOHN KAIN, Harvard University

Discussants: to be arranged

ECONOMICS AND NON-ECONOMICS OF POVERTY

Chairman: MARTIN COHNSTAEDT, University of Saskatchewan, Regina*Papers:* A Clinical Economist in Rural Poverty

WILLIAM H. COOPER, Bucknell University

Casualty Rates and the War on Poverty

ROBBIN R. HOUGH, Oakland University

The Natural Law of Human Reproduction

RICHARD A. LABARGE, Louisiana State University, New Orleans

Discussants: JAMES D. SMITH, Office of Economic Opportunity

WILLIAM B. NEENAN, University of Michigan

HERMAN G. BERKMAN, New York University

INVITED STUDENT DISSERTATIONS

Chairman: FRITZ MACHLUP, Princeton University*Papers:* Terms of Trade and Economic Development: A Case Study of IndiaDENNIS R. APPELYARD (Ph.D., University of Michigan, 1966),
University of North Carolina

The Tax Policy in Investment in Manufacturing, 1954-1966

ROBERT M. COEN (Ph.D., Northwestern University, 1967),
Stanford UniversityA Distributed Lag Analysis of Adjustment of Size of Labor Force
in Selected U.S. Major Labor Market AreasMAURICE PFANNESTIEL (Ph.D., Oklahoma State University,
1967), Wichita State University

Information and Market Efficiency

JOHN O. LEDYARD, (Ph.D., Purdue University, 1967), Purdue
University10:30 A.M. ECONOMIC REFORM IN EASTERN EUROPE AND THE U.S.S.R. (Sponsored jointly
by the American Economic Association, the Association for Comparative
Economics and the Association for the Study of Soviet-type Economies)*Chairman:* E. E. HAGEN, Massachusetts Institute of Technology*Papers:* The U.S.S.R.ROBERT W. CAMPBELL, Indiana University
Czechoslovakia

GEORGE J. STALLER, Cornell University

Yugoslavia

BENJAMIN WARD, University of California, Berkeley

Discussants: ABRAM BERGSON, Harvard University

JOHN M. MONTIAS, Yale University

ROUND TABLE ON THE SELECTION AND TRAINING OF FOREIGN GRADUATE
STUDENTS IN ECONOMICS*Chairman:* HAROLD F. WILLIAMSON, Northwestern University*Panel:* RALPH K. DAVIDSON, The Rockefeller Foundation

WYN F. OWEN, University of Colorado

ALBERT G. SIMS, National Association for Foreign Student
Affairs

JAMES S. WORLEY, Vanderbilt University

ROUND TABLE ON PRODUCT QUALITY IN THE AMERICAN ECONOMY (Sponsored
jointly by the American Economic Association and the Commission for
Product Quality, American Society for Quality Control)

Panel to be arranged

12:30 P.M. LUNCHEON JOINTLY SPONSORED WITH THE U.S. NATIONAL COMMISSION FOR UNESCO

Chairman: WILLIAM H. NICHOLLS, Vanderbilt University

Speaker: CHARLES FRANKEL, Assistant Secretary of State for Education and Cultural Affairs

2:30 P.M. PROBLEMS IN PRICING AND GROWTH

Chairman: ROBERT SOLO, Michigan State University

Papers: Relative Prices in Macroeconomic Models

L. R. WEBB, University of Melbourne

Demand and Supply for Securities and Economic Growth

PAUL DAVIDSON, Rutgers—The State University

Pricing and Optimum Size in a Non-Profit Institution: The University

HANS H. JENNY, The College of Wooster

Discussants: to be arranged

2:30 P.M. NON-MARKET DECISION-MAKING

Chairman: GORDON TULLOCK, Rice University

Papers: The Peculiar Economics of Bureaucracy

WILLIAM A. NISKANEN, Institute for Defense Analyses

Some Organizational Influences on Urban Renewal Expenditures

CHARLES PLOTT, Purdue University

Democracy and Duopoly: A Comparison of Models

JAMES M. BUCHANAN, University of Virginia

Discussants: STEPHEN ENKE, General Electric-TEMPO, Santa Barbara

OTTO A. DAVIS, Carnegie-Mellon University

EARL THOMPSON, University of California, Los Angeles

ARTISTS ARISE!

Once more the oeconomical artists assemble at the Christmas Meetings, Sheraton Park Hotel, Washington, D.C., December 28-30, 1967. Members of the Association who have art work to exhibit at the next AEA annual meetings should write at once to William J. Baumol, Department of Economics, Green Annex, Princeton University, Princeton, N.J. Please indicate whether you propose to submit painting, sculpture, or other, the number of pieces, and the approximate size of each.

INTERNATIONAL ECONOMIC ASSOCIATION WORLD CONGRESS

The Third Congress of the International Economic Association will be held in Montreal, Canada, at the Queen Elizabeth Hotel, from September 2 to 7, 1968.

The program will be under the general heading of "The Future of International Economic Relations." Morning sessions will be devoted to the discussion of the main papers, each of which will be introduced by two to three speakers. In the afternoons less formal round table discussions will be under the direction of five chairmen who will be responsible for organizing a number of smaller meetings on various aspects of the topics. A preliminary program follows.

Monday, September 2, 1968

H. MYINT (London School of Economics): International Trade and the Developing Countries

Organization session for afternoon round tables

Presidential Address by PAUL A. SAMUELSON (Massachusetts Institute of Technology)

Tuesday, September 3, 1968

HARRY JOHNSON (London School of Economics and University of Chicago): The Theory of International Trade

Round table discussions on International Trade and the Developing Countries, and on the Theory of International Trade

Wednesday, September 4, 1968

A. NOVE (Glasgow) and I. VAYDA (Budapest): The Problems of East-West Trade
Afternoon free—no round table discussions

Thursday, September 5, 1968

G. ÖHLIN (Stockholm): International Trade in a Non-Laissez-Faire World
Round table discussions on the Problems of East-West Trade, and on International Trade in a Non-Laissez-Faire World

Friday, September 6, 1968

TIBOR SCITOVSKY (University of California, Berkeley): International Liquidity and Basic-Mechanism Reform
Round table discussions on International Liquidity and Basic-Mechanism Reform

Saturday, September 7, 1968

Summary Reports by five chairmen of afternoon round tables and close of Congress at noon.
Participation in the program is now planned to include, among others, Fritz Machlup, Charles P. Kindleberger, Jacques Rueff, Evsey Domar, Robert Triffin, Milton Friedman, Kenneth Boulding, Murray Kemp, J. Bérard, J. M. Montias.

All members of the associations that are affiliated to the International Economic Association are invited to take part in the Congress. On behalf of the IEA, the Montreal Convention and Visitors' Bureau will be responsible for the registration of participants, for the collection of the Congress fee, and for hotel reservations. Those wishing to participate should write as soon as possible to the Montreal Convention and Visitors' Bureau, 1400 Metcalfe, Montreal 2, Quebec, Canada, or to the International Economic Association, 92 rue d'Assas, Paris 6^o, France. It is advisable to register as soon as possible.

FOURTH CONGRESS OF THE INTERNATIONAL ECONOMIC HISTORY ASSOCIATION

The Fourth International Congress of the International Economic History Association will be held on the invitation of the University of Indiana at Bloomington, Indiana, September 9-13, 1968, inclusive.

The following main themes, each encompassing a number of sessions, have been established. The subject of the main theme is followed by the name and address of the organizer of the theme. Those who wish to participate in the work of preparation or in the discussion of a particular theme may contact the appropriate organizer:

Formation and Development of Capitalism: F. RUIZ MARTÍN, University of Bilbao, Spain
Twentieth Century Changes in Economic Structure: P. MATHIAS, Queen's College, Cambridge, England

Technology and Economic Growth: A KLIMA, University of Prague, Czechoslovakia
Consumption and Distribution of Production: W. KULA, University of Warsaw, Poland
Demography: D. EVERSLEY, University of Birmingham, England

Ancient History: I. DIAKONOFF, University of Leningrad, USSR

Relations Between Developed and Underdeveloped Regions: O. KUMAR, 9 Ashoka Road, New Delhi, India

Agrarian Organization and Land Policies on the Frontiers: A. JARA, Universidad de Chile, Santiago, Chile

A detailed circular with a registration blank will be sent out in January 1968 to all the participants of previous Congresses and to anyone else who requests one. Requests should be sent to the Secretary General of the Association, Professor Jean-François Bergier, Université de Genève, rue de Candolle, 1205, Genève, Switzerland.

VISITING SCIENTIST PROGRAM IN ECONOMICS

The American Economic Association has received a grant of \$6,540 from the National Science Foundation to continue its "Visiting Scientist Program in Economics" during the 1967-68 academic year. The purpose of this program is to stimulate improved teaching and interest in modern economics at colleges and universities whose major focus is at the undergraduate level. Visits to about 20 campuses have been provided by the program in 1966-67.

The NSF grant provides financing for visits to such campuses by distinguished economists, who normally will spend a day or a day and a half on such a visit. The program for each visit is planned jointly between the visitor and the host institution; customarily it includes a talk to undergraduate major students, perhaps an informal seminar with faculty and students, discussion of current course and research interests of the host faculty, and other activities as may be desirable. While the ultimate concern is to encourage greater interest in, and better teaching of, economics at the undergraduate level, visitors' talks may be on research, current developments in economics, policy issues, or other topics that may seem appropriate to the needs of the particular institution.

This program is under the general direction of the Association's Committee on Economic Education. Professor Phillip Saunders, Department of Economics, Carnegie Institute of Technology, administers the program, under the policies established by the Committee. Professor Saunders works with an informal roster of economists around the country who may be available for such visits, depending on their own schedules and the degree of common interest between them and the inviting institutions. Institutions are free to suggest visitors they would especially like to have, so long as these are within a radius of 100-200 miles of the campus, in order to minimize traveling costs. The host institution is expected to provide lodging and meals for the visitor when he is on the campus; other costs will be covered by the NSF grant.

Any institution that wants further information on this program, or wishes to submit a request for a "visiting scientist" during 1967-68, is invited to write directly to Professor Saunders at Carnegie Institute of Technology, Pittsburgh, Pennsylvania 15213.

FACILITIES IN HONG KONG FOR ECONOMISTS

The American Economic Association is attempting to bring about closer cooperation between Asian and U.S. economists. AEA members who are planning to visit, teach, or do research in Hong Kong are welcome to call on the following Hong Kong individuals and organizations for assistance: Professors R. Hsia and S. Klein, Economics Dept., Hong Kong University; Director of Studies, Economic Research Center, Chinese University of Hong Kong, Kowloon; Chief Economic Section, U.S. Consulate General, 26 Garden Road; Universities Service Center, 155 Argyle St., Kowloon; Union Research Institute, 9 College Rd., Kowloon; and The Asia Foundation, No. 2 Old Peak Road. Mainland China and Hong Kong economy specialists should note that the Universities Service Center has an excellent research library, facilities for the duplication of research material, and some office and desk space. Similar facilities exist at the Institute of Modern Asian Studies, Hong Kong University, and the Union Research Institute. The U.S. Consulate General sometimes is able to arrange briefings for visiting economists. The Hong Kong Economic Association welcomes academic, government, and business economists to its meetings. Requests for information or the use of facilities should be made as far in advance as possible.

FELLOWSHIPS AND GRANTS

The Social Science Research Council's annual announcement describing fellowships and grants to be awarded in 1967-68 is now ready for distribution. It lists research training fellowships, foreign area fellowships, faculty research grants and grants for research on foreign areas. Inquiries about research training fellowships, offered to postdoctoral and advanced predoctoral candidates, and faculty research grants should be addressed to Social Science Research Council Fellowships and Grants, 230 Park Ave., New York 10017. Inquiries about co-sponsored programs should be addressed to the other offices

indicated below. To avoid missing deadline, correspondence should be initiated no later than early October.

Foreign area fellowships for multidisciplinary predoctoral or postdoctoral training relating to Asia and the Near East, the Soviet Union and Eastern Europe, Africa, Latin America, or Western Europe are under the joint auspices of SSRC and the American Council of Learned Societies. Address inquiries to the Foreign Area Fellowship Program at 444 Madison Ave., New York, N.Y. 10022.

Grants for research on foreign areas will be offered by joint committees of SSRC and ACLS to social scientists holding the Ph.D. degree or its equivalent. Grants for research on Africa, Contemporary China, Latin America, and the Near and Middle East are administered by the SSRC. Grants for Asian and for Slavic and East European Studies are administered by the American Council of Learned Societies, 345 East 46th St., New York, N.Y. 10017.

Announcements

The Johns Hopkins University announces that the Second Conference on the Economics of Health has been scheduled for three days during the late fall of 1968. About fifteen papers will be delivered and discussed, and a volume based upon them will be prepared. The following subject headings, among others, will be covered: (1) Productivity, including cost functions for delivering various forms of health services; (2) Demand for health services, including demographic changes and price and income elasticities; (3) Organizational arrangements, including the structural characteristics of the health services industry from the standpoint of both buyers and sellers; (4) Cost-benefit and cost-effectiveness analyses for health services in this country or abroad; and (5) Prices and financing of health services, including methods of payment, the use of incentives, and the flow of intergovernmental grants-in-aid.

Prospectuses from persons who presently may be working in one of these areas and who may wish to present papers at the Conference are invited for the consideration of the Planning Committee. They may be sent to Mrs. Helen Jaszi, Executive Secretary, Second Conference on the Economics of Health, 4910 Cumberland Ave., Chevy Chase, Maryland 20015.

During November 19-22, 1967, in Washington, D.C., the National Center for Education Statistics, U.S. Office of Education, will sponsor a symposium on "Operations Analysis of Education." Further information may be obtained by writing Dr. Stephen C. Clark, Program Chairman, Division of Operations Analysis, NCES, U.S. Office of Education, Washington, D.C. 20202.

The Committee on Shipping of the United Nations Conference on Trade and Development is making a series of studies in shipping economics. Research is being undertaken on the determination and impact of freight rates, port economics, the economic problems and consequences of developing national merchant marines, and consultation machinery between shipowners and shippers. Parallel studies in the fields of shipping statistics and the economic implications of technological changes are also in progress. It is intended to arrange international seminars at which work in all these fields may be discussed.

To enhance the value of this research and to contribute to similar work elsewhere, organizations and persons engaged in research on any aspect of shipping economics are invited to communicate with the Secretary-General of UNCTAD, Palais des Nations, Geneva. He hopes that such contacts and the discussions and exchanges to which they may give rise will be to the benefit of all concerned with research in this field.

Deaths

Henry F. Andersson, associate professor of business administration, Long Beach State College, February 9, 1967.

Ralph E. Freeman, professor emeritus, Massachusetts Institute of Technology, May 12, 1967.

Martin G. Glaeser, emeritus professor of economics, University of Wisconsin, March 19, 1967.

Naum Jasny, Washington, D.C., April 22, 1967.

Ellis Newsome, associate professor of marketing and advertising, University of Iowa, March 12, 1967.

Albert B. Wolfe, professor emeritus, Ohio State University, June 3, 1967.

Retirements

Thomas J. Anderson, Jr., professor of economics, School of Commerce, New York University.

John C. Blommestein, assistant professor of economics, University of Notre Dame, June 1967.

Frank E. Dykema, professor of economics, University of Alabama.

John S. Gambs, professor of economics, Hamilton College, June 1967.

Simon Naidel, professor of economics, American University.

W. A. Neiswanger, professor of economics, University of Illinois, Urbana.

Paul W. Paustian, professor of economics, University of Alabama.

Paul N. Rosenstein-Rodan, professor of economics, Massachusetts Institute of Technology, June 1967.

Reginald H. Scott, assistant professor of economics, University of Pittsburgh, April 15, 1967.

Nelson L. Smith, professor of business, Graduate School of Business, Columbia University, June 30, 1967.

Ruth J. Woodruff, professor of economics, Whittemore School of Business and Economics, University of New Hampshire, June 30, 1967.

Visiting Foreign Scholars

Noel G. Butlin, Australian National University: Irving Fisher research professor, department of economics, and research associate, Economic Growth Center, Yale University.

Lauchlin Currie, National University, Colombia: visiting professor of economics, Simon Fraser University, 1967-68.

Malcolm Fisher, Cambridge University: visiting professor of economics, University of Chicago, winter 1968.

Flemming Hansen, Copenhagen School of Business Administration: visiting associate professor of business administration, Whittemore School of Business and Economics, University of New Hampshire.

Philip Hansen, University of Exeter: visiting lecturer in economics, University of Michigan, 1967-68.

Werner Hildenbrand, University of Heidelberg at the Technische Hochschule at Karlsruhe: visiting associate professor, department of economics, University of California, Berkeley, fall-winter 1967-68.

Jack Johnston, University of Manchester: distinguished visiting professor of econometrics, Bernard M. Baruch School of Business and Public Administration, and Graduate Center, City University of New York, 1967-68.

Toshinoske Kashiwazaki, Waseda University, Tokyo: visiting professor of economics, Lafayette College, fall 1967.

Erich Klinkmuller: visiting professor of economics, University of Arizona, 1967-68.

Heinz Lampert, Technical University of Berlin: visiting professor of economics, Massachusetts Institute of Technology, fall 1967.

Maurice Levy-Leboyer, Université de Caen: visiting professor of economic history, Massachusetts Institute of Technology, spring 1968.

E. Victor Morgan, University of Manchester: visiting professor, department of economics and commerce, Simon Fraser University, fall 1967.

Gunnar Myrdal, Stockholm University: visiting professor, Ohio State University, fall 1967.

John O'Shaughnessy, Sr., College of Aeronautics, Cranfield, England: visiting associate professor, Graduate School of Business, Columbia University.

Marshall Sarnat, Hebrew University: visiting associate professor of finance, Graduate School of Business Administration, New York University.

David Smyth, University of Birmingham: visiting professor of economics, State University of New York at Buffalo, 1967-68.

T. N. Srinivasan, Indian Statistical Institute: visiting professor of economics, Stanford University.

Bernard H. Slicher van Bath: visiting professor of economics, University of Chicago, 1967-68.

Carl Christian von Weitzsaker, University of Heidelberg: visiting professor of economics, Massachusetts Institute of Technology, spring 1968.

Promotions

Robert F. Adams: associate professor of economics, University of Maryland.

Peter S. Albin: associate professor of economics, Washington Square College, New York University.

Ralph Andreano: professor of economics, University of Wisconsin.

Wallace Barr: professor, department of agricultural economics and rural sociology, Ohio State University.

W. Bruce Bassett: assistant professor, Graduate School of Business, Columbia University.

William F. Beazer: associate professor of economics, University of Virginia.

Laurie D. Belzung: associate professor of economics, University of Houston.

R. Albert Berry: associate professor of economics, Yale University.

Robert C. Bingham: professor of economics, College of Business Administration, Kent State University.

Gordon C. Bjork: associate professor, Graduate School of Business, Columbia University.

Alan A. Brown: associate professor of economics, University of Southern California.

Andrzej Brzeski: associate professor of economics, University of California, Davis.

Omer L. Carey: associate professor of business administration, Washington State University.

Kang Chao: associate professor of economics, University of Wisconsin.

Glen G. Cain: associate professor of economics, University of Wisconsin.

David Cass: associate professor of economics, Yale University.

Janet G. Chapman: professor of economics, University of Pittsburgh.

Charles R. Chittle: assistant professor of economics, Bowling Green University.

George Dalton: professor of economics, Northwestern University, Evanston.

Martin David: professor of economics, University of Wisconsin.

John Deiter: associate professor of economics, University of South Florida.

George Delehanty: associate professor of economics, Northwestern University, Evanston.

John Doll: professor of economics, University of Missouri.

Magdi M. El-Kamash: associate professor of economics, North Carolina State University.

Edgar Feige: associate professor of economics, University of Wisconsin.

Charles Floyd: associate professor, department of banking and finance, College of Business Administration, University of Georgia.

Helmut J. Frank: professor of economics, University of Arizona.

Frank W. Gery: professor of economics, St. Olaf College.

Fred H. Goldner: associate professor, Graduate School of Business, Columbia University.

Jean S. Goodman: associate professor of accounting, University of Maine.

Henry G. Grabowski: assistant professor of economics, Yale University.

Alan G. Green: associate professor of economics, Queen's University, Kingston, Ontario.

Marshall Hall: associate professor of economics, Washington University.

Reed R. Hansen: professor of economics, Washington State University.

Einar Hardin: professor of labor and industrial relations, Michigan State University.

John Henning: associate professor of economics, Syracuse University.

Bernard P. Herber: professor of economics, University of Arizona.

Dale Hiestand: associate professor, Graduate School of Business, Columbia University.

Glenn C. Himes: associate professor, department of agricultural economics and rural sociology, Ohio State University.

William T. Hogan: professor of economics, Fordham University.

David B. Houston: professor of economics, University of Pittsburgh.

John D. Huffnagle: associate professor of economics, College of Business Administration, Kent State University.

Saul Hymans: associate professor of economics, University of Michigan.

Charles H. Ingraham: associate professor, department of agricultural economics and rural sociology, Ohio State University.

Paul E. Junk: professor of economics, University of Missouri.

Ralph S. Kaczor: professor of economics, College of Business Administration, Kent State University.

Allen C. Kelley: associate professor of economics, University of Wisconsin.

Wade R. Kilbride: associate professor of economics, U.S. Air Force Academy.

David Klahr: assistant professor, Graduate School of Business, University of Chicago.

Philip Kotler: professor of marketing, Graduate School of Business Administration, Northwestern University.

E. John Kottman: associate professor of marketing and advertising, University of Iowa.

V. N. Krishnan: professor of economics, Bowling Green University.

Robert A. Lynn: professor of business administration, University of Missouri.

Allan B. Mandelstamm: professor of economics, Michigan State University.

Bruce W. Marion: associate professor, department of agricultural economics and rural sociology, Ohio State University.

Joseph Mason: assistant professor of economics, Boston University.

Jon P. McConnell: associate professor of business administration, Washington State University.

R. Marvin McInnis: associate professor of economics, Queen's University, Kingston, Ontario.

Robert W. McKenzie: associate professor of finance, Graduate School of Business Administration, New York University.

Peter M. Mieszkowski: associate professor of economics, Yale University.

Deborah D. Milenkovich: assistant professor of economics, Barnard College.

David W. Miller: professor, Graduate School of Business, Columbia University.

Richard A. Miller: associate professor of economics, Wesleyan University.

Roger F. Miller: professor of economics, University of Wisconsin.

- Jerry Miner: professor of economics, Syracuse University.
- Herbert Mohring: professor of economics, University of Minnesota.
- Bernard Munk: assistant professor of economics, University of Michigan.
- Robert Murphy: associate professor of economics, University of South Florida.
- William B. Neenan: assistant professor of economics, University of Michigan.
- Joseph Newhouse: associate professor, College of Commerce, West Virginia University.
- Fred Nordhauser: assistant professor of economics, U.S. Air Force Academy.
- G. Warren Nutter: Paul Goodloe McIntire professor of economics, University of Virginia.
- Martin P. Oettinger: associate professor of economics, University of California, Davis.
- Ernest C. Pasour, Jr.: associate professor of economics, North Carolina State University.
- Ralph J. Peeler, Jr.: associate professor of economics, North Carolina State University.
- Martin M. Perlman: associate professor of economics, Wichita State University.
- Donald R. Plane: associate professor of economics, U.S. Air Force Academy.
- John Quigley: assistant professor of accounting, School of Business Administration, American University.
- Kenneth J. Rothwell: professor of economics, Whittemore School of Business and Economics, University of New Hampshire.
- Peter P. Schoderbek: associate professor of production management, University of Iowa.
- Harold T. Shapiro: associate professor of economics, University of Michigan.
- Karl Shell: associate professor of economics, Massachusetts Institute of Technology.
- Kenneth L. Shellhammer: assistant professor, College of Commerce, Regional Research Institute, West Virginia University.
- Hirofumi Shibata: associate professor of economics, Queen's University, Kingston, Ontario.
- Irwin Silberman: associate professor of banking, School of Commerce, New York University.
- Hugo Sonnenschein: associate professor of economics, University of Minnesota.
- Clarence A. Spencer, Jr.: assistant professor of finance, School of Commerce and Business Administration, University of Alabama.
- Charles F. Stewart: professor, Graduate School of Business, Columbia University.
- Bernt P. Stigum: associate professor of economics, Cornell University.
- Peter Temin: associate professor of economic history, Massachusetts Institute of Technology.
- Paul R. Thomas: assistant professor, department of agricultural economics and rural sociology, Ohio State University.
- Richard Thorn: professor of economics, University of Pittsburgh.
- Arthur B. Treadway: assistant professor of economics, Northwestern University, Evanston.
- Elias H. Tuma: associate professor of economics, University of California, Davis.
- Carl B. Turner: associate professor of economics, North Carolina State University.
- Arlon R. Tussing: associate professor of economics, University of Alaska.
- Kenneth P. Uhl: professor of marketing and consumer behavior, University of Iowa.
- J. F. Vallery, Jr.: assistant professor of economics, University of Alabama.
- Richard Wallace: associate professor of economics and community health and medical practice, University of Missouri.
- Leon L. Wegge: associate professor of economics, University of California, Davis.
- Finis R. Welch: associate professor of economics, Southern Methodist University.
- Charles K. Wilber: associate professor of economics, American University.

Paul Wonnacott: professor of economics, University of Maryland.

Patrick Yeung: assistant professor of economics, University of Illinois, Urbana.

Edward Zabel: professor of economics, University of Rochester.

Jack Zwick: associate professor, Graduate School of Business, Columbia University.

Administrative Appointments

Khamis Abdul-Magid: chairman and professor, department of economics, Guilford College.

Hugh G. J. Aitken: chairman, department of economics, Amherst College.

Merrill D. Bartlett: associate professor and assistant dean, College of Business Administration, University of Maine.

Louis F. Biagioni: associate professor and chairman, department of accounting, University of Iowa.

Charles E. Bishop, former head of department of economics, North Carolina State University: vice president of public service programs, Consolidated University of North Carolina, Chapel Hill.

Harvey E. Brazer: chairman and professor, department of economics, University of Michigan, 1967-70.

Frank P. R. Brechling: chairman, department of economics, Northwestern University.

Robert D. Calkins, retiring president of Brookings Institution: vice chancellor, social sciences, and professor of economics, University of California, Santa Cruz.

Edwin G. Caudill: assistant professor of business administration and assistant director of doctoral program, American University.

Jan E. Clee: dean, Whittemore School of Business and Economics, University of New Hampshire.

Tom E. Davis: professor and chairman, department of economics, Cornell University.

Hugh N. Emerson, Albright College: professor of economics and dean, business administration, New Haven College.

James S. Earley, University of Wisconsin: chairman, department of economics, University of California, Riverside.

Frederic H. Glade, Jr.: assistant dean, School of Business Administration, American University.

Kermit Gordon: president, Brookings Institution.

Daniel Hamberg: chairman, department of economics, State University of New York, Buffalo.

Niles M. Hansen, University of Texas: associate professor of economics and associate director, Center for Development Change, University of Kentucky.

William T. Hicks, University of Mississippi: chief, Economics Branch, Mississippi River Commission and U.S. Army Corps of Engineers, Vicksburg.

Richard H. Holton: dean, School of Business Administration, University of California, Berkeley.

John W. Hooper: chairman, department of economics, University of California, San Diego.

Edward Samuel Howle: assistant professor of economics and assistant dean of the Graduate School, University of North Carolina.

Max B. Jones: acting chairman, department of business management, Old Dominion College.

James R. Longstreet, University of Pennsylvania: professor and chairman, department of finance, College of Business Administration, University of South Florida.

John E. MacNab: associate professor of economics and chairman, division of social science, Algoma College, Sault Ste. Marie.

Ray Marshall, University of Texas: Alumni professor and chairman, department of economics, University of Kentucky.

Robert H. Marshall: head of department and professor of economics, University of Arizona.

Robert A. Minick: associate professor and chairman, department of economics, Fresno State College.

Gail E. Mullin: associate professor of economics and associate dean and director of graduate programs, College of Business Administration, Kent State University.

Wladimir Naleszkiewicz: acting manager, Economic Analysis and Forecasting Department, Financial Planning Division, Communications Satellite Corporation, Washington, D. C.

Dick Netzer: head, department of economics, Graduate School of Arts and Science, New York University.

Merton J. Peck: chairman, department of economics, Yale University.

Svetozar Pejovich: coordinator of graduate programs, department of economics, Texas A&M University.

Gustav Ranis: director, Economic Growth Center, Yale University.

Laurence C. Rosenberg: director, Office of Planning and Research, Treasury Department, Bureau of Customs, Washington, D. C.

Milton R. Russell: associate professor and chairman, department of economics, Southern Illinois University.

Herbert E. Scarf: director, Cowles Foundation for Research in Economics, Yale University.

John C. Shearer: professor of economics and director, Manpower Training and Research Center, Oklahoma State University.

William D. Shipman: chairman, department of economics, Bowdoin College.

Norman Simler: chairman, department of economics, University of Minnesota.

Irvin Sobel, Washington University: chairman and professor, department of economics, Florida State University.

Harold M. Somers: dean, division of social sciences, University of California, Los Angeles.

Leo T. Surla, Jr.: head, Economic Development Program, Regional Service Institute, Southern Colorado State College, Pueblo.

William D. Toussaint: head, department of economics, North Carolina State University.

Louis D. Volpp, University of Illinois: professor and chairman, department of business administration, Duke University.

Marshall D. Wattles: associate dean of faculties, University of Oregon.

James H. Weaver: chairman, department of economics, American University.

Sidney Wertimer, Jr.: Leavenworth professor and head, department of economics, Hamilton College.

Stephen T. Worland: acting chairman, department of economics, University of Notre Dame.

James E. Young, University of Arizona: professor of economics and director, Bureau of Economic and Business Research, College of Business Administration, Kent State University.

Appointments

Brian H. Aby: economist, Economic Development Administration, U.S. Department of Commerce.

Hamdy H. Afifi: lecturer in economics, University of Illinois.

Dennis J. Aigner, University of Illinois: associate professor of economics, University of Wisconsin.

- Robert J. Allison: assistant professor of economics, Fresno State College.
- Guvenc Alpander, University of Maine: assistant professor, department of economics and commerce, Simon Fraser University.
- Jay K. Alperin: instructor in business administration, University of Maine.
- Edward Altman: assistant professor of finance, Graduate School of Business Administration, New York University.
- John A. Anderson: instructor in economics, University of South Florida.
- Henry G. Aubrey, Sarah Lawrence College: senior fellow, European Institute, Columbia University, 1967-69.
- Harvey Babiak: lecturer in accounting, Graduate School of Business, University of Chicago.
- Trevor Bain, University of Arizona: visiting associate professor of economics, University of Michigan.
- Helmy H. Baligh, University of Illinois: associate professor of economics and business administration, Duke University.
- Solomon Barkin, OECD, Paris: professor of economics and associate of the Labor Center, University of Massachusetts.
- Martha V. Barnett, Washington College: lecturer in economics, Vassar College.
- Steven L. Barsby: assistant professor of economics, University of Arizona.
- Peter S. Barth: visiting assistant professor of industrial relations, Graduate School of Business, University of Chicago, winter quarter 1968.
- Allen M. Beals, Jr.: instructor in economics, North Carolina State University.
- William J. Beeman: assistant professor of economics, University of Maine.
- Carl R. Beidleman: instructor in finance, Lehigh University.
- Robert D. Beland: instructor in economics, U.S. Air Force Academy.
- Frederick W. Bell: associate professor of economics, Clark University.
- Arthur Benavie: associate professor of economics, University of North Carolina.
- Lee Benham: instructor in business economics, Graduate School of Business, University of Chicago.
- Sylvester E. Berki, Cornell University: Michigan State University.
- Irwin Bernhardt, Carnegie Institute of Technology: assistant professor of economics, Pennsylvania State University.
- Richard Bilas, University of Southern California: associate professor, Georgia State College.
- Charles W. Bischoff: research staff economist and lecturer in economics, Yale University.
- Claude A. Bitner: associate professor of economics, University of Houston.
- W. E. Black, Air Staff College: assistant professor of finance, College of Business Administration, University of Georgia.
- Bernard H. Booms: assistant professor of economics, Pennsylvania State University.
- Elbert V. Boden: associate professor of economics, Texas A&M University.
- David F. Bramhall, Johns Hopkins University: professor of economics, University of Pittsburgh.
- John Brandl, St. John's University: assistant professor of economics, University of Wisconsin.
- Phillip D. Brooks: assistant professor of economics, University of Kentucky.
- Byron B. Brown, Jr., Wisconsin State University: associate professor of economics, East Carolina College.
- Murray Brown, George Washington University: professor of economics, State University of New York at Buffalo.
- Herrington J. Bryce: assistant professor of economics, Clark University.

Phillip J. Bryson: assistant professor of economics, University of Arizona.

Robert C. Bushnell, Oberlin University: assistant professor of economics, Ohio State University.

Stewart E. Butler, University of Rhode Island: assistant professor of economics, Hamilton College.

Ralph N. Calkins: associate professor of economics, University of Alaska.

Joseph Caltigirone: assistant professor of finance, Graduate School of Business Administration, New York University.

J. Martin Carovano: assistant professor of economics, Hamilton College.

Robert C. Cauthorn: associate professor of economics, University of Arizona.

Neil Chamberlain, Yale University: professor of business, Graduate School of Business, Columbia University.

Winston Chang, University of Rochester: assistant professor of economics, State University of New York at Buffalo.

John H. Chapman, Jr., West Virginia University: associate professor of economics, Coe College.

Samuel B. Chase: professor of economics, University of Montana.

Walter A. Chudson, United Nations Industrial Development Organization: professor of business, Graduate School of Business, Columbia University.

W. Douglas Cooper: instructor in economics, North Carolina State University.

Dennis R. Cox: assistant professor of economics, University of Arizona.

Helen Crafton: lecturer in economics, University of Michigan.

Robert Crawford: management specialist, College of Commerce, West Virginia University.

William J. E. Crissy, Michigan State University: visiting professor, department of economics and commerce, Simon Fraser University, spring 1968.

William M. Crosswhite: associate professor of economics, North Carolina State University.

Larry L. Cummings, Indiana University: visiting associate professor, Graduate School of Business, Columbia University.

William W. Curtis: assistant professor of economics, University of Montana.

James M. Cypher: lecturer in economics, Fresno State College.

Norman E. Daniel, University of Alabama: associate professor of transportation, University of Houston.

James R. Davis: assistant professor of economics, Iowa State University.

Robert K. Davis, Resources for the Future, Incorporated: economic adviser to Kenya's Ministry of Tourism and Wildlife.

J. D. DeForest: consulting economist, staff of Deputy Assistant Secretary for Policy Coordination, Economic Development Administration, U.S. Department of Commerce.

Thomas R. De Gregori: assistant professor of economics, University of Houston.

Jacob De Rooy: instructor in economics, Lehigh University.

Floyd R. Dill: assistant professor of economics, Washington State University.

Robert R. Dince: regional economist, Sixth National Bank Region: resigning professorship but remaining head, department of banking and finance, University of Georgia.

Alberto DiPierro: assistant professor of economics, University of Virginia.

William P. Dommermuth: associate professor of marketing, University of Iowa.

Louis A. Dow: professor of economics, University of Houston.

Thomas D. Duchesneau: assistant professor of economics, University of Maine.

Dennis C. Duell: assistant professor of economics, Wichita State University.

Dean Dutton: assistant professor of economics, University of Iowa.

William Dwyer: instructor in economics, Lafayette College.

Peter C. Eckstein: assistant professor of economics and research associate, Center for Research on Economic Development, University of Michigan.

Mathew D. Edel, Yale University: assistant professor of economics, Massachusetts Institute of Technology.

Francis J. Egan, College of New Rochelle: instructor in economics, Trinity College.

Kenneth Elzinger: assistant professor of economics, University of Virginia.

John Fair, University of Illinois at Chicago: assistant professor of finance, College of Business Administration, University of Georgia.

Bernhard Felderer: lecturer in economics, University of North Carolina.

Max E. Fieser: senior associate, Robert R. Nathan Associates, Washington, D. C.

H. Gordon Fitch: assistant professor of business administration, School of Business, University of Kansas.

Max E. Fletcher: visiting professor of economics, University of Texas.

Ivan Flores: visiting professor of computer methodology and statistics, Bernard M. Baruch School of Business and Public Administration, City University of New York.

Peter A. Formuzis, Jr.: assistant professor of economics, University of Montana.

Charles R. Frank, Jr., Yale University: associate professor of economics and international affairs, Princeton University.

Richard B. Freeman: lecturer in economics, Yale University.

Peter Frevert: associate professor of economics, University of Kansas.

Klaus K. J. Friedrich: assistant professor of economics, Pennsylvania State University.

A. Blake Friscia: associate economist, Chase Manhattan Bank, N. A.

Rolf Funck: visiting associate professor of economics, University of North Carolina.

David Funk: lecturer in economics, Boston University.

James Funna: instructor in economics, Boston University.

Eirik G. Furubotn: professor of economics, Texas A&M University.

Bradley Gale: assistant professor of economics, University of Massachusetts.

Gerald Garb: professor of economics, Lehigh University.

Paul D. Gayer: lecturer in economics, Yale University.

George R. Gebhart: assistant professor of economics, University of Texas.

Hourmouzis Georgiadis: associate professor of economics, New York University.

Shaikh Ghazanfar: instructor in economics, Washington State University.

Roland Gibson, retiring chairman, department of economics, Washington College: New Hampshire College of Accounting and Commerce.

Herman L. Gilster: instructor in economics, U.S. Air Force Academy.

Victor P. Goldberg: assistant professor of economics, University of California, Davis.

Kenneth D. Goldin: assistant professor of economics, University of California, Davis.

Henry N. Goldstein: associate professor of economics, University of Oregon.

Dale I. Gramley: instructor in economics, North Carolina State University.

William P. Gramm: assistant professor of economics, Texas A&M University.

Harry Grubert, University of Rochester: Bureau of the Budget, Washington, D. C.

Gerald A. Gunderson: assistant professor of economics, University of Massachusetts.

K. L. Gupta: assistant professor of economics, University of Saskatchewan.

Paul F. Hass, U.S. Army: assistant professor of economics, Bowling Green University.

Robert E. Hall: acting assistant professor of economics, University of California, Berkeley.

Gerald D. Hansen: instructor, department of economics and commerce, Simon Fraser University.

John L. Harpell: assistant professor, College of Commerce, West Virginia University.

William Hamberger, University of North Carolina: associate professor of economics, graduate faculty, New School for Social Research.

K. Peter Harder: assistant professor of economics, Wichita State University.

Leonard J. Hausman: assistant professor of economics, North Carolina State University.

George A. Hay: lecturer in economics, Yale University.

Donald G. Heckerman: instructor in business economics, Graduate School of Business, University of Chicago.

Lloyd A. Helms, retiring dean of the Graduate School, Bowling Green University: professor of economics, Bowling Green University.

Robert Hessen: associate in business, Graduate School of Business, Columbia University.

Gerald F. Higgins: assistant professor of economics, University of Texas.

Reginald V. Hobbah: research scientist, staff of State of Connecticut Research Commission.

James Holmes, Purdue University: assistant professor of economics, State University of New York at Buffalo.

Donald C. Horton, Bowling Green University: Northern Illinois University.

William R. Hosek: assistant professor of economics, Whittemore School of Business and Economics, University of New Hampshire.

H. Keith Hunt: assistant professor of marketing and advertising, University of Iowa.

Zia Husain: lecturer in finance, Graduate School of Business Administration, New York University.

Raymond Jackson: assistant professor of economics, Boston University.

Rosella James, Temple University: assistant professor of economics, University of South Florida.

Jerry J. Jasinowski: instructor in economics, U.S. Air Force Academy.

R. Steven Jenks: assistant professor of business administration, Whittemore School of Business and Economics, University of New Hampshire.

Ivan Johnson: visiting assistant professor of economics, University of Virginia.

Norris O. Johnson: Maine Bankers Association professor of economics, University of Maine.

Shirley Johnson, Washington Square College, New York University: associate professor of economics, Vassar College.

Jon M. Joyce: lecturer in economics, Wesleyan University.

Arnold Katz, Columbia University: associate professor of economics, University of Pittsburgh.

Bernard Katz: instructor in economics, Lafayette College.

Woo Sik Kee: associate professor of economics, College of Commerce, West Virginia University.

Warren J. Keegan: associate in business, Graduate School of Business, Columbia University.

Michael Keenan: assistant professor of finance, Graduate School of Business Administration, New York University.

Jerry S. Kelly, Harvard University: assistant professor of economics, Syracuse University.

Jay B. Kennedy, Arabian American Oil Company: associate professor of economics, University of South Florida.

Kwan S. Kim: assistant professor of economics, University of Notre Dame.

Bruce W. Kimzey: instructor in economics, Washington State University.

Walter J. Klages: assistant professor of economics, University of Alabama.

- Charles Klasson: professor of management, University of Iowa.
- Alvin K. Klevorick: research staff economist and lecturer in economics, Yale University.
- Benjamin E. Laden, Johns Hopkins University: assistant professor of economics, Ohio State University.
- Arthur B. Laffer: instructor in business economics, Graduate School of Business, University of Chicago.
- Jose D. Langier, Michigan State University: visiting assistant professor of economics, Washington University.
- Jerome La Pittus, Cornell University: First National City Bank, New York.
- J. William Leasure, University of North Carolina: associate professor of economics, San Diego State College.
- Francis A. Lees: associate professor of economics, Brooklyn College.
- Nathaniel H. Leff, Harvard University: assistant professor, Graduate School of Business, Columbia University.
- Jane Leuthold: instructor in economics, University of Illinois.
- Irving Leveson: research economist, New York City Health Services Administration.
- David H. Levey: lecturer in economics, Yale University.
- Frank S. Levy: acting assistant professor of economics, University of California, Berkeley.
- Ben W. Lewis: program advisor, International Division, Ford Foundation.
- Raymond Lewis: instructor in accounting and business administration, School of Business Administration, American University.
- William Long, University of California, Berkeley: assistant professor of economics, Cornell University.
- Thomas C. Lowinger: assistant professor of economics, Washington State University.
- Taylor D. MacLafferty: associate professor of business administration, Emory University.
- G. S. Maddala, Stanford University: associate professor, University of Rochester.
- John L. Madden: assistant professor of economics, University of Kentucky.
- John E. Maher, Joint Council on Economic Education: lecturer in economics, Harper College, State University of New York at Binghamton.
- Dennis R. Maki: instructor, department of economics and commerce, Simon Fraser University.
- Fred A. Mangum, Jr.: assistant professor of economics, North Carolina State University.
- Huntley G. Manhertz, National Planning Association: assistant professor of economics, University of Pittsburgh.
- Patrick Mann: assistant professor of economics, College of Commerce, West Virginia University.
- Richard G. Marcis: assistant professor of economics, Bowling Green University.
- Bernard J. Marks, Southern Illinois University: visiting research professor of economics, University of Arizona.
- Jacob Marschak, professor emeritus, University of California at Los Angeles: visiting professor, department of economics and Center for Research in Management Sciences, University of California, Berkeley.
- Joseph B. Mason: lecturer in marketing, School of Commerce and Business Administration, University of Alabama.
- John Masten, University of Kentucky: Georgia Bankers Association professor of banking, College of Business Administration, University of Georgia.
- Charles Maurice: assistant professor of economics, Texas A&M University.

Alex Maurizi: assistant professor of economics, University of Iowa.

Bennett McCallum: assistant professor of economics, University of Virginia.

F. William McElroy, Georgetown University: assistant professor of economics, State University of New York at Buffalo.

William McFarland: assistant professor of economics, University of Michigan.

Martin C. McGuire, Office of the Assistant Secretary of Commerce: associate professor of economics, University of Maryland.

David L. McKee: assistant professor of economics, Kent State University.

William H. McWhinney: visiting associate professor of industrial administration, Graduate School of Business, University of Chicago.

Frederic Meyers, University of California, Los Angeles: visiting professor of industrial relations, Massachusetts Institute of Technology.

Constantine Michalopoulos, Trinity College: assistant professor of economics, Clark University.

Norman C. Miller, Bowling Green University: Carnegie Institute of Technology.

Reuben G. Miller, University of Massachusetts: assistant professor of economics, Smith College.

Richard L. Mills: assistant professor of economics, Whittemore School of Business and Economics, University of New Hampshire.

Albert Mitchell: associate professor of accounting, University of Iowa.

Paul M. Montrone, Office of the Assistant Secretary of Defense (Systems Analysis): assistant professor of business, Graduate School of Business, Columbia University, January 1968.

John Morris: assistant professor of economics, University of Iowa.

Kent D. Nash: instructor in economics, North Carolina State University.

Leo J. Navin: assistant professor of economics, Bowling Green University.

Wallace L. Nelson, Jr.: assistant professor of economics, Wichita State University.

Howard W. Nicholson: professor of economics, Clark University, returning from leave.

John H. Niedercorn: assistant professor of economics and urban and regional planning, University of Southern California.

William D. Nordhaus: research staff economist and lecturer in economics, Yale University.

Gur Ofer, Harvard University: lecturer, Massachusetts Institute of Technology.

Frank Otterstrom: instructor in economics, U.S. Air Force Academy.

George G. C. Parker, Stanford University: associate in business, Graduate School of Business, Columbia University.

Malvika S. Patel, University of Southern California: assistant professor of economics, San Diego State College.

A. Paus-Jenssen: assistant professor of economics, University of Saskatchewan.

Walter H. Pearce, University of Kentucky: visiting lecturer in economics, University of Arizona, spring 1968.

Arnold Pearlman: assistant professor of economics, Hampden-Sydney College.

Carlos Palaez: visiting assistant professor of economics, University of Pittsburgh.

Joseph D. Peno, Michigan State University: instructor in economics, Hamilton College.

Boris P. Pesek, Michigan State University: University of Wisconsin at Milwaukee.

Louis F. Pisciotoli: lecturer in economics, Fresno State College.

Hugh M. Pitcher: assistant professor of economics, University of Kentucky.

James L. Plummer: instructor in economics, U.S. Air Force Academy.

Donald E. Pursell: assistant professor of economics, College of Commerce, West Virginia University.

Robert C. Puth: assistant professor of economics, Whittemore School of Business and Economics, University of New Hampshire.

H. Raymond Radosevich: assistant professor of business administration, School of Business, University of Kansas.

James J. Rakowski: assistant professor of economics, University of Notre Dame.

Roger Ransom, University of Virginia: visiting lecturer in economics, University of California, Berkeley.

T. V. S. Rama Mahan Rao: visiting lecturer in economics, University of Southern California.

L. T. Reeves, Samford University: professor of finance, School of Commerce and Business Administration, University of Alabama.

Ricardo C. Reinoso: instructor in economics, North Carolina State University.

Nagesh S. Revankar, University of Wisconsin: assistant professor of economics, State University of New York at Buffalo.

Michael Rieber: associate professor of economics, University of Missouri.

Blaine Ritts: assistant professor of accounting, University of Iowa.

Stefan H. Robock, Indiana University: professor of business, Graduate School of Business, Columbia University.

Robert J. Rohr, Purdue University: assistant professor of economics, Brown University.

Alfredo Roldan: associate professor of economics, University of Nebraska.

M. Richard Roseman: assistant professor of economics, University of Iowa.

David F. Ross: associate professor of economics, University of Kentucky.

Gilbert L. Rutman, University of Arizona: assistant professor of economics, College of Commerce, West Virginia University.

Krishan G. Saini: assistant professor of economics, University of Texas.

Ryuzo Sato, University of Hawaii: professor of economics, Brown University.

Sidney Schoeffler, University of Massachusetts: General Electric Company, New York.

Myron S. Scholes: instructor in finance, Graduate School of Business, University of Chicago.

Linus E. Schrage: assistant professor of applied mathematics and production management, Graduate School of Business, University of Chicago.

Marc C. Scriven: assistant professor of economics, Washington State University.

Lawrence H. Seltzer, Wayne State University: visiting professor of economics, University of Pennsylvania.

Nathaniel E. Shechter: associate professor of economics, Old Dominion College.

James J. Sherman, University of Buffalo: assistant professor of economics, University of South Florida.

James S. Shulman: visiting professor of management, Massachusetts Institute of Technology.

David L. Siders: instructor in economics, College of William and Mary.

Horst Siebert: visiting lecturer in economics, Texas A&M University.

Bertram Silverman, Cooper Union: assistant professor of economics, Hofstra University.

Barbara Simpson, University of Maryland, Munich: assistant professor of economics, College of William and Mary.

Rafael A. Sison: senior economist, Asian Development Bank, Philippines.

Richard E. Slitor, Office of Tax Analysis, U.S. Treasury: professor of economics, University of Massachusetts.

Wilbur J. Smith: assistant professor of economics, College of Commerce, West Virginia University.

Philip Sorensen, University of California, Santa Barbara: visiting assistant professor of economics, University of California, Berkeley.

Mrs. Graham Spry: visiting associate professor of economics, University of Saskatchewan.

Dennis Stene: instructor in economics, St. Olaf College.

Edmund B. Stewart: instructor in economics, U.S. Air Force Academy.

Clark W. Strausser, University of Minnesota: assistant professor of economics, State University of New York at Buffalo.

Lanny Streeter, Hunter College: assistant professor of economics, University of Miami.

Robert R. Sterling: associate professor of business administration, School of Business, University of Kansas.

Donald L. Sternitzke, University of Arizona: associate professor of economics, Bowling Green University.

Richard A. Stevenson: assistant professor of finance, University of Iowa.

Joseph E. Stiglitz: assistant professor of economics, Yale University.

M. Richard Sussman, Emory University: professor of finance, College of Business Administration, University of Georgia.

Richard C. Sutch: acting assistant professor of economics, University of California, Berkeley.

Leopold Sveikauskas: assistant professor of economics, Brooklyn College.

P. A. V. B. Swamy, University of Wisconsin: assistant professor of economics, State University of New York at Buffalo.

Donald G. Tailby: department of economics, University of New Mexico.

Vito S. Tanzi: associate professor of economics, American University.

Ben B. Tarwater: assistant professor of economics, Bowling Green University.

Rasesh Thakkar, University of Rochester: assistant professor of economics, State University of New York at Buffalo.

Arthur A. Thompson: associate of economics, University of Alabama.

Russell G. Thompson, University of Missouri: associate professor of economics, Texas A&M University.

James Thornblade, Massachusetts Institute of Technology: assistant professor of economics, Syracuse University.

Victor Tidwell: associate professor of accounting, University of Iowa.

Douglas J. Tigert: assistant professor of marketing, Graduate School of Business, University of Chicago.

Ralph D. Tomkin: research associate, department of economics, Iowa State University.

John J. Treacy, Texas A&M University: associate professor of economics, Wright State University, Dayton.

Jack E. Triplett, University of Oregon: assistant professor of economics, Washington University.

Edwin M. Truman: lecturer in economics, Yale University.

Allen T. Unsworth: instructor in economics, Pennsylvania State University.

Myron Uretsky, University of Illinois: visiting associate professor of business, Graduate School of Business, Columbia University.

Dan Usher, Graduate School of Business, Columbia University: associate professor of economics, Queens University, Kingston, Ontario.

Richard G. J. Vangermeersch: assistant professor of accounting, University of Maine.

C. Daniel Vencill: lecturer in economics, University of California, Davis.

Frenck Waage, University of California: associate in business, Graduate School of Business, Columbia University.

- William P. Wadbrook, Ithaca College: assistant professor of economics, Trinity College.
- John E. Walker: associate professor of economics, Lehigh University.
- John C. Wardlaw, Jr.: assistant professor of economics, Old Dominion College.
- Darwin Wassink: assistant professor of economics, University of Iowa.
- Douglas W. Webbink: lecturer in economics, University of North Carolina.
- John C. Weicher, University of California at Irvine: assistant professor of economics, Ohio State University.
- Robert M. Weidenhammer, University of Pittsburgh: professor of finance, School of Business Administration, American University.
- Steven J. Weiss: visiting lecturer, University of Colorado.
- Thomas Weiss: assistant professor of economics, University of Kansas.
- Martin L. Weitzman: research staff economist and lecturer in economics, Yale University.
- John Whitaker: visiting professor of economics, University of Virginia.
- Gary A. Wicklund: assistant professor of quantitative methods, University of Iowa.
- B. J. Widick, University of Michigan—Wayne State University, Labor and Industrial Relations Institute: visiting associate professor, Graduate School of Business, Columbia University.
- Sam Williamson: assistant professor of economics, University of Iowa.
- James E. Willis: assistant professor of economics, University of Houston.
- John O. Wilson: assistant professor of economics, Yale University.
- Lloyd Woodman, Jr.: instructor in economics, U.S. Air Force Academy.
- Robert G. Wyckham: instructor, department of economics and commerce, Simon Fraser University.
- Richard Y. C. Yin, University of Southern California: associate professor of economics, George Washington University.
- Paul Zarembka: acting assistant professor of economics, University of California, Berkeley.
- James A. Zwerneman: Operations Research and Economics Division of the Research Triangle Institute, North Carolina.

Leaves for Special Appointments

- Edmund L. Auchter, Fresno State College: Office of Planning, Bureau of Viet Namease Affairs, Agency for International Development, 1967-68.
- Vernon M. Briggs, Jr., University of Texas: U.S. Department of Health, Education and Welfare, June 1967-January 1968.
- Sherrill Cleland, Kalamazoo College: visiting professor of economics, University of Beirut, 1967-1969.
- Robert Eisner, Northwestern University: fellow at Center for Advanced Study in the Behavioral Sciences, Palo Alto, January-June, 1968.
- David Felix, Washington University: Center for International Affairs, Harvard University, 1967-68.
- Helmut J. Frank, University of Arizona: University of Denver, Research Institute, 1967-68.
- Izzat I. Ghurani, University of Texas at Arlington: Fulbright lecturer in economics, University of Liberia, Monrovia.
- Bruce Herrick, University of California, Los Angeles: Instituto de Economía and ESCOLATINA, Santiago, Chile.
- Benjamin H. Higgins, University of Texas: professor of economics, University of Montreal, 1967-68.

Gary C. Hufbauer, University of New Mexico: Harvard University Center for International Affairs, Development Advisory Service in Lahore, Pakistan, 1967-1969.

Saul Hymans, University of Michigan: staff member, Council of Economic Advisors, 1967-68.

Paul R. Johnson, North Carolina State University: Economics Department, RAND Corporation, Santa Monica, 1967-68.

C. P. Kindleberger, Massachusetts Institute of Technology: Clark College, Atlanta University Center, 1967-68.

Lawrence R. Klein, University of Pennsylvania: Ford rotating research professor of economics, University of California, Berkeley, January-June 1968.

Sidney Klein, Rutgers University: visiting professor of economics, University of Hong Kong, 1967-68.

Fred D. Levy, Jr., Syracuse University: Agency for International Development, Rio de Janeiro, Brazil.

Bevars D. Mabry, Bowling Green University: visiting professor, Thammasat University, Bangkok, Thailand, 1967-68.

Erwin E. Nemmers, Northwestern University: professorial research fellow in industrial economics, Case Institute of Technology, 1967-68.

Roger G. Noll, California Institute of Technology: professional staff, Council of Economic Advisers.

Bernard Okunsi, Brooklyn College: Makerere University College, Kampala, Uganda. 1967-68.

R. G. Penner, University of Rochester: consultant, government of Tanzania, first semester 1967-68.

Richard C. Porter, University of Michigan: mission to Colombia, Development Advisory Service, Harvard University, 1967-68.

Richard H. Puckett, American University: Federal Reserve Board, 1967-68.

Sherwin Rosen, University of Rochester: National Bureau of Economic Research, 1967-68.

Peter P. Schoderbek, University of Iowa: visiting professor, Michigan State University, 1967-68.

William G. Shepherd, University of Michigan: economic advisor, Antitrust Division, U.S. Department of Justice, 1967-68.

Shih C. Yu, University of Maine: visiting professor of accounting, University of Florida, 1967-68.

Resignations

Herbert A. Adamson, U.S. Air Force Academy, June 1967.

William W. Alberts, Graduate School of Business, University of Chicago, September 30, 1967.

Merrill J. Bateman, U.S. Air Force Academy, June 1967.

Robert E. Coleberd, Jr., Western Maryland College, June 1967.

S. Dasgupta, University of Saskatchewan.

Charles Goetz, University of Illinois.

Robert S. Johnson, University of Virginia.

J. Daniel Khazzoom, New York University.

Robert W. Knapp, Graduate School of Business Administration, University of California, Los Angeles, June 30, 1967.

Leonard J. Konopa: University of Iowa.

Ichirou Inukai: University of Alaska.

Harvey Leibenstein: University of California, Berkeley, June 30, 1967.

Marvin J. Levine, Ohio State University, September 1967.
Paul A. Montavon, University of Notre Dame, June 1967.
Bruce W. Morgan, University of Illinois, Urbana.
Clarence C. Morrison, University of Virginia.
John Peterman, University of Virginia.
Ronald G. Ridker, Syracuse University.
James Robinson, University of Missouri.
Wilson E. Schmidt, George Washington University, June 30, 1967.
James D. Smith, American University.
Gordon L. Tullock, University of Virginia.
Clinton L. Warne, Ohio State University.

SIXTY-FOURTH LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

EDITOR'S NOTE

The present list specifies doctoral degrees conferred during the academic year terminating June 1967. The list excludes theses undertaken in the same period. Last year the Editorial Board decided to terminate its long-standing practice of publishing titles of "Theses in Preparation," and to begin the publication of brief abstracts of completed doctoral dissertations. These decisions were carried out in the September 1966 issue and are continued in this one. We invite comments on and criticism of these decisions.

General Economics; Methodology

MARTIN R. BLYN, Ph.D. New York 1966. Essays in innovation among academic economists.

ROBERT G. FABIAN, Ph.D. Florida 1966. The structure of economic theory and the goals of scientific analysis.

JERRAL C. RAYMOND, Ph.D. Virginia 1967. Factors affecting the production of economic knowledge by American economists.

This study has attempted to investigate the relationship between the production of economic knowledge and five biographical characteristics in the life histories of the individual producers. First a conceptual framework was created by adapting the tools of conventional price theory. Then the framework was applied to actual data, with statistical tests evaluating the results. It was found that four of the five biographical characteristics were associated with higher levels of knowledge production.

Price and Allocation Theory; Related Empirical Studies

GEORGE A. AKERLOF, Ph.D. Mass. Inst. Technology 1967. Wages and capital.

CHARLES BLACKORBY, Ph.D. Johns Hopkins 1967. Rational rules for intertemporal decision making.

The purpose of this thesis is to examine concepts of rationality in intertemporal allocation problems. Using discrete time a consistent path is found, but myopia prevents a consumer from following it. A definition of intertemporal rationality is offered which requires consistency. This definition allows the consumer to avoid his myopia and justifies the consistent path found earlier.

KENNETH M. BROWN, Ph.D. Johns Hopkins 1967. Efficiency, profits, and the regional growth of manufacturing in the United States.

This study attempts to explain interstate differences in growth rates of SIC two-digit industries in the United States during the 1954-62 period. In a simultaneous equation model of the firm, it is shown how a variety of regional differences may affect growth. Of greatest empirical significance is the "efficiency parameter," a broad term which measures differences in output resulting from causes other than differences in quantities of capital and labor inputs.

FREDERICK CLEVELAND, Ph.D. Columbia 1967. Resource allocation in the firm.

MARK DANIELS, Ph.D. Johns Hopkins 1967. International differences in productive efficiency.

The primary objective of the study is to provide some evidence on the nature of efficiency in production among manufacturing industries in developing countries. The gen-

eral method employed is to analyze efficiency estimates obtained by utilizing a CES production function for two-digit manufacturing industries. Results indicate that no country studied clearly has a majority of relatively efficient or inefficient industries, rather industrial efficiency appears to vary systematically across the countries for which data were available.

HAMID HABIBAGAHI, Ph.D. Purdue 1966. Qualitative analysis of dynamic stability in general equilibrium theory.

FRED H. LEONARD, Ph.D. Michigan 1966. An analysis of short-run changes in functional income distribution.

WILLIAM F. E. LONG, Ph.D. George Washington 1967. Price and nonprice practices under the uncertain conditions of rapidly improving technologies—a case study.

The dissertation is a case study of cost-plus pricing to reduce the uncertainty as to which of the several techniques of producing the types of transistors that are used in computers will be the lowest-cost technique. Price is set as a constant function of average variable cost. The trends in price are valid indicators of the relative rates at which technologies are being improved and they therefore identify the surviving technology at an early date.

C. A. KNOX LOVELL, Ph.D. Duke 1966. A comparison of statistical properties of aggregate production functions.

WILLIAM D. NORDHAUS, Ph.D. Mass. Inst. Technology 1967. Theory of endogenous technological change.

NANDIRAJU K. RAO, Ph.D. Stanford 1967. Returns to scale and the production function with an application to the steam power generating plants in the U.S., for the period 1937-1961.

A constant elasticity of substitution production function of two variable inputs which under conditions of cost minimization by the firm implies first increasing and then decreasing returns to scale is derived and applied to the steam electric power generating plants newly built in the U.S., during the period 1937-61. *Ex ante* substitution possibilities, returns-to-scale changes due to technological change and differences in size of plant are studied. Also, the concept of returns-to-scale-envelope curve is introduced.

GEORGE N. REYNOLDS, Ph.D. Tulane 1967. User cost and intertemporal allocation in a holding firm.

A firm summarizes its future profit anticipation from the use of capital in its marginal efficiency of capital. But market prices which the firm faces as the future becomes the present may diverge from expected prices. The marginal efficiency of capital must therefore diverge from initially expected values. Keynes' definition of user costs takes into account this type of risk in planning the use of capital and the dissertation develops the definition in a mathematical model of a firm.

JOHN W. ROWE, JR., Ph.D. Illinois (Urbana) 1966. The production function: its history, properties and importance of the firm.

WILLIAM A. SCHEFFER, Ph.D. Duke 1966. Structural changes and plant-employment proportions, 1954-57.

CHANDLER H. STEVENS, Ph.D. Mass. Inst. Technology 1967. Information and the division of labor.

ARTHUR B. TREADWAY, Ph.D. Chicago 1967. Rational entrepreneurial behavior and the dynamics of investment.

A theoretical study of maximizing entrepreneurial behavior under adjustment costs, including (a) a model of rational excess capacity, (b) analysis of the determinants of the firm's (flow) demand for investment and (c) a derivation of the "flexible accelerator" equation as a linear approximation to an optimal adjustment path with a resulting model of the adjustment coefficient and an evaluation of the hypothesis of its constancy.

JAMES P. WALLACE, III, Ph.D. Brown 1967. Market stability under simultaneous price and quantity adjustment.

A simultaneous price and quantity adjustment model, having the Walrasian and Mar-

shallian processes as degenerate cases, is presented and its stability investigated. The problem of the diametrically opposite stability conditions of Walras and Marshall, when supply is downward sloping, is resolved. Market models are then formulated as Volterra integral equation systems to exhibit mathematically the feedback nature of market adjustment and to increase the adjustment process generality. Stability analysis is performed using theorems of Laplace Transformation.

Income Theory; Related Empirical Studies

THEODORE C. BERGSTROM, Ph.D. Stanford 1967. Human capital in an economic growth model.

A growth model is constructed in which human capital is a factor of production. Human capital need not be a perfect substitute for either raw labor or physical capital. The supply of human capital depends on income with an income elasticity which may exceed unity. The interaction between income and the stock of human capital is explored, stability conditions are derived, and the parameters relevant for stability are estimated.

JAMES R. DAVIS, Ph.D. Virginia 1967. Pre-Keynesian economic policy proposals in the United States during the Great Depression.

Despite modern descriptions to the contrary, leading economists in the United States during the Great Depression were not recommending wage reductions as the solution to unemployment and depression. Instead, most of them prescribed deficit budgets and other expansionary policies. By 1936, the notions of compensatory public spending and counter-cyclical balancing of the budget were commonplace, but Washington was not to be dissuaded from contractionary policies. Accordingly, myths regarding "classical" economists and economic policy during the Depression should be dispelled.

PATRICK B. DEFONTENAY, Ph.D. Yale 1967. Inflation growth and cost structure: an international comparison.

FREDERICK O. GODDARD, Ph.D. Duke 1966. A two-sector model of economic growth with technological progress.

ROBERT E. HALL, Ph.D. Mass. Inst. Technology 1967. Essays on the theory of wealth.

DONALD J. HARRIS, Ph.D. California (Berkeley) 1966. Inflation, capital accumulation, and economic growth: a theoretical and numerical analysis.

HENRY F. LEE, Ph.D. Pennsylvania 1966. Relative prices and cereal consumption functions—Korea, 1952 to 1962.

F. JOHN MATHIS, Ph.D. Iowa (Ames) 1966. The concept of underemployment theory and policy.

EUGENE MELANDER, Ph.D. Minnesota 1966. Longer-term household saving: some models and their empirical evaluation.

D. M. NOWLAN, Ph.D. Toronto 1965. Technical change in models of economic growth.

JOSEPH E. STIGLITZ, Ph.D. Mass. Inst. Technology 1967. Studies in the theory of economic growth and income distribution.

MORRIS F. TEUBAL, Ph.D. Chicago 1967. Inflation, unemployment and economic growth.

The analysis deals with the patterns of growth, inflation and unemployment of a one-sector monetary economy. Money is increased at a constant rate by a monetary authority by means of transfers to the private sector. Particular attention is given to the conditions under which the economy will shift from unemployment to full employment. Alternative assumptions concerning the formation of expectations and the behavior of money wages are postulated.

ADRIAN W. THROOP, Ph.D. Stanford 1967. Sources of inflationary bias in the American economy, 1949-62.

This dissertation analyzes the inflationary bias which has been manifested by the post-war Phillips curve. Alternative hypotheses are tested at several different levels of aggregation. Support is found for the hypothesis that collectively bargained wages fell relative to competitively determined ones during the demand inflation of the 1940's. It is

shown that the ensuing increase in the union-nonunion wage differential was the source of a once and for all cost-push inflation in the 1950's.

TERENCE J. WALES, Ph.D. Mass. Inst. Technology 1967. The effect of accelerated depreciation on investment, an empirical study.

EVANGELOS A. VOLOUDAKIS, Ph.D. Yale 1967. An empirical investigation of the Greek manufacturing investment function.

History of Economic Thought

GHALIB M. A. BAQIR, Ph.D. Saint Louis 1967. The Historical School in economics.

This dissertation has analyzed the rise and the development of the main teachings of the Historical School in economics as they were offered in the writings of its leading members in Germany, Great Britain, and the United States of America. The historical economists in these three countries have rendered a major service not only to historical or institutional economics but also to economic science taken as a whole.

ROBERT B. EKELUND, JR., Ph.D. Louisiana 1967. A critical evaluation of Jules Dupuit's contributions to economic theory and policy.

This study has analyzed in depth the works of the French economist, Jules Dupuit, including major economic works not heretofore available in English translation. Examined in detail are Dupuit's theories of marginal utility, demand and consumers' surplus, monopoly and price discrimination as well as his theory of "marginal cost pricing." In addition to developing original microeconomic tools of great theoretical interest, this research found him to be the first important welfare economist.

JOHN W. MCKINNEY, Ph.D. Columbia 1967. A critique of Frank H. Knight's economic philosophy.

A particular conception of the human mind as creative, the location of free choice and active decision-making, is the foundation of Knight's teaching as an economic theorist, methodologist of social science, and moralist. The study undertakes an appraisal of his ideas in the three fields from the point of view of an alternative conception of the creative mind, one that denies the status of the mental as a separate existence.

ROBIN F. NEILL, Ph.D. Duke 1966. The content and context of the work of Harold Adams Innis.

EDWARD J. POWERS, Ph.D. Boston College 1967. The social economics of John Maurice Clark.

Economic writings of John Maurice Clark are expounded and appraised in three important areas: the theory of overhead cost, business fluctuations, and competition. Clark's work is interpreted in terms of three key analytical concepts he formulated, those of social value, social efficiency and social balance. Clark's emphasis on a broadly conceived and policy-oriented social economics is viewed as his distinctive characteristic, which tends to weaken somewhat the analytic rigor of the Clarkian system.

Economic History; Economic Development; National Economies

GLEN G. ALEXANDRIN, Ph.D. Clark 1967. A study of investment, employment and growth in Newfoundland; with a statistical analysis of the period 1949-1961.

SUPHAN F. ANDIC, Ph.D. Michigan 1967. Fiscal survey of Surinam: planning, economic development, and fiscal requirements.

DENNIS R. APPLEYARD, Ph.D. Michigan 1966. Terms of trade and economic development: a case study of India.

R. M. ARUNACHALAM, Ph.D. Purdue 1966. Credit and capital formation in the agricultural development of four Trichy villages, Madras State, India, 1962-64.

NOEL C. BACCHUS, Ph.D. Fletcher School 1966. A theory of investment priorities for Sierra Leone.

DAVID P. BARKIN, Ph.D. Yale 1967. An economic evaluation of the regional development program for the Tepalcatepec River Basin in Mexico.

GARY W. BICKEL, Ph.D. Stanford 1967. Economic development in Latin America—the effects on economic growth of international trade, trade barriers, and the possibilities offered by international economic integration.

SANJIT BOSE, Ph.D. Mass. Inst. Technology 1967. Essays in development planning.

JOHN L. BRIDGE, Ph.D. Cornell 1966. Aggregate excess demand, and spare capacity and the foreign trade in the United Kingdom, 1954-1964.

ANNE M. BROWN, Ph.D. Texas 1966. Changing patterns of resource use in East Texas.

DAVID J. CANTOR, Ph.D. Harvard 1967. Import-replacing industrialization and economic development in Ghana.

Given markets for manufactures, Ghana stresses import-substitution in its development program. The prospects for its success appear doubtful because of the need for Ghana to maintain exports of primary products to finance capital imports in the short run and raw material inputs in the long run. Therefore, the dual structure of Ghana's economy, in which the majority of the population earn a low and stagnant level of income, is likely to be perpetuated.

VICTOR E. CHILDERS, D.B.A. Indiana 1967. Unemployment in Venezuela.

SOON CHOUGH, Ph.D. California (Berkeley) 1967. Financing of economic development in South Korea, 1954-1964.

JON S. COHEN, Ph.D. California (Berkeley) 1966. Finance and industrialization in Italy, 1894-1914.

FRANCIS X. COLAÇO, Ph.D. California (Berkeley) 1967. Factors affecting changes in the cost of living in Brazil: a sectoral analysis.

APOSTOLOS CONDOS, Ph.D. Iowa (Ames) 1966. The application of microeconomic models to development planning: Peru.

ALBERT L. DANIELSEN, Ph.D. Duke 1966. Education and economic growth in the Philippines.

ROBERT H. DEANS, Ph.D. Pittsburgh 1966. Economic effects of Public Law 480 Title I Local Currency in economic development with special reference to Israel, Colombia, Turkey and Spain.

GHAZI B. DUWAJI, Ph.D. Duke 1966. The Tunisian economy.

SCOTT McN. EDDIE, Ph.D. Mass. Inst. Technology 1967. The role of agriculture in economic development of Hungary, 1867-1913.

EMMANUEL EDOZIEN, Ph.D. Michigan 1966. The impact of direct foreign investment on an underdeveloped economy: the Nigerian case, 1924-1964.

MAGDI M. EL-KAMMASH, Ph.D. Duke 1966. An analysis of the process of development of the Egyptian economy with special reference to the supply approach to economic development.

JOHN R. ERIKSSON, Ph.D. California (Berkeley) 1966. Wage structures and economic development in selected Latin American countries: a comparative analysis.

MARK Z. FABRYCY, Ph.D. City University New York 1967. Determinants of technological advance—twenty countries, 1950 to 1963.

A Cobb-Douglas production function with analysis of covariance was used to measure technological change which was then regressed on four determinant variables. On-the-job-learning was shown to be by far most important. It was then demonstrated that technology may depend to a very important degree on the accumulated effect of capital input and can be expressed in a Cobb-Douglas function by a capital coefficient which is much larger than that of labor.

IRWIN FELLER, Ph.D. Minnesota 1966. Regional variations in the diffusion of innovations: the case of the Draper loom.

Prevailing accounts of the response of New England cotton textile mills to the Draper

loom hold that these mills were slow in responding to the technique and that this hesitation was due to the "conservative" quality of management. The conclusion here is that the response by these mills is explainable in terms of the adjustment behavior expected of firms operating within competitive markets given a new production technique which is a functional rival of installed equipment.

JOSEPH C. FUREY, Ph.D. Maryland 1966. The crisis of feudalism in the fourteenth century in England.

CARL H. GOTSCH, Ph.D. Harvard 1967. Technological change and private investment in agriculture: a case study of the Pakistan Punjab.

The study examines two hypotheses: (1) that peasants in a backward agriculture are poor but efficient, and (2) that the rapid private investment in water resource development observed in recent years in the Punjab is consistent with the idea that peasants are responding to a disequilibrium situation created by the availability of new water producing technology. The investigation was conducted by constructing linear programming models for both "traditional" and "modern" agriculture. Special attention was devoted to incorporating linear approximations of nonlinear water response curves into the programming models.

CYRIL J. GRANT, Ph.D. Purdue 1966. Sources of regional technological change as indicated by patent granting statistics.

NACHUM T. GROSS, Ph.D. California (Berkeley) 1966. Industrialization in Austria in the nineteenth century.

SUMNER J. HOISINGTON, JR., Ph.D. Brown 1967. Predicted private development and prescribed public expenditures in a redevelopment area.

Estimates the rates of return resulting from the public investment required to generate full employment in a redevelopment area, under alternative subsidy programs. An evaluation is made of current policies, and of programs designed to subsidize capital requirements and/or the wage-bill of Rhode Island manufacturing industries over the 1958-1970 period. Estimates are made of three-digit manufacturing employment and total non-manufacturing employment, population and labor force for 1970 in order to provide a basis for determining program requirements.

JARRETT HUDNALL, JR., Ph.D. Alabama 1966. An economic analysis of income and employment in a four-state Deep South region: 1950-1960.

SINDEHA K. HULBE, Ph.D. Texas 1966. Economic development through persuasion: case studies of community development in Ahmednagar Block (India).

AHMED S. ISPAHANI, Ph.D. Southern California 1966. The optimization of economic resources for economic growth in Iran.

Findings seem to indicate that the future of Iran looks encouraging. Possessing substantial human and rich productive mineral resources, Iran has one of the best potentials for advancement among the developing countries. Its petroleum industry is one of the largest in the world. The substantial natural gas reserves, which together with oil, will provide the power for new industry is one of the largest in the world. Mining is in its infancy, but there are indications of considerable wealth here too. The rich fishing resources of the Persian Gulf are largely untapped. Iran has no population growth problem, or likelihood of future pressure on the land. Substantial revenue can be obtained from taxes by reforming the tax structure of Iran. Iran has an enlightened, intellectual, democratic, progressive and far-sighted leader in Reza Shah. Under his leadership, Iran can make solid advantages toward its goal of a modern economy.

ABDEL-FATTAH M. KANDEEL, Ph.D. Southern California 1966. The "Surplus" approach for project appraisal (an application to the Aswan High Dam).

The criterion of consumers' and producers' superior to other criteria in going beyond a simple comparison between costs and outcomes. Although the Dam would result in a net gain, stripping from the project its contribution to industry would have serious effects on its contribution to agriculture. To recapture the loss of potential surplus, a deliberate process to increase national income outside the agricultural sector is required.

S. STANLEY KATZ, Ph.D. American 1966. The external assistance contribution to Indian economic growth and development 1951-1961.

Qualitative and quantitative assessment of the contribution made by external economic assistance to the economic growth and development achieved by India during the 1951-1961 decade of the First and Second Indian Five Year Plans.

DOUGLAS H. KEARE, Ph.D. Princeton 1966. Planning and French economic policy: 1958-1965.

The French planners possess virtually no tools for influencing the economy. They must rely upon persuading others, most notably the Minister of Finance, to use their formidable powers to achieve ends desired by the planners. This has been done with limited success. Planning has not exerted a comprehensive influence upon the conduct of policy. Nonetheless, it has succeeded in promoting certain specific goals and the removal of "structural" obstacles to the development of the economy.

DAVID KLINGAMAN, Ph.D. Virginia 1967. The development of Virginia's coastwise trade and grain trade in the late colonial period.

The coastwise trade and the total grain exports of Virginia were quantified, and the development and growth of these two branches of commerce were analyzed in detail. The average annual rate of growth of both branches of trade surpassed that of the population. Total grain exports to world markets grew especially fast over roughly the last 30 years of the colonial period and compensated the economy in part for the sluggish growth of tobacco exports.

ROBERT O. KRUEGER, Ph.D. Texas 1967. Corporate property and responsibility: an historical and cultural study.

BETTY R. MACLEOD, Ph.D. Duke 1967. A history of Canadian economic development with special reference to immigration.

WAYTIN MALLIKAMAS, Ph.D. Wisconsin 1967. Industrial promotion in Thailand.

LLOYD J. MERCER, Ph.D. Washington 1967. The Central Pacific System: an estimate of social and private rates of return for a land-grant aided railroad system.

This study provides estimates of the private and social rates of return for the Central Pacific Railroad System, 1863-1889. These estimates are used to evaluate some government policies designed to promote growth, particularly the land grant policy. Three rates of return are calculated: (1) Unaided private rate—12.9 per cent; (2) private rate with land grant—14.1 per cent; (3) social rate—28.6 per cent. Even the lowest of the three is much above the market rate of return for the period, 7.5 per cent, indicating that this railroad would have been profitable in the absence of land grants. Still since the social rate of return is much above the private one, to the extent that land grants provided an extra incentive to undertake the railroad construction, it was not an unattractive policy.

MAUNG MYINT, Ph.D. California (Berkeley) 1966. Agriculture in Burmese economic development.

ROBERT T. NEWSOM, Ph.D. Tulane 1966. Changes in the structure of employment in Texas, 1940 to 1960.

The employment shift analysis is the statistical technique utilized to relate the structural changes in the State Economic Areas of Texas to the nationwide structural changes. The goals are: (1) to analyze employment change in each state sub-area in terms of the role of industrial composition and the role of differential growth rates between the sub-area and the nation, and (2) to reveal the patterns of change associated with the economic specialization of sub-areas.

STAHIS S. PANAGIDES, Ph.D. Iowa (Ames) 1967. An econometric study of the Cyprian economy.

RAMANBHAI C. PATEL, Ph.D. Southern California 1966. Applications of stochastic processes to economic development.

This dissertation attempts to develop a theory of economic growth using stochastic processes of the diffusion type. Besides the purpose of the stochastic model of economic

growth to discover and explain the nature of economic growth exhibited by temporally interdependent economic variables, its empirical applications to a wide variety of economies provide a basis of comparison of the growth process for different regions. The empirical results indicate similarity of economic growth although parameter values are different for different situations.

MARGARET H. PFANNER, Ph.D. Cornell 1966. An examination of the postwar shift in the balance of trade of primary producing underdeveloped countries with particular reference to Burma, Ceylon and the Philippines.

JAMES D. RAE, Ph.D. Purdue 1967. Great Lakes commodity trade, 1850-1900.

FRANCIS J. REBELLO, Ph.D. Saint Louis 1967. Macro-economic planning in India.

IDRIAN N. RESNICK, Ph.D. Boston 1967. Two routes to manpower development: the Tanzania case.

SYED I. RIZVI, Ph.D. Southern California 1967. The evaluation of the Second Five Year Plan of Pakistan.

Mechanization of an optimal piece of and installation of a steel industry sufficient for mechanization are very important. Effective "grass roots" participation is a strategic prerequisite for the impartation of flexibility and mobility of factors of production for a self-sustained rate of growth. Measures have been suggested to infuse elasticity, both in taxation and the money supply, coupled with an optimal demarcation of the allocation of scarce resources between the production of consumers' goods and producers' goods.

DANIEL K. ROYER, Ph.D. Florida 1966. Economic development of El Salvador.

A summary of available income and price level statistics for various sectors of the economy during the postwar years, with particular effort to determine the relationship between income/investment ratios for new and for old investment sectors. Numerous unpublished economic development reports were used, and an effort was made to appraise the value of past and future investment in specific projects as well as in general areas of production.

WILLIAM T. RYAN, D.B.A. Indiana 1966. Planning in Peru: an analysis and strategy for development.

JAMES F. SHEPHERD, Ph.D. Washington 1966. A balance of payments for the thirteen colonies—1768-1772.

The general contours of colonial economic development can be understood only when the essential data are obtained. Up until this time such information has been lacking. A crucial aspect of the colonial economy was its economic relationships with the rest of the world. This study for the first time has estimated the values of exports and imports and invisible items in the balance of payments. As a result we can now precisely determine the importance of tobacco, rice, and shipping services in the credits of the balance of payments, and also get an equally precise breakdown of the relative importance of each of the debts.

SAMIR Y. SIDHOM, Ph.D. Southern California 1967. Developmental planning in the United Arab Republic: an economic analysis and evaluation of the Egyptian experience.

The Egyptian experience in comprehensive planning during the First Five Year Plan indicates that it was significantly successful in promoting economic growth, raising the living standard, and attaining a high degree of equitable income distribution. However, the First Plan was not effective in dealing with some development problems including rapid population growth, decreasing the rate of increase in consumption, curbing inflationary pressures, and solving the efficiency problem. Measures to aid future plans are included in the study.

SHELDON R. SIMON, Ph.D. Cornell 1966. Changes in income, consumption, and investment in an Eastern Uttar Pradesh village.

JOHN E. STAHL, Ph.D. Iowa (Ames) 1966. Economic development through land reform in Puerto Rico.

- G. T. STEWART, Ph.D. Alabama 1966. The economic development of Uruguay.
- JAMES A. TERNENT, Ph.D. Oregon 1967. Inflation and private investment in Latin America.
- HELEN S. TICE, Ph.D. Yale 1967. Depreciation, obsolescence, and the measurement of the aggregate capital stock of the United States, 1900-1962.
- MARY B. VANIYAPURAYIL, Ph.D. Boston College 1967. Some aspects of capital formation and its financing in the Indian economy, 1948-49 to 1960-61.

This dissertation analyzes the role of capital formation and its financing in India from 1948-49 to 1960-61. Average and marginal capital-output and saving ratios are analyzed. Investment increased sharply during the period, capital-output ratios rose slightly, and saving ratios fell in the latter years. It is argued that these lower savings ratios in the latter years resulted from lower agricultural production and the shift in the terms of trade against the urban high-saving sector.

- BENSION VARON, Ph.D. Pennsylvania 1967. Export instability and economic development—the Turkish experience 1948-60.

Turkey had one of the lowest foreign trade ratios in the world and her export fluctuations were moderate compared to those of 80 countries. Yet these fluctuations were instrumental in bringing about a slackening in the growth rate of real investment and a deterioration in the productivity of capital. The dissertation analyzes the causes and consequences of export fluctuations with particular emphasis on the major factors which account for their disproportionate repercussions on the Turkish economy.

- JAMES N. VEDDER, Ph.D. Michigan 1966. Improved measurement of state economic growth, its sources, and the impact of state economic development programs.

- BRENDAN M. WALSH, Ph.D. Boston College 1966. An economic model of Ireland 1944-62.

On the basis of historical and institutional material concerning the Irish economy in the years 1944-1962, a model was formulated and statistically tested to study the determination of 21 key macroeconomic variables. Using an industry-agriculture dichotomy, a number of behavioral equations were estimated for each sector. The dependence on external trade was studied and estimates of import and export price elasticities obtained. The model was applied to the simulation of the economy in 1962 and the preparation of forecasts for 1963 and 1964.

- GARY M. WALTON, Ph.D. Washington 1966. A quantitative study of American colonial shipping.

There has been almost no analytical work done exploring the rate of growth or the sources of growth during the first 170 years of colonial existence. This thesis is a study of a major colonial industry—shipping—and provides new information on the volume of shipping, trade routes, and income generated. However, its most important contribution is to provide a rough measure of productivity improvement during that period, and then through an examination of inputs into shipping to explore the sources of that productivity increase. The results of this investigation are twofold: (1) that there was substantial increase in productivity in colonial shipping from the middle of the seventeenth century to the Revolutionary War, and (2) that the decline in piracy and improving economic organization accounts for this growing efficiency. Technological change on the other hand which is usually thought of as the major source of productivity increase plays little or no part.

- FRANCIS M. WATTERS, Ph.D. California (Berkeley) 1966. Land tenure and financial burdens of the Russian peasant, 1861-1925.

- THOMAS E. WEISSKOPF, Ph.D. Mass. Inst. Technology 1967. A programming model for important substitution in India.

- GUNTER J. WITTICH, Ph.D. California (Berkeley) 1966. The German road to full employment.

- KUNIO YOSHIHARA, Ph.D. California (Berkeley) 1966. An econometric study of Japanese economic development.

CLARENCE ZUVEKAS, Ph.D. Washington (St. Louis) 1967. Argentine economic policies under The Frondizi government, 1958-1962.

Statistical Methods; Econometrics; Social Accounting

A. EUGENE ABRASSART, Ph.D. Illinois (Urbana) 1967. Dimensional analysis in economics.

PAUL ALDUNATE, Ph.D. Purdue 1967. Labor and capital sectoral allocation in the Chilean economy: a linear programming macroeconomic model.

An intersectoral macro-economic linear programming model was used to analyze the Chilean economy. The objectives were: (1) a static analysis when additional labor or capital resources were freely allocated to all the sectors of the economy and when these resources were allocated to the agricultural or manufacturing sectors; (2) A dynamic analysis when labor and capital resources were freely allocated to all the sectors of the economy and when additional labor or capital resources were allocated to the agricultural sector. The static analysis indicated that when additional workers were freely allocated among the sectors, the manufacturing and other sectors were most profitable. When additional capital was freely allocated among the sectors of the economy, manufacturing and mining were most profitable. The dynamic analysis indicated that when labor and capital resources were freely allocated, gross domestic product grew at increasing rates. These growth rates were always higher than the rate of increase in population.

THOMAS E. BORCHERDING, Ph.D. Duke 1966. The growth of non-federal employment in the U. S., 1900-1963.

ALAIN CEOPPIN DE JANVRY, Ph.D. California (Berkeley) 1966. Measurement of demand parameters under separability.

A number of ways of testing the hypothesis of a separable utility function and of determining a suitable partition of the commodity space are presented. Methods for measuring demand parameters are then systematically developed under various types of separability. Particular use is made of the two-stage maximization hypothesis.

FRANCES D. FERGUSON, Ph.D. Boston College 1967. Survey and analysis of time series econometric studies of U.S. manufacturing investment in plant and equipment.

This study examines the structural stability and predictive ability of the investment equations of deLeeuw, Kareken and Solow, Goldfeld, Bourneuf, and Liu. Although covariance tests suggest that all the equations, except Liu's, are stable over time, the "t" tests indicate that various coefficients within the equations are not. The predicted series lagged or moved simultaneously with the actual series; the differences between the predicted and actual series were seldom more than ten per cent of the actual series and often less.

R. JEFFERY GREEN, Ph.D. Illinois (Urbana) 1967. A long-run econometric model for Illinois.

JOSÉ S. GUTIERREZ, Ph.D. Iowa (Ames) 1966. Regression analysis of cross-section survey data for planning and evaluation of economic development programs.

FLOYD K. HARMSTON, Ph.D. Missouri 1966. Some simulation experiments with regard to a state-region of the United States.

An input-output model was used as a simulation of a state economy. A generalization of economic base theory was coupled with the theory of matrix multipliers to build a base for analysis. Several sectors were removed from the matrix and the impact was measured. Only removal of the household sector made any appreciable difference to the model. Two major new industries with approximately the same investment size were imposed on the model. The impacts were drastically different.

LAWRENCE HARRIS, Ph.D. New York 1966. Some interdependent decision problems.

THOMAS V. HEDGES, D.B.A. Indiana 1967. Improving measures of primary metals inventory change for national income accounting.

J. LAWRENCE HEXTER, Ph.D. Wisconsin 1966. Investment in physical capital: an empirical study of five industries using corporate data.

WILLIAM E. HOEHN, Jr., Ph.D. Northwestern 1966. Simultaneous interpolation and parameter estimation.

This dissertation addresses the problem of maximum-likelihood estimation in models containing lagged endogenous variables when observed values of the dependent variable are obtained less frequently than observations on the independent variables. Two estimation strategies are considered. The first strategy is to obtain parameter estimates only, treating missing observation values essentially as nuisance parameters. The second strategy is to obtain simultaneously both parameter estimates and estimates of missing observations, by a straight-forward maximization of the joint density function. Several estimation procedures are developed and Monte Carlo estimation of some numerical examples is performed. On the basis of two distribution sampling experiments, the second strategy, that of estimating both parameters and missing observations, appears marginally better than the first.

KOTESWARA R. KADIYALA, Ph.D. Minnesota 1966. Regression analysis with autocorrelated disturbances.

ALFRED J. KANA, Ph.D. Columbia 1967. Analysis of statistical activities of trade associations.

GEORGE M. LADY, Ph.D. Johns Hopkins 1967. The structure of economic models.

The purpose of this paper is to examine the concept of causality, the conditional relation, as it applies to the variables of an economic model. Using the theory of directed graphs, applications of the conditional relation are shown to correspond to properties of linear systems and certain optimization problems. The point of view is applied to linear models of balanced accounts, qualitatively determinate linear systems and the Strotz budgeting problem.

LAWRENCE K. LYNCH, Ph.D. Kentucky 1967. A statistical model of intercounty commuting.

A theory of intercounty commuting is developed based on Goldner's "normal preference area" concept, and including such variables as populations, distances, and wage levels. A multivariate statistical test involving 2000 observed commuting flows to metropolitan counties (published by the Bureau of the Census) gives support to the theory.

F. WILLIAM McELROY, Ph.D. Georgetown 1967. Linear expansion path production functions.

ROBERT M. McINNIS, Ph.D. Pennsylvania 1966. Regional income differentials in Canada, 1911-1961.

EDWARD J. MITCHELL, Ph.D. Pennsylvania 1966. An econometric study of international and inter-industrial differences in labor productivity.

ROBERT T. PARRY, Ph.D. Pennsylvania 1967. The separation of normal and transitory components of income from cross-section data.

RICHARD L. PETERSON, Ph.D. Michigan 1966. Solving for discrete changes in dynamic development planning models.

DAVID H. RICHARDSON, Ph.D. Purdue 1966. An investigation of the sampling distribution of estimators and test statistics associated with a simultaneous equations model.

ROGER F. RIEFLER, Ph.D. Washington 1966. Interregional input-output: Washington and California.

For some decades, perhaps beginning with Ohlin in the early 1930's, economists have recognized the feedback relations between two economic regions. In international trade analysis, this is the familiar foreign trade multiplier concept. While it has been recognized that two states can be tied in an interregional network, empirically no one has implemented such a model for two states. This thesis constructs an interregional input-output for the states of California and Washington. Both states have input-output studies and the major task was to link them together empirically. This involved developing a two-region input-output model with 53 sectors.

The thesis has developed some careful measuring techniques to generate interregional

flow data. In addition, it has tested the stability of certain trading coefficients and found them, in general, stable.

WILLIAM E. ROBERTS, Ph.D. Colorado 1966. Projections of employment in a metropolitan area: an examination of methods of reconciling available data, with particular reference to the Denver Metropolitan Area.

JOHN A. SAWYER, Ph.D. Chicago 1966. Forecasting industry output and imports in an open economy: some experiments for Canada, 1950-1958.

A comparison of the forecasting ability of three alternative input-output models and an econometric model for each of the seventeen major groups of Canadian manufacturing industries. The econometric model includes demand and supply functions for domestic industry and a demand for competitive imports function.

PERRY SHAPIRO, Ph.D. California (Berkeley) 1967. Aggregate time series production functions and technical change: an econometric study.

JAY E. STRUM, Ph.D. New York 1966. Non-classical formulae for powers of a matrix and for solution to difference equations, with applications in economics and business.

MILAD A. M. TAWADROUS, Ph.D. Iowa 1966. An econometric analysis of the consumption sector of the State of Iowa.

HODSON THORNER, Ph.D. Chicago 1966. Applications of decision theory to econometrics.

After a review of decision theory, and of its relation to sampling theory and Bayes theorem, a number of popular econometric devices are shown to be optimal solutions to decision theory problems. Decision theory is then used to derive new solutions to several estimation and testing problems in the context of the multiple regression model, the first order auto-regressive model, and a distributed lag model.

JACK E. TRIPLETT, Jr., Ph.D. California (Berkeley) 1966. Measurement of quality change.

CEE TSAO, Ph.D. Wisconsin 1966. The dynamics of industrial performance: a recursive programming study of the U.S. iron and steel industry.

JOSE VENTURA, Ph.D. Columbia 1967. The use of quantitative systems for macroeconomic analysis.

TERRY M. WALKER, Ph.D. Alabama 1966. A study of the small sample properties of selected simultaneous equation estimators utilizing distribution sampling techniques.

Economic Systems; Planning and Reform; Cooperation

JAMES G. ABERT, Ph.D. Duke 1966. The coordination of economic policy in the Netherlands.

ALAN A. BROWN, Ph.D. Harvard 1967. The economics of centrally planned foreign trade: the Hungarian experience.

This study is a theoretical, quantitative, and institutional analysis of Hungarian foreign trade (1949-1960). Its basic purpose is to further our understanding of centrally planned foreign trade, in general. The analysis is polarized around two central themes: the role of foreign trade in a traditional centrally planned economy, and the foreign trade reform movement. The general conclusions are that (a) the economic problems tend to intensify over time in a small centrally planned economy because of foreign trade; and (b) the foreign trade reforms, if they are to be effective, need to be well coordinated. These conclusions, in turn, may be derived from a single proposition, i.e., the dynamic effects of centrally planned foreign trade are functionally interrelated. The study also contains a theoretical model of centrally planned foreign trade, which is used to systemize and generalize the relationships among the variables.

DAVID W. CONKLIN, Ph.D. Mass. Inst. Technology 1967. Essays on Soviet agriculture and decentralization reforms.

PAUL C. ROBERTS III, Ph.D. Virginia 1967. An administrative analysis of Oskar Lange's theory of socialist planning.

The natures of hierarchical and polycentric organization are examined and found fundamentally different. Lange's theory is examined in the context of the Socialist Debate and

found not to reflect the intentions and aspirations of what was meant by central planning, but to be rooted in polycentric organization. The famous refutation of Mises' argument about the impossibility of a planned economy is found to be illusory. Five factors are given that contributed to perpetuating the illusion.

FAUAD E. M. EL TAHER, Ph.D. Pennsylvania 1967. A management perspective on the U.A.R.'s First Five Years of national economic planning.

The specific objectives of this study are: (1) to provide an analytic description of the U.A.R.'s Central Planning experience within the full historical context of the country's cultural, physical, institutional and economic development; (2) to uncover and analyze the areas of relative success and failure in the first five years of the centrally planned economy; (3) to identify the adaptations required of business institutions by the national planning structure and process; (4) to offer insights for alternative adaption patterns for business institutions within the National Planning structure and process.

SALWA A. S. FARGHALI, Ph.D. Southern California 1966. Planning under socialism—and risk.

Data limitations and the clarity of priorities in early stages of development limit the possible uses of the great advantages of the present stochastic analysis; and under conditions of central planning, the "imperfections in the conditions of plan execution" represent the existence of new random elements, inherent in the system, which reflect a situation or uncertainty. Since these elements are inseparable from technical aspects of the allocation policy, the actual situation under which anticipations in a centrally planned system are made is one of uncertainty rather than risk. This, in turn, justifies the acceptance of planners' choice and value-judgement.

WILLIAM L. HOLMES, Ph.D. Illinois (Urbana) 1966. Control aspects of economic planning under information restrictions.

PAUL JONAS, Ph.D. Columbia 1966. A critique of Sovietology: the theory of a centrally planned economic system.

The study shows that the general analytical diagrammatic framework, with modifications, can be applied to centrally planned economies. For these systems, models are developed to represent and analyze (1) price structure; (2) determination of product-mix; (3) distribution of product-mix; (4) trade equilibria; (5) estimation of national income. The study also argues that in the family of special purpose theories a sub-family should be developed to deal with the pure theory of the Soviet type systems.

LEONARD KIRSCH, Ph.D. Harvard 1967. A study of Soviet economics: wage administration and structure in the USSR since 1956.

In broad outline this investigation is divided into seven parts. The chapter following the introduction is devoted to the determination of basic wage rates. Chapters III through V concentrate on incentive wages: the scope and nature of the various incentive systems; the determination of output norms for piece-rate workers; and the effect of incentive wages upon differentiation of earnings. In Chapter VI, Soviet wage theory and proposed reforms are discussed. The final chapter is devoted to an appraisal of the Soviet approach to intra-industrial wage determination.

JOYCE S. PICKERSGILL, Ph.D. Washington 1966. Soviet monetary policy 1914-1937.

The essential contribution of this study is the application of western macro-economic theory to the investigation of monetary policy in the Soviet economy. Traditional demand theory is applied to an analysis of changes in the holdings of cash and non-cash assets of consumers and firms. Several economic variables were chosen to test the stability of the demand function for money in the context of Soviet institutions. By conventional statistical criteria, the demand function was remarkably stable over the long period despite major institutional changes that occurred over the period.

RALPH G. SAYLOR, Ph.D. Duke 1966. Economic system of Sierra Leone with special reference to the role of the government.

AKRAM SHAKRA, Ph.D. Oklahoma 1966. Land reform in Syria.

ARMAND M. J. VAN NIMMEN, Ph.D. Columbia 1967. French planning: an essay in evaluation.

The dissertation is an attempt to (1) evaluate French planning as a form of economic

forecasting and (2) verify whether there exist any statistically verifiable indications in support of the hypothesis that planning has had a discernible impact on the pattern of French economic growth. The analysis is carried out in terms of a series of statistical tests involving correlation coefficients, inequality coefficients and input-output experiments.

MARTIN L. WEITZMAN, Ph.D. Mass. Inst. Technology 1967. Toward a theory of iterative economic planning.

Business Fluctuations

ROSANNE COLE, Ph.D. Columbia 1967. Errors in estimates of Gross National Product and their relation to forecasting accuracy.

Study appraises the accuracy, as indicated by the successful revisions, of the preliminary estimates of GNP and its components and analyzes the effects of errors in these data on the accuracy of short-term forecasts. Estimates are made of the extent to which the accuracy of different types of forecasts was impaired as a consequence of using preliminary rather than final (1965) data.

MOHEB A. GHALI, Ph.D. Washington 1967. Rational models of finished goods inventories.

A review of the literature on inventory holdings reveals a gap between theory and data. This dissertation helps to bridge this gap by combining a theory of inventory holdings predicated on profit maximization and sound statistical analysis of data on finished goods inventories. An empirically verifiable model of inventory holdings in response to predictable seasonal fluctuations in sales is developed.

FAITH HALFTER, Ph.D. Harvard 1967. The cyclical behavior of materials' prices in United States industry, 1947 to 1965.

This is an empirical study of the cyclical behavior of prices of the major crude industrial raw materials in the American economy in the postwar period. Although price equations are estimated for several individual materials, the chief interest is in explaining macro behavior. The hypothesis is neoclassical, namely, changes in domestic materials' prices can be explained by demand pressures in the product market both at home and abroad. The results largely support this hypothesis.

DAVID A. PATTERSON, D.B.A. Indiana 1966. An intensive analysis of the Indiana economy: 1947-1963

WILLIAM C. PERKINS, D.B.A. Indiana 1966. A simulation analysis to evaluate the impact of the automatic stabilizers.

DENNIS R. STARLEAF, Ph.D. Vanderbilt 1967. The upper turning point of 1960.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

PETER W. BACON, D.B.A. Indiana 1967. An analysis of postwar bank mergers in Marion County, Indiana.

ALLAN E. BERGLAND, Ph.D. Arizona 1967. The response to monetary policy in the Eleventh Federal Reserve District 1954-1963.

This study determined, for the Eleventh Federal Reserve District, the length of the intermediate lag in monetary policy. The intermediate lag was defined as the time lapse occurring between a "dynamic" change in open market operations and the response of selected member bank variables. The study concluded that free reserves, gross reserves, and loans showed relatively little response to monetary policy, while regional demand deposits responded after an approximate lag of eight months.

PETER BERMAN, Ph.D. Johns Hopkins 1967. A theoretical and empirical examination of large bank use of the discount facility in the United States.

A study of the use of the discount facility by 143 large member banks active in the Federal Funds market using weekly data supplied by the Board of Governors suggests that: (1) large banks borrow continuously for long periods from the Fed during tight money thereby denying the "tradition of staying out of the Fed"; (2) significant inter-

District differentials in discount facility administration exist; (3) the facility should be modified and retained solely as a member bank convenience.

GEORGE BETZ, Ph.D. Wisconsin 1967. Member bank borrowing from the Reserve Bank: a micro-analytic analysis.

JOHN O. BORNEOFEN, Ph.D. Illinois (Urbana) 1967. The effect of banklines of credit on corporate holdings of secondary reserves.

ERIC BRUCKER, Ph.D. Duke 1966. Structural changes in North Carolina banking: 1950-63.

JOHN E. BUEHLER, Ph.D. State New York (Buffalo) 1967. An evaluation of some aspects of Federal Reserve policy.

JOSEPH M. BURNS, Ph.D. Chicago 1967. The saving-investment process in a theory of finance.

The saving-investment process is shown to be affected by various finance costs. In particular, the costs of effecting transactions and of bearing uncertainty are shown to be factors affecting saving and investment demand. The nature of these costs, together with the ways in which savers and investors seek to minimize them, are examined. Financial innovations are shown to reduce the finance costs. Evidence on financial developments in the United States is presented.

JOHN F. CHANT, Ph.D. Duke 1966. The effectiveness of monetary policy in Great Britain and the London Clearing Banks: 1951-58.

JAMES C. COOPER, Ph.D. Illinois (Urbana) 1967. The demand for bank outputs and the bank-customer relationship.

JAMES D. DANA III, Ph.D. Mass. Inst. Technology 1967. Some aspects concerning the theory of consumer durables.

BERNARD DAVIS, Ph.D. Kentucky 1967. The development of Kentucky financial institutions 1946-1964.

The study evaluates selected types of Kentucky financial institutions in terms of the national experience of the same type of institution. Also, it examines the changes in the institutional mix within Kentucky and makes a projection for the future. The study employs historical information, both legal and statistical. Firms included are commercial banks, savings and loan associations, credit unions, life insurance companies, consumer loan companies, and industrial loan companies.

NANCY S. DORFMAN, Ph.D. California (Berkeley) 1967. The role of money in the investment boom of the twenties and the 1929 turning point.

ARTHUR R. DORSCH, Ph.D. Florida 1966. Consumer installment sales credit in the United States economy and its relationship to monetary policy, 1946-1963.

PETER A. FROST, Ph.D. California (Los Angeles) 1966. Banks' demand for excess reserves.

An inventory model is developed for an individual bank as a basis for deriving a demand function for excess reserves. The major implication of the model is that the demand function is sharply kinked, being inelastic above some critical short-term interest rate (approximately two-thirds of one per cent) and elastic below this rate. The theory is used simultaneously to explain the behavior of excess reserves and the short-term interest rates during the 1930's.

B. V. GESTRIN, Ph.D. Toronto 1966. The structure of interest rates in Canada.

JAGDISH HANDA, Ph.D. Johns Hopkins 1967. A new approach to the theory of portfolio selection.

The thesis generalizes the concepts of motives inherited from Keynes and of characteristics from existing portfolio selection theories. It argues that assets possess quantifiable characteristics which satisfy quantifiable uses or motive. Any number of assets, characteristics and motives can thus be easily treated. Two characteristics are expected returns and variance (for risk). The individual has a preference ordering over motives and maximizes utility subject to a wealth constraint. The concept of the Efficiency Substitution Effect is then used to explore problems in portfolio selection.

THOMAS M. HAVRILESKY, Ph.D. Illinois (Urbana) 1966. An investigation of monetary policy action, 1952-1965.

ALBERT G. HEEBNER, Ph.D. Pennsylvania 1967. Negotiable certificates of deposits: the development of a money market instrument.

This dissertation examines the development of negotiable certificates of deposit (CDs). The primary and secondary markets are investigated, as well as the structure and behavior of interest rates. It is suggested that influences on CD outstandings include bank reserve positions, anticipated loan demand, the volume of short-term investment funds, corporate tax and dividend payments, and interest rate differentials, and these hypotheses are largely supported by multiple regressions. Also, the implications of CDs for monetary policy are considered.

C. ELTON HINSHAW, Ph.D. Vanderbilt 1967. The recognition pattern of the federal open market committee: a study of the inside lag of monetary policy, 1947-1960.

BERISLAV KARČIĆ, Ph.D. Columbia 1966. The role of money, credit, and related institutions in postwar Yugoslavia till 1960.

A discussion of the Yugoslav economic system precedes the detailed analysis of the financial institutions. Flow-of-funds analysis is employed; monetary and real flows are carefully scrutinized. It is suggested that in spite of a very favorable overall economic record, excessive monetary and credit expansion did occur. The view is advanced that the shortcomings of overall planning, rather than those of the financial institutions proper, were responsible. The appendix contains extensive statistical information.

JOONG-KOON LEE, Ph.D. Columbia 1967. Behavior of money and prices in the Korean inflation, 1953-1964.

An attempt to explain the time distribution of the rate of price change in Korea between 1953 and 1964. Done by first investigating the factors determining the demand for real money assets à la Gurley-Patrick-Shaw Mission study; then, by investigating the price changes, using three alternative models. Agricultural component of GNP seems to have most significantly influenced the demand for money and, in turn, the time rate of price changes.

PETER LINDERT, Ph.D. Cornell 1967. Key currencies and gold exchange standard.

DAVID L. MCKEE, Ph.D. Notre Dame 1966. An economic analysis of credit unions in Michigan.

ANTHONY MACH, Ph.D. Boston College 1967. The institutional, statistical, and analytical importance of Federal Reserve check collections.

This thesis analyzes and compares bank debits and Federal Reserve check collections as alternative series for the study of expenditure flows. A monthly series on check collections for the period 1946-1964 was developed from the operational data of the Reserve Banks. Check collections were more closely associated with GNP and industrial output than were bank debits. Check collections data may be used in velocity and regional economic studies.

DONALD MARKET, Ph.D. Louisiana 1967. The theory of 100 per cent reserve banking: historical development and critical analysis.

This study traces historically the development of the 100 per cent reserve banking plans up to and including the proposals of Milton Friedman. Included in the study are the plans developed by John R. Cummings, Frederick Soddy, Irving Fisher, and Henry Simons. The theoretical and policy issues are examined in their historical contexts and re-examined in the context of contemporary monetary and banking theory.

MELVYN MEER, Ph.D. Minnesota 1966. Securities, intermediaries, and consumer theory.

A financial model postulating community demands for assets, one of which is a financial intermediary liability, is presented. Theorem I establishes necessary conditions for the intermediary to affect monetary policy in a specified manner. Theorem 2 assumes the integrability of the demand functions and demonstrates that with specified security forms, restrictions on the operations of financial intermediaries affect monetary policy in a way which runs counter to some of the prominent intermediary literature.

L. CHARLES MILLER, JR., Ph.D. Yale 1967. The distribution of member bank reserves among the twelve Federal Reserve Districts.

CHARLES T. NISBET, Ph.D. Oregon 1967. The informal credit market in rural Chile: its nature, significance and relationship to the institutional credit market.

MORRIS PERLMAN, Ph.D. Chicago 1966. International differences in liquid asset portfolios.

An international cross-section study of the holdings by the private nonbanking sector of the economy of currency and various types of deposits. Forty seven countries are included in the sample. The main explanatory variables used in the study are income, urbanization, the rate of interest and the expected rate of inflation. The currency-deposits ratio and its determinants are also examined.

SAMUEL H. PHILLIPS, JR., Ph.D. Virginia 1966. The Radcliffe Report: its implications for United States monetary policy.

A critical review and summary of the *Radcliffe Report* is presented. The purpose is to defend the uniqueness of the supply of money in monetary policy and to show that control of the cash base of the banking system will provide control of general liquidity. It shows that the *Radcliffe Report* does not give support to the Gurley-Shaw thesis since the problem of liquidity in the United Kingdom is not one created by non-bank financial intermediaries.

JOHN A. POWERS, Ph.D. Purdue 1966. The existence of economies of structure and of economies of scale in commercial banking.

JON J. PRAGER, D.B.A. Indiana 1966. An analysis of bank chartering policies in Indiana.

ROBERT H. RASCHE, Ph.D. Michigan 1966. Bond prices, interest rate structure, and debt management policy.

MIGUEL A. RODRIGUEZ, Ph.D. California (Berkeley) 1966. Individual supply of funds and demand for money.

PAUL J. L. SCHATZ, Ph.D. Alabama 1967. The development of credit unions in Alabama.

EDWARD B. SELBY, JR., Ph.D. Louisiana 1967. Response of the Federal Open Market Committee to economic changes 1951-1966.

The publication of the minutes of the Federal Open Market Committee (FOMC), provides information heretofore unavailable to "outside" economists. This study, utilizing the minutes, considers the following topic areas: FOMC meeting procedure; goals, indicators, and targets of the FOMC; and the FOMC's response to economic changes, 1951-1960. Conclusions—the FOMC has been flexible in its use and refinement of targets and it has usually responded to economic changes within three months after their occurrence.

MIGUEL SIDRAUSKI, Ph.D. Chicago 1967. Rational choice and patterns of growth in a monetary economy.

An attempt to introduce monetary factors in the theory of economic growth. It analyzes the effects of different rates of inflation on short-run capital accumulation and on the long-run capital intensity of an economy in which money can be held in the portfolio of wealth owners as an alternative asset to real capital. It concludes that while under certain assumptions the long-run capital intensity is independent of the rate in inflation, an increase in the rate of monetary expansion lowers capital accumulation in the short run.

WILLIAM L. SILBER, Ph.D. Princeton 1966. Monetary policy, debt management and capital allocation: the implications of behavior in the financial sector and the substitutability between securities.

A disaggregated quarterly econometric model of the United States with major emphasis on the financial sector is specified and estimated, with the initial objective of examining the substitute-complement relationships between securities in financial-intermediary portfolios. After estimating the demand equations for securities (disaggregated according to each intermediary), an entire model is estimated from which the reduced-form equations are calculated. The impact multipliers provide insight into the relationship between monetary (and debt) policy and substitutability between securities.

ANDREW STERN, Ph.D. Columbia 1967. The neutralization of foreign assets; a study of eleven countries: 1955 to 1964.

Quarterly data for the countries in the "Group of Ten" and Switzerland support the

hypothesis that central bank domestic assets are a negative function of foreign assets and a positive function of industrial output. A stock adjustment model is specified, and the time shape of central bank adjustment to changes in the independent variables is estimated. In addition, the short- and long-run effects of neutralization on high-powered money and on the money stock are measured.

EDWARD M. SYRING, Ph.D. Oregon 1966. The demand for money in Norway: an econometric analysis.

RYLAND A. TAYLOR, JR., Ph.D. Notre Dame 1966. The use of a sponsor's computer in a credit union operation.

ALAN TECK, Ph.D. Columbia 1967. Mutual savings banks and savings and loan associations: functional convergence and divergent growth.

RICHARD E. TOWEY, Ph.D. California (Berkeley) 1967. Commercial bank time deposits and some of their implications for Federal Reserve policy.

ROBERT C. VOGEL, Ph.D. Stanford 1967. The demand for cash and liquid assets by manufacturing corporations: estimates and implications.

This dissertation investigates the cash and government security holdings of manufacturing corporations during the postwar period, using both cross-section and time-series regressions. The principal findings are: (1) a transactions model appears more adequate than a wealth model; (2) interest rates have a significant influence, and there is evidence of substitution between liquid assets and trade credit and inventories; (3) adjustment lags are shorter, but more complex, than those found in most other studies. Some implications of these results for monetary economics are also discussed.

CHARLES W. WALTON, Ph.D. Georgetown 1967. The determinants of member bank free reserves—some demand considerations.

The Meigsian hypotheses concerning the determinants of member-bank free reserves are retested for the period 1960-1966. Interest rate spreads continue to be a more important determinant of free reserves than open market operations. However, the influence of the discount rate is inconsistent with the earlier period and the profit motive. Substituting the federal funds rate for the Treasury bill rate improves the explanatory power of the equations. Attempts to measure the influence of loan demand were inconclusive.

DONALD C. WELLINGTON, Ph.D. Chicago 1966. Inflation during the French Revolution.

The study is concerned with the paper currencies, the assignats and the mandats, that were issued during the French Revolution, 1789-1797. It attempts to measure the depreciation of the assignats and mandats, and explain the depreciation in terms of monetary theory.

MARK H. WILLES, Ph.D. Columbia 1967. The inside lags of monetary policy.

OLIVER G. WOOD, JR., Ph.D. Florida 1966. The Federal Reserve System's "Operation Nudge."

LOUIS H. ZINCONI, JR., Ph.D. Virginia 1967. The real-balance effect: aspects and evidence.

The purpose of this dissertation is to discuss different aspects of the real-balance effect (RBE) to show how it can be used to resolve the paradox of the predominant macro-wealth effect and the micro-income effect. It finds three aspects of the RBE (the net wealth aspect, the portfolio balance aspect, and the Cambridge aspect), each of which affects consumption directly or by increasing investment, incomes, and, finally, consumption. In the light of these aspects, the dissertation analyzes several empirical studies and finds that (1) due to the indirect operation of the RBE, some influence of wealth on consumption is shown through the income variable and (2) that demand-for-money studies also reflect the presence of the RBE.

Public Finance; Fiscal Policy

HENRY F. ANDERSSON, Ph.D. Southern California 1966. Effects of Swedish fiscal policy and planning on economic growth and stability, 1930-1964.

The success of economic policies can be attributed to the following factors: (1) Invest-

ment was the strategic factor in attaining growth. (2) Public investment supplemented private investment during economic downturns. (3) Private investment was encouraged by allocation and release of investment reserve funds, accelerated depreciation rates, and a tax policy which induced plant and equipment expansion. (4) Planning became the principal instrument for integrating fiscal and monetary policies with anticipated private investment. (5) Partnership between government and private business was realized by encouraging private enterprise to participate in the planning and determination of the course of action for realization of the economic objectives. (6) Responsible labor unions co-operated with government and business to attain economic objectives without interruptions.

DAVID F. BRADFORD, Ph.D. Stanford 1967. Public finance: aspects of public goods.

To elucidate the notion of "draft uncertainty" a model of the individual's choice of enlistment age (the main variable under his control) is developed, using a simple non-stationary Markov process representation of the situation. A variety of implications of an expected-utility maximization assumption are derived. Uncertainty costs are seen to arise from individuals' preferences about the age of entering military service. A final chapter sketches out the nature of production costs due to draft uncertainty.

MARVIN BRAMS, Ph.D. Clark 1967. State assessments and shared taxes in Massachusetts, with special attention to Boston.

EDWARD W. BRENNAN, Ph.D. Pennsylvania 1966. The personal property tax on intangibles in Pennsylvania.

In Pennsylvania, intangibles are subject to a county property tax of four mills. The thesis had three objectives: (1) a study of the evolution of the tax, and related concepts; (2) a survey of present administration of the tax by the counties; (3) an analysis of the tax's effect on relative yields of taxable and exempt investments. The major part, the administrative study, disclosed more vigorous enforcement than is popularly supposed, at modest cost. This, plus exchange of data with the Internal Revenue Service, leads to the expectation of more vigorous enforcement in the future.

THOMAS W. CALMUS, Ph.D. California (Berkeley) 1966. Depreciation for tax purposes.

DONALD C. CELL, Ph.D. Columbia 1966. Federal-local fiscal relations through 1935: an economic interpretation of Federal aid decisions.

This study develops hypotheses toward explaining why the states have been by-passed in some Federal aid programs while permitted to participate in others. Downs' vote maximizing decision rule is applied to a multi-part system of interdependent governments in which administrative channels—Federal, state, and local—are responsive to different sets of political demands. The study analyzes some pre-Depression attempts by rural governments to by-pass the states and the city-state struggles of the early 1930's.

DEAN T. CHEN, Ph.D. Minnesota 1966. An analysis of inter-community expenditure differentials in the provision of public services to rural communities in Southwestern Minnesota.

PAUL L. CHEN-YOUNG, Ph.D. Pittsburgh 1966. An economic evaluation of the tax incentive program in Jamaica.

JOHN P. COOKE, Ph.D. Colorado 1967. Determinants of the variation in local public expenditures in Colorado.

JOHN R. COOPER, Ph.D. Virginia 1967. Institutional factors affecting the outcome of school bond referenda.

This paper is an attempt to determine whether or not alternative voting rules have any influence on the outcome of school bond elections. The voting requirements in the various states were grouped according to the size of majority required for passage and the existence of a property qualification for voting in the election. The initial hypothesis was that as the voting requirement became more restrictive and the majority necessary for approval increased, fewer bond proposals would be passed. The total number and total value of bond proposals voted upon were considered in relation to the voting rule. There is no indication that the property qualification alone decreases the chance for approval of a bond proposal. The evidence does indicate that as the size of majority necessary for approval increases, there is a greater chance for the defeat of a school bond proposal.

STUART R. CRANE, D.B.A. Indiana 1967. Federal financing for toll projects incorporated in the interstate highway system.

JAMES CUTT, Ph.D. Toronto 1966. Taxation and economic development in India.

ANTAL DEUTSCH, Ph.D. McGill 1967. Income redistribution through Canadian federal family allowances and old age benefits.

This thesis measures the immediate net effects on the distribution of disposable income of the operations of the Canadian government's family allowance and old age benefit programs. The basic information used originates largely with the Dominion Bureau of Statistics. Results are presented by income group, family type and geographic region. The old age benefit program acts as a more progressive income redistributor than family allowances. Family allowances redistribute income from unattached individuals to farm and non-farm families. Unattached individuals and institutional residents gain from non-farm families through old age benefits. Residents of Ontario and British Columbia lose to those of Quebec and the Atlantic Provinces through family allowances. Old age benefits favor residents of the Prairies, the Atlantic Provinces and British Columbia at the expense of Quebec residents.

VIJAYA G. DUGGAL, Ph.D. Harvard 1967. Fiscal policy and economic stabilization: simulations with the Brookings Quarterly Econometric Model of the United States.

This thesis studies the effectiveness of expansionary fiscal policies. The results are examined in the context of the structure of the model that generates them. In coping with recessions, the expenditure increase policy is found to be most effective, and the increased balanced budget the next best alternative. The tax reduction policy, being less efficient, is feasible only in raising stagnantly low activity levels in the long run. Automatic stabilization schemes to reduce fluctuations are experimented with and found successful.

JOSEPH R. FAHEY, Ph.D. Mass. Inst. Technology 1967. The advisability and probable economic effects of a local income or local sales tax in Boston.

ROBERT S. FELL, Ph.D. Brown 1967. Tax incentives and investment growth.

An analysis of empirical studies determines areas in which tax incentives could prove effective. The type of tax incentive used depends upon its purpose, economic conditions, future expectations, the prevailing tax system and characteristics of the government. The author recommends federal tax incentive aids for area development, allowance of direct costing and lower of LIFO or market inventory valuation, congressional elimination of reserve-ratio tests and permanent reinstatement of the investment credit and accelerated depreciation.

RICHARD F. FRYMAN, Ph.D. Illinois (Urbana) 1967. The extent and effects of sales taxation of producers' goods.

DAVID F. GATES, Ph.D. Princeton 1967. The effect of population growth on the fiscal strength of residential suburbs.

This study is an empirical analysis of the effects of variations in growth rate upon the public needs, public resources, and the ratio of public needs to public resources in 56 New Jersey residential suburbs.

BRUCE L. GENSEMER, Ph.D. Michigan 1966. Determinants of the fiscal policy decision of local government in urban areas: public safety and public education.

RONALD B. GOLD, Ph.D. Princeton 1966. Subsidies to industry and alternative policies to reduce regional unemployment.

This study consists of two parts. In the first, benefits and costs of alternative policies to reduce regional unemployment are analyzed from the viewpoint of an individual community and from a national viewpoint. In the second part, the efficacy of one policy, subsidies to attract new industry, is investigated through a case study of subsidies given by communities in Pennsylvania. It is demonstrated that the subsidizing communities and the state experienced significant employment gains.

WILLIAM J. GRASTY, Ph.D. Florida 1967. An evaluation of the value added tax in the fiscal structure of a state: Florida, a case study.

WILLIAM HABACIVCH, Ph.D. Illinois (Urbana) 1967. Congressional preferences for expenditure proposals: some theoretical and empirical considerations.

CHARLES A. HEDGES, Ph.D. California (Berkeley) 1966. A peak-load pricing scheme for congested urban freeways.

WILLIAM R. HENDLEY, Ph.D. Duke 1966. Swedish fiscal measures to stabilize investment and analysis of the investment-reserve system and the temporary investment tax.

WALTER J. HERMAN, Ph.D. Florida 1965. Federal debt management; an analysis of its objectives and application from 1953 through 1962.

DAVID H. KOPF, Ph.D. Princeton 1967. Government debt, intergeneration welfare and economic activity.

This study examines the proposition that government debt can have an effect on the distribution of economic welfare among generations. Individual utility from amount and distribution of lifetime consumption can be altered by government debt operations, and the impact of such operations on consumption and capital stock are examined. The effect of inherited wealth and international trade are considered, and a general model incorporating money and government debt is developed.

JOHN B. LEGLER, Ph.D. Purdue 1967. Regional trends in federal receipts and expenditures, 1815-1900.

HENRY M. LEVIN, Ph.D. Rutgers 1967. Estimating the municipal demand for public recreational land.

Land is considered as an input into the production function for municipal recreation. On the basis of a model of revealed preference in the public sector, the parameters of derived demand for local recreational land are specified and estimated for a "public-goods" region. The cross-sectional estimates yield the expected results. Finally, some insights into the allocation of recreational land are deduced from the findings.

PETER B. LUND, Ph.D. California (Berkeley) 1967. Municipal costs arising from business and industry: a case study of San Leandro, California.

WILMA S. MAYERS, Ph.D. California (Berkeley) 1966. The real property transfer tax.

WILLIAM E. MITCHELL, Ph.D. Duke 1967. State and local government legal debt limitations.

JOHN M. C. OLSON, JR., Ph.D. Southern California 1966. An analysis of fiscal policy during the Truman Administration (1945-1953).

The art of practicing fiscal policy does not appear to have been sufficiently developed at the time of the Truman Administration to cope with the problem of inflation. Easy money and credit, tax reduction, liquid assets and inadequate supply combined in such a manner that the purchasing power of the dollar declined 33 per cent during the period. The Administration failed to achieve voluntary or restrictive action to curb the inflationary pressures.

LARRY L. ORR, Ph.D. Mass. Inst. Technology 1967. Municipal governmental policy and the location of population and industry in a metropolitan area: an econometric study.

JACK W. OSMAN, Ph.D. Rutgers 1966. The determinants of interstate variations in state and local government expenditures in the United States.

Multivariate regression analysis was used to find the determinants of state-local government outlay by function. Federal aid was found to have a dual impact on expenditures, "stimulating" those functions to which it was directed, while federal aid to *all other* functions was also found to have increased outlay for particular functions. Other variables included were income, urbanization, state's share of tax revenue, the rate of population growth and variables "specific" to the function under investigation.

ANNETTE PINCKNEY, Ph.D. Clark 1967. State and local government industrial aid bond finance: a critique.

JESSE E. RAINE, Ph.D. Kentucky 1967. The tax status of religious property in the United States, with special emphasis on Nashville and Tennessee.

This is a study of the gradual narrowing, over time, of the scope of the tax exemption granted to real property owned by religious institutions in the United States. It considers in depth the statutes and case decisions marking this narrowing in Tennessee, and

a random sample was taken to estimate the total value of such exempt property in Nashville.

DAVID D. RAMSEY, Ph.D. Minnesota 1967. Optimal financing of local government capital projects.

ROY W. RICHARDS, D.B.A. Indiana 1967. The federal income tax: 1872 to 1913.

JEAN Y. RIVARD, Ph.D. Michigan 1967. Determinants of city expenditures in Canada.

JEFFREY M. SCHAEFER, Ph.D. Columbia 1966. Charitable transfers at death under the Federal Estate Tax.

Data cited in this study indicate that upper income and wealth groups give proportionately larger amounts of their contributions for educational and social welfare purposes than lower income and wealth groups do. Therefore, it may not be entirely unsound for the public sector to share a greater cost of contributions by wealthier individuals, since such contributions are fairly good substitutes for government expenditures on civilian goods and services.

PHILIP A. SHADE, D.B.A. Indiana 1967. Municipal capital budgeting: a case study of Indianapolis, Indiana.

BERNARD SOBIN, Ph.D. Columbia 1967. Shadow prices for money in government expenditure policy.

The paper discusses the current practices of U.S. government agencies and the evolution of thought in professional literature with respect to interest rate criteria for government investment. It goes on to a more general analysis of the concept of shadow prices for government funds, which are marginal exchange rates between particular kinds of funds and money of other kinds or dates of availability; and it discusses uses of the concept in government expenditure policy.

BYRON G. SPENCER, Ph.D. Rice 1967. The shifting of the corporation income tax: the Canadian experience.

The dissertation is an econometric study of short-run shifting of the Canadian corporation income tax using the Krzyzaniak-Musgrave model. The author finds in the open Canadian economy there is less shifting of corporation income tax than in the U.S. economy.

VITO TANZI, Ph.D. Harvard 1967. The structure of the individual income tax in the major industrial countries: an international comparison.

The main objective of this study is an analysis of the structure of the individual income taxes in the six major industrial countries (France, Germany, Italy, Japan, United Kingdom, and United States). The study is concerned with the question of "sacrifice" connected with these taxes; with the relationship between individual income tax and the propensity to save of the household sector; and with the relationship of the individual income tax of each country to the growth of the economy.

GEORGE M. VON FURSTENBERG, Ph.D. Princeton 1967. The impact of tax and credit incentives and political risks on industrial growth in West Berlin over the period 1952 to 1964.

The pattern of industrial investment in West Berlin has been primarily determined by the changing impact of political threats and tax and credit preferences. Income statement simulations for Berlin based on West German input structures reveal the factor compositions and operating characteristics favored. An econometric model standardizing the conditions of sale other than location allows quantification of the political risk discount by interpreting the relation between selling price and annual yield of certain immobile assets.

HUBERT D. WHITE, Ph.D. Southern California 1967. A dynamic model for predicting the effects of fiscal policy measures.

Using a block recursive model, the following are among the conclusions supported by the study. First, the form of the model has limited usefulness as a tool for evaluating fiscal policy measures. Second, the most significant fiscal policy variable was the govern-

ment deficit. Finally, fiscal policy measures were not of sufficient intensity to appreciably diminish the recessions of the period.

JAMES WIGHTMAN, Ph.D. Clark 1967. The impact of state and local fiscal policies on redevelopment areas in the northeast.

JOHN O. WILSON, Ph.D. Michigan 1967. Capital budgeting and local government investment decision-making.

MUHAMMAD YAQUB, Ph.D. Princeton 1966. The tax structure of a developing country: a case study of Pakistan.

The study contains a quantitative analysis of the elasticity of taxes in Pakistan. Its results broadly support the hypothesis that, in an underdeveloped country, tax structure may be inelastic with respect to money national income. It, however, contradicts the generally held view that income taxes are more elastic than commodity taxes. It has four sections. A discussion of the statistical approach in Section I is followed by an analysis of the elasticity of income taxes in Section II and of commodity taxes in Section III. Section IV contains an exercise in projections.

International Economics

CHARLES H. ANDREWS, Ph.D. Vanderbilt 1967. The role of La Compañía de Acero del Pacífico in the changing structure of the Chilean economy.

JOHN C. BEYER, Ph.D. Fletcher School 1966. The economic implications of federation among undeveloped countries, with special reference to the less-developed unit of the Federation: a case study of the Federation of Malaysia.

JAGDISH N. BHAGWATI, Ph.D. Mass. Inst. Technology 1967. Essays in international economics.

ROBERT L. BIRMINGHAM, Ph.D. Pittsburgh 1967. Interterritorial imbalance in customs unions among developing economies: adjustment mechanisms.

WILLIAM H. BRANSON, Ph.D. Mass. Inst. Technology 1967. The capital account in the United States balance of payments.

HAROLD L. BRYANT, Ph.D. Cincinnati 1967. Problems of discipline in the international balance of payments.

JOHN F. BURKE, JR., Ph.D. Notre Dame 1966. Internal and external effects of an E.C.C. common currency area.

WILLIAM L. CASEY, JR., Ph.D. Boston College 1967. External payments problems of a debtor country: the case of Brazil, 1948-1963.

An investigation of the impact of external debt accumulation on Brazil's balance of payments, 1948 to 1963. Contractually-fixed debt service payments are measured in relation to fluctuations in elements of disturbance (e.g., declines in export earnings) in the formulation of liquidity crisis indicators. The role of Brazil's debt service burden during 1955-1963 in producing the chronic imbalance which necessitated the introduction and intensification of stringent measures of external adjustment.

ROBERT L. CURRY, JR., Ph.D. Oregon 1967. An economic analysis of the gains from trade effects of Webb-Pomerene Export Trade Associations.

HERMAN E. DALY, Ph.D. Vanderbilt 1967. Trade control and Uruguayan economy.

ROBERT M. DUNN, JR., Ph.D. Stanford 1957. Flexible exchange rates and the prices of traded goods: a study of Canadian markets.

Internal prices of traded goods may not fluctuate with an exchange rate; oligopolists with strong preferences for stable prices may maintain local currency prices in each market as the exchange rate varies within some range. Such pricing reverses the normal relationship between the exchange rate and the terms of trade, and has effects on the reactions of the trade account. Regressions and questionnaires are used to study seven markets, and the results support these hypotheses.

WILLIAM O. FREITHALER, Ph.D. Michigan 1966. The contribution of the export sector to Mexico's balance of payments and economic growth.

PAUL GRAESER, Ph.D. Columbia 1967. Money, growth and the balance of payments.

Open growth models, after the work of George Borts, were constructed to test the following proposition: permitting a long-term balance of payments disequilibrium will alter the response of the model economy to policy and other exogenous variables, from what it would be if payments equilibrium were maintained. Conclusion: the response to fiscal and exchange rate policy, and especially to monetary policy, is very much altered.

ABDESSATAR R. GRISSA, Ph.D. Brown 1967. The stability of flexible exchange rates: the case of the French and the British experience in the 1920's.

The dissertation is primarily an empirical study whose main object is to determine whether changes in the values of the franc and of the pound in the foreign exchange market were the result of destabilizing speculation against these currencies or due to shifts in their equilibrium levels. Another question this study is designed to investigate is the relation between fluctuations in the external value of the franc, the French money stock, and the demand for pegged government securities.

ROBERT G. HAWKINS, Ph.D. New York 1966. Canadian economic stagnation, the flexible exchange rate, and economic policy, 1957-1961.

REUVEN HENDLER, Ph.D. Pittsburgh 1967. An extension of the theory of comparative advantage.

HOWARD J. HILTON, JR., Ph.D. George Washington 1967. Aid—the problem of definition.

The dissertation analyzes the problems involved in defining the term, discusses concepts for distinguishing aid from all other resource transfers, proposes a definition of aid, suggests methods and standards for measuring the net transfer represented by aid, and concludes with recommendations for action to give effect to the definition.

HUGH HIMAN, Ph.D. Illinois (Urbana) 1966. The balance of payments effects of direct foreign investment upon the host economy.

DOROTHY HODGES, Ph.D. Wisconsin 1966. An international comparison of industry production functions: implications for international trade theory.

JAMES M. HOLMES, Ph.D. Chicago 1967. An econometric test of some modern international trade theories: Canada 1870-1960.

The modern theory of the balance of payments and the purchasing power parity theory are formulated in a five equation, sixteen variable model. These theories imply ten statistical tests in one reduced form equation. One identifiability test can also be made from this equation. Yearly data is used for Canada and the U.S. for 1870-1960. Both of these theories are acceptable on the basis of the tests made. This thesis has been reprinted in the "Krannert Paper Series" and is available through the Department of Economics at Purdue University.

JOHN IRON, Ph.D. Johns Hopkins 1967. Economic development and the external sector (with reference to post-war Brazil).

An attempt to study the Brazilian economy's vulnerability to export real earnings fluctuations. Reduced-form relationships for real gross product, real total gross investment, and total real consumption are derived from a model in which the real value of merchandise export earnings is an exogenous variable. The conclusion is that during the sample period export real earnings instability destabilized real total gross investment and real consumption and very slightly stabilized real gross product.

GEORGE J. KALAMOTOUSAKIS, Ph.D. New York 1966. Economic integration and economic development: a case study of Greece and the Common Market.

GENE E. LABER, Ph.D. Maryland 1967. International travel in the Canadian balance of payments, 1949-1964.

The objectives of this study are to (1) explain the determinants of Canadian travel transactions and (2) to explain the secular behavior of Canadian travel receipts and payments between 1949 and 1964. A conceptual framework for analyzing travel receipts is developed theoretically and statistically tested on a cross-section group of U.S. automobile travellers. Canadian receipts from state *i* are found to be significantly affected by income in state *i*, past migration, and distance. Regression estimates reveal that over

the 1949-1964 period, Canadian travel transactions were generally elastic with respect to prices and income. Canadian receipts from the OECD area were substantially depressed by exchange restrictions prevailing in the early 1950's.

PETER D. LATIMER, Ph.D. Fletcher School 1966. The formation of the Alliance for Progress.

CHUNG H. LEE, Ph.D. California (Berkeley) 1966. The demand for foreign securities and long-term capital movements: the United States-Canadian case.

KIE B. LEE, Ph.D. Virginia 1967. Measure of and factors affecting "the level of trade dependence" with special application to Great Britain, Japan and Taiwan.

The dissertation is concerned with the empirical study and conceptual problems of Werner Sombart's alleged law of the declining dependence on trade. There is a discussion concerning factors that have the effects of raising or reducing such a "trade dependence" measured in the ratio of commodity trade to aggregate income. The relationships between the changes in such trade ratios and the standard of living for Great Britain, Japan, and Taiwan are examined in detail.

RAYMOND LUBITZ, Ph.D. Harvard 1967. United States direct investment in Canada and Canadian capital formation, 1950-1962.

The study tries to estimate the net contribution (on aggregate and sectoral levels) to capital formation of United States direct investment in Canada. It employs a statistical model in which Canadian capital formation is made a function of domestic Canadian determinants of capital formation (accelerator and residual funds variables) and direct investment. The size and significance of the direct investment variable showed that the direct investment did make a net contribution.

JOHN LYNCH, Ph.D. Georgetown 1967. The voluntary export quota as a modern type of quantitative restriction—a case study of Japanese voluntary controls on cotton, textile, exports.

The voluntary quotas that the Japanese placed on their cotton textile exports to the United States and later to other parts of the world marked a major step away from the avowed spirit of trade liberalization embraced by developed countries after Breton Woods. After a consideration of the history of the Japanese textile industry, a detailed account of American pressures for controls and of Japanese conditions leading to controls is presented. The final chapter features an attempt to analyze what is meant by the frequently used term "orderly market."

SHAH MAHMOUD, Ph.D. Columbia 1967. The balance of payments problem in underdeveloped nations: a case study of Afghanistan.

ROBERT H. MASON, Ph.D. Stanford 1967. Joint international business ventures in less-developed countries.

JAMES R. MELVIN, Ph.D. Minnesota 1966. Intermediate goods and international trade theory.

RICHARD H. MEYER, Ph.D. Columbia 1967. Stabilization loans and credits: 1926-1928.

The history of the negotiation of monetary stabilization loans and credits to the governments and the central banks of Belgium, Italy, Poland and Rumania is studied from the point of view of their impact on the relations between the world's major central banks (Bank of England, Bank of France, Federal Reserve System). In addition to the published major pertinent material, the dissertation relies heavily on unpublished material from the files of the Federal Reserve Bank of New York.

NORMAN C. MILLER, Ph.D. Pittsburgh 1966. The theory of international capital flows: a restatement.

NORMAN N. MINTZ, Ph.D. New York 1965. Economic union and currency integration.

VICHITVONG NA POMBHEJARA, Ph.D. Harvard 1967. Monetary management under the gold-exchange standard: a study of Thailand's experience, 1902-1941.

This dissertation examines the gold-exchange standard from the standpoint of a small developing country, using Thailand during 1902-41 as a case study. The holding of foreign exchange as monetary reserve is considered a logical practice, whereas the need

for a large reserve is imperative. Arguments in favor of a system of fixed exchange rate are also presented.

SYED N. H. NAQVI, Ph.D. Princeton 1966. Commercial policy and resource allocation in Pakistan.

This study attempted an empirical analysis of changes in the "allocative bias" of Pakistan's commercial policy during 1953-63. It revealed that, contrary to prevalent views in Pakistan, these policy changes were broadly consistent with overall development strategy. This empirical analysis was supplemented by a theoretical examination of the question of the "efficiency" of commercial policy in promoting the "desired" pattern of investment. The discussion of "structure" of Pakistan's commercial policy provided the necessary institutional background.

YOON B. OUI, D.P.A. New York 1966. An analysis of the Kennedy Administration's 1961 proposal for a new economic aid policy and aid organization.

JOHN PIPPENGER, Ph.D. California (Los Angeles) 1966. The behavior of flexible exchange rates: theory and evidence.

The type of exchange rate behavior implied by theories used to explain flexible rates, e.g., destabilizing speculation, is developed. Autocorrelation, spectral analysis, and analysis of runs are used to analyze behavior of flexible rates (daily) for six countries during the 1920's, and Canada during the 1950's. The conclusions based on a comparison of implied and observed behavior are: (1) speculation is not destabilizing; (2) stabilization funds have stabilized exchange rates; and (3) devaluation does not lead to inflation.

KOVIT POSHYANANDA, Ph.D. Cornell 1967. The European Economic Community as an effective Customs Union: an analysis of trade in chemical and machinery products.

WILLIAM V. RAPP, Ph.D. Yale 1967. A theory of changing trade patterns under economic growth: tested for Japan.

T. RUSSELL ROBINSON, Ph.D. Yale 1967. Foreign trade and economic stability: the Canadian case.

HARL E. RYDER, JR., Ph.D. Stanford 1967. Optimal accumulation and trade in an open economy.

Optimal accumulation is considered in two neoclassical two-sector two-factor models. We seek to maximize discounted per-capita consumption. In the first model, the terms of trade depend on the amount offered. Departures from competitive trade result not from dynamic considerations, but from static monopoly power. The second model has endogenous technical change in the investment good sector. There may be specialization in the investment good despite a (temporary) comparative advantage in the consumption good.

ROBERT H. SHAFFER, Ph.D. Duke 1967. The balance of payments of a high income primary export dependent economy: the case of New Zealand.

GUY V. G. STEVENS, Ph.D. Yale 1967. Fixed investment expenditures of foreign manufacturing affiliates of U.S. firms: theoretical models and empirical evidence.

ALEXANDER K. SWOBODA, Ph.D. Yale 1967. Outer currency uses, capital mobility, and the Euro-dollar market.

KIMON VALASKAIEIS, Ph.D. Cornell 1967. The influence of French nationalism of the balance of payments and balance of indebtedness of France, 1870-1914.

DALE R. WEIGEL, Ph.D. Stanford 1966. The relation between government economic policy and direct investment in developing countries.

SIDNEY WEINTRAUB, Ph.D. American 1966. Trade preferences for less-developed countries: an analysis of United States policy.

The study analyzes current proposals for preferential tariff treatment of manufactured exports from developing countries. Its focus is on the potential benefits and costs to developing countries of three overall types of preferences: (i) general preferences by most developed to most developing countries, without distinction among the latter; (ii) selective preferences by particular developed to particular developing countries for se-

lected products; (iii) preferences among developing countries. Suggestions are made regarding future United States trade policy.

WHATARANGI WINIATA, Ph.D. Michigan 1956. United States managerial investment in Japan, 1950-1964: an interview study.

GEORGE D. WOOD, Ph.D. Pennsylvania 1967. The impact of the European coal and steel community on the level and direction of trade in basic steel commodities.

MERLE I. YAHR, Ph.D. Columbia 1967. Estimating the elasticity of substitution from international manufacturing census data.

This empirical work suggests that the point estimate of the elasticity of substitution for all-manufacturing industry is slightly less than unity, but for many individual industries, the estimates of the elasticities are not significantly different from unity. The all-manufacturing elasticity is significantly different from zero, as are the elasticities for the detailed industries. Furthermore, it is shown that differences in the quality of the labor input affect the elasticity of substitution.

ERNEST V. ZUBER, JR., Ph.D. Iowa 1967. The economics of the UNCTAD Conference.

Business Finance; Investment and Security Markets; Insurance

EARL M. ARIS, Ph.D. Michigan 1966. A comparative study of the impact on financial reporting of investment allowances in the United Kingdom and Australia and the investment tax credit in the United States.

JOHN S. BURR, Ph.D. New York 1966. Relevant factors in the financial analysis of steel-works projects in developing countries.

GEORGE DOUGLAS, Ph.D. Yale 1967. Risk in the equity markets: an empirical appraisal of market efficiency.

H. RUSSELL FOGLER, Ph.D. Columbia 1967. A study of the use of common stock by large, listed corporations: 1946-63.

ROBERT J. FRAME, Ph.D. Colorado 1966. Corporate financial policy and growth: a behavioral model.

CHARLES B. FRANKLIN, JR., Ph.D. Florida 1965. The treatment of security holders under the absolute priority rule in Chapter X reorganizations.

Possible inequities and means of minimizing them are examined. The valuation required by the absolute priority rule results in some degree of unfairness. The capitalization of expected earnings method is inexact when applied. No provision is made for error. Greater participation in the reorganized corporation may be provided by hierarchical capital structures with different rates of capitalization for each stratum of new securities. Warrants should be allowed as a stratum for maximum valuation and equity.

JAMES A. GENTRY, D.B.A. Indiana 1966. An analysis of the characteristics of the common stocks purchased and sold by six life insurance companies and six balanced investment companies in 1963 and 1964.

PAUL T. GROSS, JR., Ph.D. Columbia 1967. True yields earned in electric public utilities operating under alternative regulatory formulae and managerial investment practices.

SANFORD R. KAHN, Ph.D. Cincinnati 1967. An evaluation of the real estate investment trust.

MILTON LEONTIADES, Ph.D. American 1966. Financing corporate investment and the role of the capital market.

The "market test" as developed in classical economic theory referred to an economy typified by many small capitalists and a few financial institutions. Today, corporations account for the predominant share of new capital formation and finance such expenditures primarily from internally generated funds. The significance of earlier economic theory in explaining supply and demand of investment funds is examined in view of the drastically altered business scene.

E. J. LEVERETT, JR., D.B.A. Indiana 1967. A simulation of the financial operations of a life insurance company under various operating assumptions with special emphasis on solvency and paid-in surplus and capital.

- STEWART C. MYERS, Ph.D. Stanford 1967. Effects of uncertainty on the valuation of securities and the financial decisions of the firm.
- HUMBERT O. NELLI, D.B.A. Georgia State 1966. The insurance business and its role in the economy and growth of a state—the Georgia experience.
The purpose of this study is to identify, measure and analyze the role and impact of insurance on the economy and growth of Georgia, and to develop a methodology which might be of benefit in the conduct of similar studies in other states.
- GEORGE PHILIPPATOS, Ph.D. New York 1966. Discrepancies of expectations in financial analysis with an application to dividend policy.
- SHANNON P. PRATT, D.B.A. Indiana 1967. Relationship between risk and rate of return for common stocks.
- T. M. RUSSELL, Ph.D. Toronto 1966. Retention financing, dividends and share prices: a case study of the Canadian primary iron and steel industry, 1947-1961.
- SURENDRA A. SINGHEVI, Ph.D. Columbia 1967. Corporate disclosures through annual reports in the United States of America and India.
- GARY K. STONE, Ph.D. Pennsylvania 1966. An analysis of the complaint handling procedures of the state insurance departments.
- SAMUEL TALLEY, Ph.D. Syracuse 1966. A capital requirement to eliminate asset restrictions.
- MAU-SUNG TSAI, Ph.D. Southern Illinois 1966. Optimum bond investment decision under uncertainty.
- MATTHEW F. TUTTE, D.B.A. Indiana 1967. An analysis of variables which affect the sensitivity of stock prices to news.
- HAROLD E. WYMAN, Ph.D. Stanford 1967. Capital budgeting in underdeveloped countries.
- ALLAN E. YOUNG, Ph.D. Columbia 1967. The managerial, market, and investor problems associated with the common stock tender offer.

Business Organization; Managerial Economics; Marketing; Accounting

- REIN ABEL, Ph.D. Columbia 1967. The German experience with uniform accounting and its relevance to the U.S. controversies on uniformity.
- FRED C. ALLVINE, D.B.A. Indiana 1966. The patronage decision-making process.
- JAMES H. BEARDEN, Ph.D. Alabama 1966. A measure of the occupational status of purchasing agents in twenty manufacturing firms in Birmingham, Alabama.
The major objective of this study was to investigate the status perceptions of a selected group of purchasing agents in order to determine the validity and value of various claims regarding the status of industrial purchasing agents. This was accomplished by using the semantic differential to gather data from a study population of purchasing agents, sales managers, and top management executives.
- EUGENE T. BYRNE, JR., Ph.D. Tulane 1967. The theory of investment of the firm under uncertainty.
The dissertation begins with an evaluation of current approaches to the problem of selecting among alternative investment proposals. These approaches have one or more shortcomings, the major one being the inadequate treatment of non-certainty. An approach is developed that utilizes developments in the areas of subjective probability and Von Neumann-Morgenstern utility supplemented by the Monte Carlo technique. The result is an approach more suited to investment decision-making under conditions of non-certainty.
- NATALIE CALABRO, Ph.D. New York 1966. Theory on which to increase the volume of subscriptions in a mail-order publishing business.
- ARTHUR E. CARLISLE, Ph.D. Michigan 1956. The effect of cultural differences on managerial and industrial relations policies and practices: a study of U.S. controlled companies operating in English and French Canada.

THOMAS J. COURCHENE, Ph.D. Princeton 1967. Inventory behavior and the stock-order distinction: an analysis by industry and by stage of fabrication with empirical applications to the Canadian manufacturing sector.

The central hypothesis (confirmed) of the thesis is that full understanding of inventory behavior requires disaggregation both by industry and by stage of fabrication. At the base of the theoretical argument leading toward the development of this hypothesis is the distinction between those industries producing to stock and those producing to order. The empirical work (quarterly, 1955-1962) deals separately with raw materials, goods in process, finished products, and total inventories, and for each maintains a disaggregation by industry.

W. DAVID DOWNEY, Ph.D. Purdue 1967. Customer services and promotions in supermarkets.

This study was designed (1) to explain the combinations of services and promotions of supermarkets in terms of external and internal environment of the firm; (2) to measure and evaluate the effects of these services and promotions on operating and financial characteristics of the firm. Results indicated that some services have been so widely accepted that they have become a necessary cost of doing business. Sales, expenses, and gross margins were influenced to varying degrees by customer services such as parking lots, number of hours open, etc.

SIERT E. DE JONG, Ph.D. Rice 1967. An analysis of capital budgeting models under various assumptions of non-profit maximizing behavior.

The dissertation investigates capital budgeting decisions by the firm operating under either sales maximization goals (Baumol) or maximization of managers' utilities (Williamson). The differences between these rules under these goals and the profit maximization goals are then investigated.

ROGER A. DICKINSON, Ph.D. Columbia 1967. Models of department store buying behavior.

MARY F. DUNSTAN, Ph.D. Alabama 1966. Marketing scientists in the United States: a study of mobility and relative achievement.

An investigation of the extent and nature of socioeconomic, educational, geographic and occupational mobility of marketing scientists—defined individuals listed in *American Men of Science* whose academic discipline is marketing. The relationship of mobility to measures of achievement—income, job title, and an achievement index based on earned doctorates, publications, activity in professional associations, and professional honors is examined. The findings evidence no important differences when compared to those reported for other elite groups.

ARTHUR ELKINS, D.B.A. Indiana 1967. An organizational model of oligopoly.

WALTER P. GORMAN, III, Ph.D. Alabama 1966. Market acceptance of a consumer durable good innovation: a socio-economic analysis of first and second buying households of color television receivers in Tuscaloosa, Alabama.

ARNOLD F. HASELEY, Ph.D. Purdue 1966. Information and decision-making in a management game.

This study examines managerial behavior in a computer programmed simulated business environment and the relationship between information feedback and firm performance. The study also explores the use of management games as tools for teaching aspects of managerial planning and control.

MURRAY HAWKINS, Ph.D. Ohio State 1967. An analysis of the structure and conduct in the wholesale meat industry.

The presentation is based upon analysis of data received from 24 retail grocery firms in Ohio. The text is concerned with formula pricing, procurement techniques, product specifications, advertising and promotional budgets and many other facets of retail meat operations. Current measures of market power do not utilize subjective criteria in evaluating market performance. The text presents a theoretical index in an attempt to remedy that and other deficiencies inherent in the market share concept.

LONNIE JONES, Ph.D. Ohio State 1967. Short-term forecasting models for average monthly prices of wholesale beef.

This study developed and tested short-term forecasting equations which may be applied to forecast monthly prices and price changes for wholesale beef. The technique used for generating forecasts from current data was to select indicators of future supply-demand conditions whose influence on price would be exerted during the forecast month. Coefficients of the forecasting equations were estimated by relating lagged values of these indicators to wholesale prices during the 1960-1965 period.

IRVING LEVESON, Ph.D. Columbia 1967. Nonfarm self-employment in the U.S.

The number, incomes and characteristics of nonfarm proprietors are examined. Opportunities for disadvantaged groups are considered. Declines in the relative importance of small businesses did not contribute perceptibly to productivity growth in the economy. Earnings of proprietors are surprisingly consistent with information on demographic characteristics. Labor's share is found to be high, supporting the notion of constancy of labor's share of national income over time. Secular declines in the relative number of self-employed have been mainly due to a movement along a supply curve.

JACK LINTOTT, Ph.D. Michigan 1966. Management development: a study of the decision process and activities in the use of outside-sponsored marketing management programs.

WILLIAM H. LUCAS, Ph.D. Alabama 1966. An empirical study of the computation and utilization of an acceptable rate of return for capital expenditures.

CHARLES D. MECIMORE, Ph.D. Alabama 1966. A study of the use of statistical techniques for the analysis of financial statements.

STEPHEN MICHAEL, Ph.D. Columbia 1967. How managers are fired: due process in the resolution of conflicts between the individual and the large private corporation.

VASANT NADKARNI, Ph.D. New York 1966. A study of the new managers in India.

ROGER G. NOLL, Ph.D. Harvard 1967. An economic analysis of network operations research techniques.

Network operations research techniques have become an important analytical tool for attacking certain kinds of production management problems. Some network techniques, such as PERT and the Critical-Path Method, are designed to be applied to production processes with technological and organizational characteristics that differ from those assumed in conventional microeconomic theory. This dissertation examines the connection of network or techniques to the conventional theory of production, and the contribution that each field can make to the other.

MITCHELL S. NOVIT, Ph.D. Michigan 1966. A field study of role conflict and ambiguity among managers in a modern department store.

ALLAN L. PENNINGTON, D.B.A. Indiana 1967. Bargaining processes in retail appliance transactions.

RICHARD B. PETERSON, Ph.D. Wisconsin 1966. The status of managerial rights in Swedish collective bargaining.

The purposes of this study were to trace the development of the management rights issue in Swedish collective bargaining and to determine if the Swedish experience has relevance for other industrial nations. In the analysis the legal, social and demographic, economic, political, institutional, and contractual factors which have influenced the status of managerial rights in Sweden were considered. It was determined through this analysis, that the Swedish experience does have some relevancy to other western industrial nations.

JOSEPH D. PHILLIPS, Ph.D. Alabama 1966. Normal volume costing: a specialized costing technique for decision making and control.

WALTER J. PRIMEAUX, JR., Ph.D. Houston 1967. An analysis of the structure-conduct relationship of firms in a selected retail market.

This is an in-depth study to ascertain how satisfactorily received economic doctrine explains the structure-conduct relationship in a particular retail household consumer market. The hypothesis tested is whether received economic doctrine, eclectically applied, satisfactorily explains structure-conduct relationships with regard to the following variables: store location, competitive goals, advertising, pricing policy, price discrimination, and product-line policy. The findings are that received economic theory explains such structure-conduct relationships rather well although certain exceptions are noted.

FREDRIC RAINES, Ph.D. Wisconsin 1967. A decision rule approach to dynamic cost estimation.

RONALD SAVITT, Ph.D. Pennsylvania 1967. Dual distribution and market competition: the ethical drug industry.

The dissertation explores the development of dual distribution in the ethical drug industry and examines the nature of competition at the wholesale level. The basic conclusions are: (1) the market share of drug wholesalers has declined; (2) competition among general line drug wholesalers and direct selling manufacturers has increased to the benefit of retail pharmacists; (3) new legislation for the control of this practice in the ethical drug industry is not warranted at this time.

LEE J. SEIDLER, Ph.D. Columbia 1967. Accounting and economic development.

ZALMAN A. SHAPELL, Ph.D. Maryland 1967. Modifications of budgeting procedures and performance measures for use by department stores.

MARTIN B. SOLOMON, JR., Ph.D. Kentucky 1967. Investment decisions in small business.

Little attention has been given to the small business man in the area of investment decisions. This study attempts to examine existing theory, current empirical study in large corporations, and then contrast these with actual practice in small firms. The conclusions indicate that small business investment problems are somewhat different from their big brothers'. In particular, the search function so vital in productive capital budgeting may require greater attention in the small firm. Case studies of actual decisions in the making are included.

GENE L. SWACKHAMER, Ph.D. Purdue 1966. A management control system for processed meat firms.

This study brings together in a management control system as much relevant decision information as is practical. Two models form the core of the system: (1) the allocative linear programming model, and (2) the sales forecasting model. The integration of these models with appropriate solution sensitivity analysis, performance analysis, and management review constitutes the management control system. The total systems approach to management problems as developed with the case firm has resulted in significant advances in control production and marketing.

CHARLES E. TREAS, Ph.D. Alabama 1966. Selected ordinal measures of travel-related information cues and choices affecting automobile motorist travel behavior: a case study.

A psychometric technique (Thurstone paired comparisons) was used to construct rank interval scales of importance for selected information sources to motorists in choosing motels. A sample of 133 respondents interviewed in 15 Birmingham lodging establishments ranked non-media-oriented cues above media-oriented ones. "Previous experience" and "appearance of the establishment" were highest. Others, in order were as follows: advice of individuals, referral services, reservation services, billboards, credit card directories, official signs, broadcast media, print media and novelties.

MARVIN W. TUCKER, JR., Ph.D. Alabama 1966. A probabilistic approach to the measurement of current assets under uncertainty.

This dissertation explores the measurement of current assets within a framework of (1) asset recognition according to the going-concern concept, and (2) the assumption of uncertainty of the asset amount. The various uncertain variables for each current asset are identified, and probabilistic models are used in their evaluation. The assets accorded treatment as random variables include marketable securities, accounts receivable, and inventories.

ANTONIE VAN SEVENTER, Ph.D. Michigan 1966. The theory of the dualistic balance sheet.

JOSEPH B. WASHINGTON, Ph.D. Alabama 1966. Matrix analysis of an accounting system, an empirical study.

In this empirical study a conventional accounting system is translated into matrix formulation in accordance with input-output analysis framework. The model developed is a flow system indicating the interdependence of flows between respective accounts. Testing indicated insufficient stability to justify use of the model for predictive purposes. The methods utilized might alternatively be used to focus attention on changes occurring between ac-

counting periods, thereby facilitating increased understanding of an accounting system that is being analyzed.

NEIL H. WEISS, Ph.D. Columbia 1967. The attest function and the annual report as an instrument of accountability.

RICHARD F. WENDEL, Ph.D. Pennsylvania 1966. Occupational differentiation as the basis of market segmentation.

HARDOL C. WHITE, Ph.D. Florida 1966. A concept of management development: applications for a health center.

The philosophy of management recognizes the dignity of man in an ordered universe. The individual possessing an equalitarian personality is the most effective group worker and leader. Learning theory is directed toward the development of the adequate personality. The design of a program for management development is based on the conclusion that the most effective learning situations also provide experiences of the manner in which the manager is being encouraged to perform.

YORAM WEND, Ph.D. Stanford 1967. Organizational factors as motivating factors in the decision to purchase industrial products.

BILL VASTINE, Ph.D. Ohio State 1967. An analysis of present and future roles of store managers and supervisors in the retail and wholesale food industry.

Based upon questionnaires completed by 145 management personnel, the study findings suggest as more decisions and functions are transferred from retail store level to computer supported headquarters, the managers of chains and independent retailers will become more similar and will become increasingly customer and employee oriented. Supervisors will become more highly trained specialists capable of quasi-research and small group instruction. The implications of these trends in selecting and developing managerial talent are proposed.

KENNETH E. WING, Ph.D. Cornell 1966. Central market quotation pricing of eggs.

Industrial Organization; Government and Business; Industry Studies

HUSSEIN A. ABDEL-BARR, Ph.D. Wisconsin 1966. The market structure of international oil with a special reference to the organization of petroleum exporting countries.

HAMDY H. AFIFI, Ph.D. Illinois (Urbana) 1967. Economic evaluation of pricing water supply in Illinois.

ANTHONY J. BLACKBURN, Ph.D. Mass. Inst. Technology 1967. A nonlinear model of passenger demand.

JAMES A. BROWN, Ph.D. Duke 1966. Antitrust and competition in the glass container industry.

WILSON B. BROWN, Ph.D. Fletcher School 1966. Governmental measures concerning exportation in Peru: a study of policy and its making, 1945-1962.

CHING C. CHEN, Ph.D. Mass. Inst. Technology 1967. Crude oil prices and growth of the Japanese oil refining industry.

DOROTHY COHEN, Ph.D. Columbia 1967. The Federal Trade Commission and the regulation of advertising in the consumer market.

THOMAS C. COMMITTE, Ph.D. Alabama 1966. An analysis of the economic philosophy followed by the Federal Power Commission in administering Section 1 (b) of the Natural Gas Act.

PETER J. CASSIMATIS, Ph.D. New School 1967. The performance of the contract construction industry, 1946-1965.

This study has surveyed some of the most important aspects of the economics of construction which can be used to assess the performance of the industry and review its prospects. The central finding of this analysis is that potential economies of large scale are not realized. This accounts in part for lagging productivity and increasing construction costs. Following the analysis a number of recommendations are offered toward the im-

provement of the industry's performance, including some suggestions for economic policy with regard to construction activity.

ELISEO DA ROSA, Ph.D. Kentucky 1967. Planning highway investment in Argentina.

An analytical framework to support applications of benefit-cost analysis in planning highway investment in Argentina is developed from conventional principles of capital investment, public expenditure theory, welfare economics, and interregional programming methods. The model is applied to Argentina's highway data to estimate rates of return to representative highway projects, to appraise the extent of regional imbalances in fund allocation, to evaluate shortcomings in pricing and financing practices, and to identify some urban transportation problems.

MARTIN J. DAVIDSON, Ph.D. Houston 1967. Economic rationale of science policy in the federal establishment.

The dissertation focuses upon rationale in the federal government's increasing allocation of resources to research and development. The role of cost-benefit analysis is considered and an attempt is made to determine if research and development allocations are internally consistent with values made manifest through the democratic political process. It is concluded that such resource allocations are increasingly rational. A model suggests the federal government's more effective pursuit of the goal of economic growth through allocation of resources to civilian research and development.

EDWARD N. DODSON, Ph.D. Stanford 1966. An evaluation of component product flows within the electronics industry.

ROBERT W. DOEDE, Ph.D. Chicago 1967. The monopoly power of the New York Stock Exchange.

The thesis illustrates that the New York Stock Exchange is one of the most effective monopolies in the United States. It shows that the monopoly is based on economies of scale in exchange organization. Finally the fluctuation in monopoly profits over the last forty years is analyzed by means of a variable weight adaptive expectations model.

JOSEPH J. FINK, Ph.D. New York 1966. The economics of entry into the petrochemical-plastics industry.

RENATE T. FLEMING, Ph.D. Alabama 1967. Changing aspects of private property concepts—a case study of the vested property: the American Potash and Chemical Corporation under the management of the alien property custodian.

FRANCIS M. FLETCHER, Ph.D. Pennsylvania 1966. Free market restrictions in the retail drug industry.

Each state has established a Board of Pharmacy to serve as an occupation licensure board for pharmacists and to regulate drugstores. Because the Board is usually controlled by independent drugstore owners through the State Pharmaceutical Association, many laws and regulations are in force that restrict the activities of chain drugstores, department stores, supermarkets and discount stores. The dissertation focusses upon an analysis of these restrictive practices, mechanisms that perpetuate the devices and the economic and managerial implications of the restrictions.

HAROLD W. FOX, Ph. D. Rutgers 1967. The economics of distributing merchandise through the trading-stamp channel.

A stratified random sample of Sperry & Hutchinson redemption merchandise was priced in department stores and discount houses in six cities; average retail value was 21% above the Company's average proceeds from retailers for the corresponding number of Green Stamps.

MAHSOUN B. GALAL, Ph.D. Rutgers 1967. An equilibrium analysis of the American crude oil industry.

The equilibrium analysis is of a short- and a long-run dimension. An analysis of the market equilibrium when "concentrated oligopoly-oligopsony with leadership"; vertical integration; and percentage depletion allowance characterize the industry. An analysis of the allocation of resources between this industry, where crude production is argued to be externality creating, and the rest of the economy. The relevant tax provisions would correct the resources allocation if the "rationality hypothesis," that in the long run ad-

dition to proved reserves at year t is equal to expected production at a future year $t + e$, was accepted.

HOWARD A. GILBERT, Ph.D. Oregon State 1967. An integration of market structure theory and decision making in selected agricultural industries.

This research tested the applicability of commonly accepted market structure theory to firm behavior within agricultural processing industries. It was primarily concerned with testing the influence of relative firm size or discretionary management decisions. Three firms of differing relative size in each of five industries provided data indicating actual behavior. Findings did not substantiate a close relationship between firm size and decision discretions in product prices, investment and technical innovation. Consequently, alternative hypotheses for future research were identified.

HENRY G. GRABOWSKI, Ph.D. Princeton 1967. The determinants and effects of industrial research and development.

Several hypotheses on the determinants and effects of industrial research and development expenditures are formulated, and then tested on firms in the chemical, drug, and petroleum industries. The empirical results indicate a significant relation between R&D and various other technological, financial, and marketing variables of the firm. The implications for government antitrust and fiscal policy are considered, and some theoretical models which incorporate the main empirical findings are developed to analyze various policy questions.

IRWIN GRAY, Ph.D. Columbia 1967. Employment considerations in the award of public contracts.

RICHARD E. HAMILTON, Ph.D. Duke 1966. The Damodar Valley Corporation: India's experiment with the T.V.A. idea.

RICHARD HELLMAN, Ph.D. Columbia 1967. Government competition in the electric utility industry of the United States: case studies of origins and results.

This empirical and historical analysis covers some 1,500 cases, 100 individually and others mainly in aggregates. A statistical definition of government competition—threatened, by example, and duplicative—is developed. Through comparisons of rates, usage and financial results with and without competition, the theories of a natural monopoly and of regulation as “the law’s substitute for competition” are demonstrated to be incompatible with the real world. A theory of government competition as a regulatory form is developed. Chapters include individual city case studies, the Power Authority of New York State, the Tennessee Valley Authority, and the Rural Electrification Cooperatives.

HENRIK HELMERS, Ph.D. Michigan 1967. Some effects of the United States-Canadian Automobile Agreement.

SEIKO HIGA, Ph.D. Northwestern 1966. Effect of motor carrier competition on railroad rates: theoretical and statistical analyses.

RAYMOND JACKSON, Ph.D. Boston 1967. An empirical evaluation of electric utility regulation.

Regulation is intended to provide lower electric rates, prevent undue discrimination and limit rates of return. Using econometric methods this study examines various aspects of regulation to see whether they significantly affect the achievement of these goals. The results indicate the only effect of regulation is to reduce the level of rate increases when prices are rising. When electricity prices are falling regulation is ineffective since commissions do not aggressively pursue greater price reductions.

HENRY D. JACOBY, Ph.D. Harvard 1967. Analysis of investment in electric power.

This dissertation is devoted to the development of an improved method for analyzing alternative investments in electric power generation and transmission. The result is a new planning model based upon digital computer simulation of the long-run capacity expansion and short-run operation of electric power systems. Use of the model is demonstrated in sample applications to two distinctly different electric power markets, one in Argentina and the other in Pakistan.

JAMES H. JORDAN, Ph.D. Pennsylvania 1956. Contracting out the maintenance function in the chemical and oil refining industries.

Evaluation of "contract maintenance," the utilization of outside maintenance workers in the chemical and oil refining industries from the viewpoints of flexibility, labor relations, cost, and quality of service. Includes a cost analysis for computing optimum workforce size. Conclusions: the chief advantage of contract maintenance is flexibility; it is used most economically to supplement an in-plant force; fear of labor relations difficulties is unfounded; the quality of service equals or exceeds that of in-plant workers.

SUNG-WOO KIM, Ph.D. California (Berkeley) 1967. Capital appropriations and investment behavior, U.S. manufacturing industries, 1953-1964.

LINDA KLEIGER, Ph.D. California (Los Angeles) 1967. Maximization of industry profits: the case of United States air transportation.

The economic theory used in this study to explain the market behavior of United States airlines states that firms act to increase industry profits. Two necessary conditions for this action are satisfied: entry is controlled by the Civil Aeronautics Board, and behavioral controls on existing firms take the form of industry trade association agreements. The study conjectures that these agreements are viable because the fixed route system limits the expansion possibilities of any firm which decides to leave.

WESLEY R. KRIEBEL, Ph.D. Missouri 1965. A managerial-economics analysis of selected Midwestern class I motor common carriers of refrigerated commodities.

RICARDO LAGOS, Ph.D. Duke 1966. Industry in Chile: some structural factors.

ROBERT C. LIND, Ph.D. Stanford 1967. The nature of flood control benefits and the economics of flood protection.

This paper deals with the problem of assessing the benefits from various types of flood protection. The first chapter contains a discussion of the nature of the benefits from flood protection and relates the various types of benefits to five different measures for coping with flood losses. The second chapter develops a theory of land use as it relates to the structure of rents. This theoretical framework is used in Chapter III to develop the proper measure of benefits from "land enhancement." The final chapter deals with the effects of alternative programs of insurance on the cost of risk-bearing associated with flooding.

RICHARD E. LOW, Ph.D. Harvard 1967. Economic defenses under the revised Section 7 of the Clayton Act.

There are eight groups of possible economic defenses against a Section 7, Clayton Act, complaint—the failing company doctrine, the against-giants defense, the investment exception, ease of entry into the postmerger industry, the natural monopoly defense, the greater efficiency of the postmerger firm, lack of market power in the postmerger firm and the competitive level of the postmerger industry. The first five have varying degrees of judicial support; the last three do not. All eight are analyzed in the light of economic theory; all but the failing company doctrine are judged to be, when correctly defined, economically sound.

JOSEPH R. MASON, Ph.D. Boston College, 1967. The financing of research and development projects contracted to private firms: an economic study of the patent policy of the National Aeronautics and Space Administration.

An analysis of the patent policy of NASA with respect to the market structure of the industries in which recipients of patents operate. The economic issue involved is whether NASA's increasing willingness to grant patents unnecessarily contributes to monopoly power in that the invention would be forthcoming without NASA's waiver of patent rights. The author concludes that NASA's waiver policy should be reexamined.

JAMES W. MEEHAN, JR., Ph.D. Boston College 1967. Market structure and excess capacity: a theoretical and empirical analysis: an abstract.

This dissertation investigates how different market structures adjust capacity to a permanent increase in demand. Competitive industries are expected to over-adjust capacity when faced with a permanent increase in demand. Oligopolistic industries are expected to behave similarly. A single-firm monopoly is expected to perform much better in adjust-

ing capacity. The empirical investigation examined five industries and found that all, except the monopoly, overexpanded capacity when faced with an increase in demand.

DAVID MERMELSTEIN, Ph.D. Columbia 1967. Large industrial corporations and asset share maintenance, 1909-1964.

This dissertation is a decade by decade statistical analysis of the changing shares of various corporate "survivors" (firms ranked among the largest 100 in both of the two years of the several decades studied) between the years 1909-1964. Using regression analysis, this study takes into account changes in industrial structure, mergers, and anti-trust dissolutions and concludes that the largest firms are increasingly able to maintain their share of assets.

ROBERT R. MILLER, Ph.D. Stanford 1967. The steel industry and U.S. antidumping policy.

JOHN H. MOORE, Ph.D. Virginia 1966. Economics of research and development and the NASA program.

A probabilistic model of investment by firms in R&D was proposed. The model placed emphasis on the externality and uncertainty aspects of R&D. After testing, some of the model's implications regarding the effects of funding of R&D by NASA were investigated. Stimulation of private effort in R&D was found to be most likely in three industries, all of which are and have been important private funders of R&D.

BERNARD A. MORIN, Ph.D. Duke 1966. An analysis of the variation in liquor consumption and prices among states.

E. WAYNE NAFZIGER, Ph.D. Illinois (Urbana) 1967. Entrepreneurship in the footwear industry in Nigeria.

KIT G. NARODICK, Ph.D. Columbia 1967. The domestic air cargo industry.

HENRY ORION, Ph.D. Columbia 1967. Domestic air cargo, 1945-1965. A study of competition in a regulated industry.

An evaluation of the competitive market performance of the industry during the first twenty years of its existence under public regulation. Emphasis is placed on the relationship between the trunk airlines, who utilize both specialized equipment and the belly-compartments of their passenger planes, and the all-cargo carriers. Costs and rates are investigated in detail, since they are assumed to be the basic determinants of industry behavior.

D. JEANNE PATTERSON, D.B.A. Indiana 1967. Local industrial development corporation in Indiana.

JOSEPH MCG. PERRY, Ph.D. Northwestern 1966. The impact of immigration on three American industries 1865-1914.

This study measures the impact of immigration on the New England cotton goods industry, the crude iron and steel industry, and iron ore mining between 1865 and 1914. Results indicate that immigration contributed strongly to growth of output, stimulated an existing trend toward relative capital-intensity, and probably hindered rises in wage rates.

CHARLOTTE A. PRICE, Ph.D. Columbia 1967. The helium industry: a study of a federal government monopoly.

The economic history of the helium industry from its beginnings in World War I through the development of a government monopoly and its decline with the present long-term conservation program is described and analyzed. Benefit-cost estimates of the planned investment in that storage program are made and possible expansions of recovery from natural gas for storage are considered and judged a net social benefit. These calculations appear to contradict the Preston-Brooks study for the CEA.

WILLIAM A. REYNOLDS, Ph.D. Columbia 1967. Innovation in the United States carpet industry 1947-63.

The dissertation is an empirical analysis of the diffusion of technological innovation in an industry. After a review of the problem, the capital saving innovations of the tufting process and man-made fibers are shown to have lowered the barriers to entry into the established industry, resulting in substantially reduced concentration and in lower

carpet prices. The demand for carpet and the nature of the factor market affecting the supply are analyzed in detail.

FRANCIS H. ROCHE, Ph.D. Notre Dame 1966. The public interest and economic regulation of educational television.

JAMES M. ROCK, Ph.D. Northwestern 1966. The Wisconsin aluminum cookware industry prior to World War II.

This study encompasses the period from the 1893 Columbia Exposition to March of 1941, when aluminum was restricted to military use. The major objectives were to apply the economic theories of location, entrepreneurship and industrial organization to this industry. It was concluded that 1) Wisconsin had no locational advantages relative to other states in the Midwest; 2) the location was basically a function of the initial entrepreneur; and 3) the structure of the industry conformed to theory.

JAMES N. ROSSE, Ph.D. Minnesota 1966. Daily newspapers, monopolistic competition and economies of scale.

Chamberlin-like econometric models are used to test whether scale economies necessary (but not sufficient) for explaining the structure of the U.S. daily newspaper industry exist. Using multiple equation methods and an assumption of profit maximizing behavior, cost function parameter estimates are inferred from cross-section observations of price and output data. The evidence indicates that such scale economies exist. Other evidence suggests that scale economies have not changed over time.

HARVEY SCHWARTZ, Ph.D. Illinois (Urbana) 1966. The changes in the location of the American agricultural implement industry, 1850-1900.

THOMAS J. SHANLEY, Ph.D. Notre Dame 1966. The public interest and the struggle for the New England natural gas market 1950-53.

DAVID L. SHAPIRO, Ph.D. California (Berkeley) 1966. Statistical appraisal of the economic efficiency of the Trinity River Division of the Central Valley Project of the United States Bureau of Reclamation.

W. PAUL SMITH, Ph.D. Northwestern 1966. The American small arms industry, 1870-1890.

This description and analysis of the industry's development revealed two distinctive aspects of the industry's operations: improvements in ammunition permitted innovations in firearms and, as a result, significantly affected the rivalry within and, in turn, the structure of the firearms industry; and, demand segmentation provided the opportunities for smaller firms to develop market niches and thereby survive. It was also shown how the varying product lines, integration, size and entrepreneurial perspectives of individual firms affected the industry's operation and performance.

FRED A. TARPLEY, JR., Ph.D. Tulane 1967. The economics of combined utility and transit operations.

This study investigates the provision of electric, gas, and transit service by a single firm. Relevant pricing models are developed for the electric and gas industries and for urban transport these models are viewed in relation to the operations of New Orleans Public Service, the only firm in the United States providing all three services. Of special interest are the pricing and welfare implications of the subsidization of transit service from electric and gas revenue.

JUDITH D. TENDLER, Ph.D. Columbia 1966. The rise of public power in Brazil.

This is a study of the breaking of a bottleneck in the electric power sector of south-central, industrialized Brazil. The author explores the technological configuration of the industry, and its effect on three major aspects of the breaking of the bottleneck: the successful introduction of state enterprise in the power sector, the remarkable degree of coexistence that prevailed between state and foreign utility, and the protracted stay of a foreign utility dissatisfied with its rates and uncertain of its future.

JOHN M. VERNON, Ph.D. Mass. Inst. Technology 1967. Public investment planning in civilian nuclear power.

HUGH D. WALKER, Ph.D. Vanderbilt 1967. Market power and price levels in the ethical drug industry.

WILLIAM D. WALSH, Ph.D. Yale 1967. The diffusion of technological change in the Pennsylvania pig iron industry, 1850-1870.

**Land Economics; Agricultural Economics
Economic Geography; Housing**

MARCELLE V. ARAK, Ph.D. Mass. Inst. Technology 1967. The supply of Brazilian coffee.
BRUNO BALDINI, Ph.D. California (Berkeley) 1966. Reversibility and demand functions for agricultural products.

JERE R. BEHRMAN, Ph.D. Mass. Inst. Technology 1967. Supply response in underdeveloped agriculture: a case study of the four major Thai annual crops, 1937-1963.

RUSSELL L. BERRY, Ph.D. Ohio State 1967. The Scully Estate and its cash-leasing system in the Midwest.

This study indicates that major farm tenure goals are fixity or security of tenure, freedom of improvement. A survey of 103 Scully cash tenants and crop-share tenants in Marion County, Kansas, indicates that the Scully tenants has been more successful in achieving these goals due largely to the cash rents, the size of the Estate, and tenant ownership of improvements. The public relations problems of large estates are also discussed.

JOHN H. BOYD, Ph.D. Minnesota 1967. The problem of external diseconomies with special emphasis on river water pollution.

This study postulates that a technological externality arises because some asset, yielding service flows to economic units, is not subject to the laws of property ownership. The case of river water pollution is examined, and formal conditions for efficient allocation of the services of the river basin are derived. The "water quality" service satisfies a generalized definition of a public good. It is also shown that costless negotiation among affected parties cannot lead to efficiency.

BOYD M. BUXTON, Ph.D. Minnesota 1967. Economies of size in Minnesota dairy farming.

The synthetic approach is used to estimate economies of size on dairy farms. In total, sixty farm situations are synthesized from (1) specified supplies of labor and (2) selected machinery, housing, milking and feeding technologies. For each farm situation, a short-run cost curve is determined by minimizing the cost of producing several levels of output (standard linear programming techniques are used). The long-run unit cost curve is derived from the short-run unit cost curves.

JU C. CHAI, Ph.D. Minnesota 1967. An economic analysis of the demand and price structure of wheat for food by classes in the United States.

The purpose of this study is to investigate the nature of the underlying economic and physical relationships that tend to determine the domestic food consumption of each of the five major classes of wheat in the United States. Results of statistical estimation were generally satisfactory. The price elasticity of demand for hard red winter was—1.37, hard red spring—1.41, durum—1.36, soft red winter—0.36, and white—0.22.

HSING-YIU CHEN, Ph.D. Ohio State 1967. Structure and productivity of capital in the agriculture of Taiwan and their policy implications to agricultural finance.

An empirical determination of the characteristics of the capital structure and tests of the productivities of capital at the micro-level of Taiwan agriculture. It employs a cross-sectional approach on 1963 farm record data for a sample of 277 farms. The marginal revenue productivities of the disaggregated components of both farm assets and operating expenses are determined for various sub-groups of farms. Policy implications are suggested at the farm, institutional, and national levels.

NG-SHEONG CHEUNG, Ph.D. California (Los Angeles) 1967. The theory of share tenancy—with special application to the first phase of Taiwan land reform.

The theory shows that resource use under various contractual arrangements is the same as long as these arrangements are aspects of private property rights. However, a legal reduction of rental share leads to increased farming intensity and marginal inequalities of resource use. Observations from Asia not only confirm the implications of standard economic theory, but they also deny other existing hypotheses of disguised unemployment, of rental share restriction, and of tax-equivalent analyses of share tenancy.

WILLIAM J. CRADDOCK, Ph.D. Iowa (Ames) 1966. The effect of grain price on the profitability of livestock production—an econometric simulation.

JAMES E. CRISWELL, Ph.D. Minnesota 1966. Insurance strategies of West Kentucky farmers.

The three major objectives of this study were to: (1) identify some of the elements of risk and uncertainty as they appear to the farmer; (2) determine the quantity of resources allocated by farmers for protection against elements of risk and uncertainty; and (3) study the degree of relationship of such factors as age of farmer, number of dependents, education, total assets, family income, acres in operating unit, etc., to the quantity of resources allocated to formal insurance.

CORTLANDT D. DITWILER, Ph.D. California (Berkeley) 1966. Political economy of water use transfer and integrated water resource management in the Salinas Valley, California.

The analysis focuses on a problem of ground water overdraft which stems primarily from a divergence between private and social cost. A criterion encompassing community or social optimality is outlined and an optimal water management rationale for a water district is developed within the composite structure of the water economy. The study emphasizes the evaluation of political-institutional processes, legal structures and administrative techniques insofar as they influence water allocation and transfer.

MOHAMMED R. EL-AMIR, Ph.D. California (Berkeley) 1967. Location models for the world rice industry.

An attempt has been made to answer certain questions related to the world rice industry quantitatively through use of a spatial equilibrium analysis applied to international trade in rice. The major components required to implement various short-run and long-run spatial equilibrium models are developed. These components include regional demand functions and long-run forecasts (for 1970 and 1975) of consumption; regional supply functions and estimates of future production (in 1970 and 1975); and transfer cost functions for rice, both ocean and rail.

GARY H. ELSNER, Ph.D. California (Berkeley) 1966. A multi-variate analysis of California's agricultural income.

This dissertation develops and furnishes exemplification of alternative methodological approaches to regional economic analysis. Defines an approach to spatial analysis mapping. Using this approach, presents static spatial analyses for 1950 and 1960 California county patterns. Develops and discusses an efficient technique for solving the generalized Weber problem. Example applications employ California data. Suggests a new approach to income distribution analysis. This involves the formulation and estimation of a multivariate income distribution determination model.

ARNOLD M. FADEN, Ph.D. Columbia 1967. Essays in spatial economics.

This is a fairly comprehensive and rigorous development of location theory. The topics treated include: (1) the determination of optimal personal movement patterns, which yield (2) a "maximizing" explanation for gravity models. Some contributions are made to (3) the industry location problem, and, in particular, to (4) the problem of optimal scale and spacing of urban facilities. (5) The Wingo-Alonso theory is extended, and applied to income-residence patterns, land values, and transportation demand.

JOHN D. FARQUHAR, Ph.D. Southern Illinois 1966. An approach to the economic allocation and use of resources for forestry and allied purposes.

BARRY C. FIELD, Ph.D. California (Berkeley) 1967. The impact of changing economic structure on the strategy of agricultural policy: the case of cotton.

Examination of U.S. cotton price and income policy, 1964-65, to determine how policy changes developed because of structural changes in cotton economy. Included are investigations of bargaining in Congress between east and west on acreage allotments, cohesion among cotton-state legislators on policy matters, and the lagged response of producer support programs to cotton market developments. The effects of increasingly heterogeneous production conditions over the cotton belt, and the difficulties of policy "feed-back" are also discussed.

C. LYNN FIFE, Ph.D. Purdue 1967. The decision-making process in large fluid milk firms in the Midwest.

Economic goals and objectives seemed unclear to management and often changed to reflect past accomplishments of the firm. Goals were usually limited by self-imposed restrictions on credit usage and by arbitrary decisions to limit sales to local territories. Firms applied rather sophisticated economic analysis to minor and routine decisions but made many major decisions on the basis of hunches and rules of thumb. Major decisions were often prescribed by prior commitments to minor decisions.

JAMES R. GARRETT, Ph.D. Washington State 1967. A cost analysis of Northwest livestock auctions.

The main hypothesis of this study was that Northwest livestock auctions can be constructed and operated more efficiently. The hypothesis was generally accepted, based upon synthesized requirements. Fixed costs were derived from construction requirements. Variable costs were divided into two categories, labor and non-labor, and obtained farm regression equations applied to data of sampled auctions. The number of men required was found to be a function of construction design rather than volume. Two other factors were examined for their effect on average costs: (1) average size of lot and (2) fluctuations in weekly volume.

GEORGE R. GEBHART, Ph.D. Michigan State 1966. A test of the economic relevancy of the Holdridge Life Zone System.

The Holdridge Life Zone System is a system of climatic determinism under which certain ranges of three climatic factors—heat, precipitation and moisture—are thought to significantly influence economic variables. Two "life zones" were compared statistically, for variations in productivity, technology, cost of production, and land use. Corn, rice and beans were analyzed for productivity differences. The results showed no significant differences by life zones. This study was carried out in Costa Rica.

GLENN A. GILLESPIE, Ph.D. Missouri 1966. An evaluation of the factors affecting the demand for water oriented outdoor recreation.

ANTONIO GUCCIONE, Ph.D. California (Berkeley) 1967. Productivity indexes and economic theory.

Various methods for ranking farmers are discussed: (1) statistically estimated production functions; (2) Farrell's linear programming method; (3) different types of elementary indexes. The foregoing methods are applied to a sample of Italian farms and compared.

J. WILLIAM HANLON, Ph.D. Minnesota 1966. An analysis of processing costs in plants that manufacture butter and nonfat dry milk.

The short- and long-run cost structures of the Minnesota butter and nonfat dry milk manufacturing industry were estimated. Long-run costs were interpolated from short-run costs which were estimated through intensive study of four of Minnesota's most efficient plants covering the industry's plant size range. Regression analysis using historical data was used to estimate production functions for plant processes. Engineering coefficients were used where data were not available. Cost economies to size have been exhausted in the size range of Minnesota's larger plants.

MARVIN L. HAYENGA, Ph.D. California (Berkeley) 1967. Sweetener substitution in food processing industries.

The primary technological, legal, and economic factors were studied which affect the substitutability among sweeteners in five major sweetener-using (food processing) industries. Supporting econometric analyses related the market share occupied by each sweetener (in each industry) to the relative sweetener prices observed during 1949-1963. Substantial differences in the ease of sweetener substitution were noted in these industries. The dominance of sucrose (cane and beet sugar) is being eroded, requiring renewed sugar policy analysis.

DOUGLAS L. HEEREMA, Ph.D. Iowa 1966. A benefit-cost analysis of Pakistan fertilizer plants.

ROBIN G. HENNING, Ph.D. Cornell 1967. California grapes—an economic allocation problem.

EITHAN HOCHMAN, Ph.D. California (Berkeley) 1967. A problem of growing inventory with particular reference to the broiler producing firm.

The idea that factors like weight, feed, and quality are functions of age of the broiler is adopted to incorporate the growth functions into a production model. The economic theory relevant to such a production model is then applied within a static framework. A dynamic decision model is developed, based on a sequential stochastic decision process. The solution is based on the assumption of stochastic distribution of prices.

JACK HOPPER, Ph.D. Texas 1966. A study of tin: with special emphasis on the problem of the years since World War II.

AARON C. JOHNSON, Jr., Ph.D. California (Berkeley) 1967. Structural characteristics of the demand for milk in California: a quantitative analysis.

Major interest centers on the role of socio-economic variables in structuring the consumption relation for fluid milk and on the usefulness of census data in such an analysis. Analyses of both California and United States data are conducted. Questions of methodological interest focus on several problems of estimation frequently encountered in empirical analysis. An appendix is devoted to a development of the technique of principal components within the framework of regression analysis.

GERALD L. KARR, Ph.D. Southern Illinois 1966. Economic models of the feeder-swine market.

ORVAL KERCHNER, Ph.D. Minnesota 1966. Economic comparisons of flexible and specialized plants in the Minnesota dairy manufacturing industry.

This study deals with the problem of flexibility versus specialization in dairy manufacturing plants. Synthesized model plants were the bases for analyzing costs and returns. Monthly data were developed for the years 1959 thru 1964. The specialized butter plant yielded higher average yearly net returns than a flexible plant producing both butter and cheese and their associated by-products. On the other hand, the flexible plant yielded higher net returns than the specialized cheese plants.

JONGBIN KIM, Ph.D. California (Berkeley) 1966. The Korean cotton manufacturing industry, 1954-1963.

The Korean cotton manufacturing industry is analyzed in the framework of industrial organization. In terms of market performance of the industry there has been no substantial excess capacity; most of the firms have operated on small scales; the industry has earned a high profit rate; product quality has considerably improved; selling cost has been negligible. Barriers to entry, especially large capital requirement, distinctively influenced the market performance. Other dimensions of market structure and market conduct have had little effect on the industry.

SETH LAANYANE, Ph.D. Stanford 1966. The oil palm industries of western tropical Africa.

The oil palm in western African economies—geographical distribution, systems of farming, land tenure and local diets. Characteristics and agronomy of oil palm—red and white oil, palm kernel cake and palm wine. Growth and development of oil palm industries in eastern, western and central sectors. Supply of oil palm products, substitutes and economic organization. Export trade—export, import and price trends. Prospects for development of oil palm industries.

THOMAS E. LISCO, Ph.D. Chicago 1967. The value of commuters' travel time: a study in urban transportation.

A study of the marginal value of commuters' travel time and comfort. Values for time and comfort are imputed from modal choice decisions made by commuters' traveling between Skokie, Illinois and downtown Chicago. The binary choice statistical tool used is multiple profit analysis.

JOHN S. LYTLE, Ph.D. Ohio State 1967. An optimal pattern of shipment for grain in Ohio with projections to 1970.

This study delineated the areas of surplus and deficit grain production in Ohio and determined the expected changes in the relative surplus-deficit position of each county by making projections of the production and consumption of grain on a county basis for the year, 1970. Optimum flows were developed for corn, oats, wheat, and soybeans

using the net surplus—deficit position and the appropriate transfer costs as inputs for a transportation model.

JULIUS MALDUTIS, JR., Ph.D. Columbia 1967. Lithuanian land reforms 1919-1939.

ALEXANDER F. MCCALLA, Ph.D. Minnesota 1966. An analysis of the possibilities for international arrangements for temperate zone, grain-livestock trade.

Proposals for international arrangements to reduce conflicts between freer trade and agricultural protectionism are analyzed historically and in a politico-economic framework. Pricing models for wheat, feed grain and meat trade are constructed individually and then are integrated for the total sector and are used to analyze the following proposals: free trade, commodity agreements, fixed price schemes and the international negotiation of domestic policies. The latter seems most likely to succeed.

ROBERT A. MCILWAIN, Ph.D. American 1967. An investigation of resource combinations leading to intrastate differentials in agricultural income in selected southern areas in the United States.

Agricultural data from the five high average farm centers were contrasted with similar data from the five low average farm sales centers in each of the six southern states of Alabama, Arkansas, Georgia, Mississippi, Louisiana and South Carolina, to develop clues as to the factors underlying the low income farm problem in the South.

CARMEN F. MENEZES, Ph.D. Northwestern 1966. Agricultural migration and rural development in Ghana.

MAX MOSZER, Ph.D. Pennsylvania 1966. American wheat farmer and agricultural programs 1939-1964, a quantitative analysis.

JOHN M. MUNRO, D.B.A. Indiana 1966. Transportation investment and depressed regions: the case of Appalachia.

ALBERT J. ORTEGO, JR., Ph.D. California (Berkeley) 1967. Impact of California contract and pool procedures on procurement practices of milk processing firms.

Blend average prices for Grade A milk were investigated and were found to vary widely among producers delivering milk to the same plants, among plants in the same area and between areas in California. The variance in prices was separated into two components, that are attributable to producer-processor contracts and to the pooling procedures. The relationship between selected producer characteristics and price was evaluated. The impact of certain modifications of the market regulations was also considered.

KENT O. SIMS, Ph.D. Colorado 1966. An evaluation of cost-benefit analysis applied to the proposed urban redevelopment of the Platte River Valley.

QUIRINO PARIS, Ph.D. California (Berkeley) 1966. Estimation of individual firm production functions.

The study is an econometric analysis of the California dairy industry located in the San Joaquin Valley. Two samples of 65 and 21 Grade A and B farms, respectively (for which an average of 25 monthly observations were available), have been analyzed in an econometric framework which allows for the estimation of production functions with variable coefficients both with respect to firms and time. Results obtained verify the hypothesis that production functions vary among farms in the San Joaquin Valley. Furthermore, due mainly to institutional restrictions imposed by the milk price control program, a substantial number of farmers (30 per cent) operate under conditions of increasing returns to scale.

ANTHONY H. PASCAL, Ph.D. Columbia 1967. The economics of housing segregation.

This study is an attempt to explain the residential segregation of non-whites in U.S. metropolitan areas by means of various econometric techniques. Using data from Chicago and Detroit, it analyzes the roles played by economic maximizing behavior of non-white households, voluntary self-segregation, and prejudicial attitudes on the part of whites in the process of observed housing segregation. It concludes that the phenomenon cannot be explained except by the inclusion of attitudinal factors.

JOHN F. PEARCE, Ph.D. Alabama 1966. Human resources in transition: rural Alabama since World War II.

WILLIAM E. PHILLIPS, Ph.D. California (Berkeley) 1967. Regional development of the Owens Valley, California: an economic base study of natural resources.

This study constituted an examination of current and alternative use patterns of land, water and associated natural resources of the Owens Valley region in an economic and institutional framework. It serves as a basis for decision making concerning some crucial issues of resource policy. Evaluation procedure involves the use of benefit-cost analysis to interrelated problems dealing with recreation resource use, wildlife management, agricultural land and water utilization, and water export for municipal and industrial uses.

MALCOLM J. PURVIS, Ph.D. Cornell 1966. Evaluation and use of underdeveloped agricultural statistics: the food economy of Malaysia.

BERTRAND RENAUD, Ph.D. California (Berkeley) 1966. The allocation of the California grape production; an econometric study.

This is an analysis of the production and marketing strategy of an agricultural industry facing non-controllable variations of its output. The structure of the industry is analyzed in terms of production and marketing sectors. Sectorial demand functions are estimated by simultaneous equation methods. A linear theory of monopolistic discrimination with multiple outlets is formulated. The industry utility function is computed by quadratic programming under technological and economic constraints. The optimum allocation strategy under fluctuations is obtained concurrently.

JOHN E. REYNOLDS, Ph.D. Iowa (Ames) 1966. An econometric investigation of farmland values in the United States.

SURA SANITTANONT, Ph.D. Wisconsin 1967. Thailand's rice export tax: its effect on the rice economy.

JUAN F. SCOTT, Ph.D. California (Berkeley) 1967. Socio-economic performance of alternative tenure systems in Mexico.

Since the conquest of Mexico, private landownership has been favored in governmental policy-making over communal landownership at most times. The private farms were the leading tenure system in the high income region of Hermosillo, while the *ejidos* led in the low income region of Balsas. Thus, neither system is intrinsically backward. The socio-economic performance of the farmers in these regions was ascribable mainly to factors other than their systems of land tenure themselves.

BISTOK L. STORUS, Ph.D. California (Berkeley) 1967. Productive efficiency in agriculture: with special reference to the farm sector of Luzon of the Republic of the Philippines.

The method proposed by M. J. Farrell for measuring technical efficiency of production was applied to agricultural data from Pakistan and the Philippines. In the latter application, by far the more extensive of the two, the level of efficiency, using nine categories of inputs, was found to range down to 30 per cent, with an average of about 87 per cent. Estimates were made of the quantity of productive factors that could be released from farm activities presumably without reducing output. The study concludes with an attempt to relate efficiency measures to a variety of socio-economic factors.

LEON SHASHOUA, Ph.D. Cornell 1967. Long-range projections of investment in dairy housing in New York State.

WALTER A. SMITH, Ph.D. Oklahoma 1967. Water use in hotels and motels in Oklahoma.

The extent, pattern, and determinants of water withdrawal and consumption in 312 hotels and 471 motels in Oklahoma from September 1963 to September 1964 are measured in this study. Withdrawal patterns are not clearly defined. They are diffused by the number and extent of functional services other than lodging. The main determinant of withdrawal is the number of clients. The variation in total withdrawal by establishment characteristics is a function of services other than lodging.

ADAM SOKOLOSKI, Ph.D. Oregon State 1967. Welfare and institutional economics as tools in the analysis of an area water resource management problem.

This study has combined results of engineering, biological and economic investigations as a preliminary step in evaluating the economic consequences of effluent disposal alternatives. Management and governing institutions are investigated to determine institutional forms for future resource administration. Results suggest the need for (1) some form of

a dynamic, multilevel input-output model; (2) knowledge of the pulp mill's investment patterns; (3) measurement of ownership uncertainties; (4) inclusion of activities complementary to the sports fishery; (5) more precise measures of flexibility needs; and (6) further evaluation of local government forms.

KARL E. STARCH, Ph.D. Colorado 1966. A practitioner's guide to regional economic analysis.

SAM SYDNEYSMITH, Ph.D. Duke 1966. The economics of outdoor recreation: a study of the market area for Cape Hatteras.

AZRIEL TELLER, Ph.D. Johns Hopkins 1967. Air pollution abatement: an economic study into the cost of control.

The dissertation demonstrates that theoretically it is more costly to society to satisfy air quality standards through emission standards than with selective abatement. The empirical results show that it is relatively cheaper to use selective abatement rather than emission standards and if air pollution problem days are forecasted accurately, the costs decrease drastically. In addition, a method is presented and demonstrated for obtaining a schedule of equipment costs for different levels of efficiency of abatement for different industries.

PAUL R. THOMAS, Ph.D. Ohio State 1967. Comparisons between buyer estimates of live cattle yield grades and actual carcass performance.

This presentation is concerned with the factors effecting pricing accuracy and operational efficiency in the marketing process of slaughter cattle. The accuracy of cattle buyers in estimating dressing percentage, quality grade, cutability grade and rail cost, as they purchase cattle from auction markets, feed lots and terminal markets is analyzed through comparisons with actual carcass measurements. Procurement costs involved in purchasing cattle from alternative sources are also delineated and accuracy in price determination is emphasized.

SAMUEL L. THORNDIKE, JR., Ph.D. Columbia 1967. Problems in appraising the economic impact of a transportation improvement: a theoretical exploration.

Models were developed permitting theoretical determination of the true value of a transportation improvement, taking into account migration of establishments (unspecified by type) from elsewhere in the region into the affected sector. Selected situations were systematically explored. Even with simplifying assumptions (including that of homogeneous, CBD oriented establishments), the true value and impact of an improvement was seen to vary, depending on specified conditions. Some general rules for project evaluation were formulated.

NICKOLAS S. TRYPHONPOULOS, Ph.D. California (Berkeley) 1967. An investigation of the economic structure of a small urban area: Napa County, California.

This study investigates the structure and workings of the Napa County economy. The first part consists of an overview of the local economy drawing on published data and attempting to establish magnitudes for major economic variables. The second, and main part, is devoted to the construction and application of input-output tables for the county. Thirteen endogenous and four exogenous sectors are identified. A number of forecasts are made.

EDWARD W. TYRCHNIEWICZ, Ph.D. Purdue 1967. An econometric study of the agricultural labor market.

An inter-dependent six-equation model was developed for the market for agricultural labor. The model contains supply and demand equations for each of the three components of the labor force: hired, unpaid family, and operator. Distributed lag models are introduced into each equation. Time series data from the period 1929-61 are used in estimation, and Theil-Basman estimating procedures are used. The estimated models and their reduced form transformations are used to evaluate various policy alternatives and situations.

HENRY VIGUES-ROIG, Ph.D. Iowa (Ames) 1966. Economic development through agrarian reform in the Central Sierra of Peru.

TONG-ENG WANG, Ph.D. Iowa (Ames) 1966. Structural change and development in Chinese agriculture.

THOMAS F. WEAVER, Ph.D. Cornell 1967. Irrigation and agricultural development in Raipur District, Madhya Pradesh, India.

PAUL W. H. WEIGHTMAN, Ph.D. Cornell 1966. Financial incentive plans for farm labor in New York State.

ARNOLD R. WELLS, Ph.D. Minnesota 1966. The economies of beef cow herds in North-eastern Minnesota.

BILL WESTERHOLD, Ph.D. Ohio State 1967. An optimal grain storage system for the State of Ohio.

This study first determined the capacity, type of construction, age and location of grain and storage facilities in Ohio in 1954 and 1964. Second, a series of graphic transportation problems were used to determine where additional grain storage should be located to most effectively and economically utilize existing storage facilities and to minimize transport costs.

RUSSELL C. YOUNG, Ph.D. Purdue 1967. An empirical study of underemployed agricultural labor in selected areas of Minas Gerais, Brazil.

The objective of the study was to test the hypothesis of zero marginal product for agricultural labor in Brazilian agriculture. The hypothesis was tested by fitting production functions with sample data from 5 regions of the state of Minas Gerais. The MVP, evaluated at the geometric mean, was tested against zero, against the wage rate, against the MVP in other agricultural regions, and against the MVP of labor in the nonfarm sector. The hypothesis of zero MVP was rejected in three of the five regions, and a serious regional imbalance in labor allocation was found.

TERRY YU-HSIEN YU, Ph.D. Purdue 1967. Analysis of factors affecting the optimum size and number of country elevators in Indiana.

This study had the objective of determining the optimum size and number of country elevators in Indiana from an aggregate and firm standpoint. Least-cost volumes, herein defined as optimum sizes, of grain, feed and fertilizer sales were jointly determined by a cost model incorporating an internal plant cost function and an assembly or distribution cost function. Findings from this study indicated that potential cost-savings to be gained from expansions of size of business appeared to be substantial.

Labor Economics

JOHN W. ALLEN, Ph.D. Cornell 1966. A study in the nature of collective bargaining in the retail food industry.

JAMES F. ANDERSON, Ph.D. Colorado 1967. Radiation hazards and United States workmen's compensation and occupational disease laws.

HENRY E. AURICCHIO, Ph.D. New York 1966. The Hutcheson Doctrine: a study of trade union activities and the antitrust laws.

YORAM BEN-PORATH, Ph.D. Harvard 1967. Some aspects of the life cycle of earnings.

Part I (Chapter 1). A production function of human capital is introduced in a model of investment in man and is used to derive the optimal path of investment in human capital, the implied allocation of time and the life cycle of earnings. Part II (Chapters 2 and 3). An empirical analysis of the cross-section earning profiles by detailed occupation and level of education. A tentative attempt to identify "depreciation" of human productive capacity, followed by a discussion of the limitations of occupational earning profiles due to mobility.

BHALCHANDRA BHATT, Ph.D. Wisconsin 1966. A case study in the emerging industrial labor force in four factories in Bombay.

The study was based on a sample of 515 male workers selected at random from four "heavy-industry" factories in Bombay, India. The purpose of the study was to present an analytical description of the labor market behavior and characteristics of the newly emerging factory work force. A majority of workers in our sample had migrated from rural areas and for most workers the present factory employment marked their first experience in urban industrial life. The findings cover the workers' employment, training,

mobility and earnings before and after their move to the industrial factory; and attempts to arrive at cause-and-effect relationships between these variables.

MICHAEL E. BRADLEY, Ph.D. Cornell 1967. Wages and incentives in Soviet agriculture.

ROBERT D. BRITT, Ph.D. Colorado 1966. New jobs and labor mobility in depressed areas.

EDWARD K. DIX, Ph.D. Maryland 1967. Unionism in the nonferrous metal industry in the postwar period, with emphasis on the International Union of Mine, Mill and Smelter Workers.

MALCOLM S. COHEN, Ph.D. Mass. Inst. Technology 1967. Determinants of the relative supply and demand for unskilled workers.

CHARLES CRAYPO, Ph.D. Michigan State 1966. The national union convention as an internal appeal tribunal.

An empirical examination and evaluation of the national union convention as a final, internal appeal body for individual members and groups. Surveys nearly 2,000 separate appeals in 100 national and international unions between 1945-65. Shows how the national convention is dominated by the leadership and, as a result, appeals cannot be reviewed in the impartial manner we expect of judicial tribunals. Independent review boards are seen as desirable final appeal agencies.

EDGAR CZARNECKI, Ph.D. Georgetown 1967. Factors affecting the growth of the American labor movement.

A critical evaluation of membership statistics, particularly from 1950 to 1966, confirmed the lack of current union growth. Academic and pragmatic writing in the field led to the establishment of exogenous and internal union variables, which were analyzed to determine if they explained any of the recent change in union membership. The conclusion was that external variables, particularly governmental assistance, were primary to union organizing, while internal union variables, such as structure, administration and organizing techniques, were secondary.

WILLIAM E. CULLISON, Ph.D. Oklahoma 1967. An examination of union membership in Arkansas, Louisiana, and Oklahoma, 1960-1963.

This dissertation examines union membership data for Arkansas, Louisiana, and Oklahoma and presents new information derived from financial reports filed by local unions under the Landrum-Griffin Act. The new information is compared to extant data, differences are analyzed, and problems encountered in evaluating financial reports are enumerated. After examining area membership trends, it is argued that area union growth during the next decade may be relatively larger than national union growth.

ROBERT D. DEAN, Ph.D. Pittsburgh 1966. A case study in the residential distribution of selected occupation groups in the Pittsburgh metropolitan area, 1960.

LAWRENCE A. DE LUCIA, Ph.D. Arizona (Tucson) 1967. The secondary boycott and the federal law.

This dissertation gives an account of the developing public policy and the law on the national level in response to organized labor's utilization of the secondary boycott. The emergent policy developments are reflected in an examination and analysis of common and statutory law decisions prior to 1947 and in a synthesis of the decisions of the National Labor Relations Board and the federal courts from 1947 through mid-1966.

DENNIS J. DUGAN, Ph.D. Brown 1967. A dynamic model of the regional education market: an econometric analysis.

A dynamic import model for human capital (heterogenous labor groups embodying the technical progress of their formal education) is presented. The demand functions are combined with dynamic supply functions, based upon optimal decision-making under uncertainty criteria, and empirically tested. The statistical results demonstrate a high degree of substitution among the educated labor groups and the significance of risk factors in determining the rate of participation of educated labor groups.

EDWIN F. ESTLE, Ph.D. Princeton 1966. On-the-job training: a theoretical and empirical analysis.

The dissertation presents a theory of on-the-job training which holds that the supply of trainees is inelastic, and that the demand for trainees depends upon the type of

skill and the firm's turnover of trained workers. Hypotheses were tested in the Boston area by means of questionnaires and personal interviews. Concludes that training is related to replacement and growth needs in a skill. Specific-type training, however, fails to keep those trained in the firm.

HELEN GINSBURG, Ph.D. New School 1967. Wage differentials in nongovernment hospitals, 1956-1963: a study emphasizing the wages of nurses and unskilled workers in non-government hospitals.

Hospitals and other industries are compared: a) occupational and geographical wage differentials are wider in hospitals; b) hospital workers' wages are more related than other workers' wages to certain economic and demographic characteristics of SMSA's (i.e. racial composition); c) confirming Dunlop's hypothesis, nongovernment hospitals and other services but not government hospitals, form a low-wage contour for unskilled workers; d) the nursing shortage and other critical problems are related to the hospital industry's low wages and special legal status.

JUDITH C. GLASS, Ph.D. California (Los Angeles) 1966. Conditions which facilitate unionization of agricultural workers: a case study of the Salinas Valley lettuce industry.

This study encompasses an economic, historical, and sociological analysis of unionization of agricultural workers in the Salinas Valley. The changing technology of harvesting and the supply conditions of harvest labor are not independent; the interrelation of vacuum cooling, labor intensive field packing, and the bracero program is discussed. Vertical and horizontal unionization are related to market power, market structure, and mechanization. A domestic work force will reintroduce unionization via institutional and technological changes.

CECIL G. GOUKE, Ph.D. New York 1967. The Amalgamated Clothing Workers of America, 1940-1960.

DAVID H. GREENBERG, Ph.D. Mass. Inst. Technology 1967. Some industry characteristics as determinants of the structure of collective bargaining.

GEORGE S. HAGGLUND, Ph.D. Wisconsin 1966. Some factors contributing to Wisconsin occupational injuries.

In this study analyses of the factors suspected of being related to occupational work injuries were conducted utilizing approximately 80,000 work injury cases settled by the Wisconsin Industrial Commission from 1950 to 1963. In addition, California injury data were analyzed to corroborate findings derived from the original study. The most significant finding was that older workers, contrary to earlier studies, had the highest injury frequency rate in Wisconsin.

PAUL T. HARTMAN, Ph.D. California (Berkeley) 1966. Work rules and productivity in the Pacific Coast longshore industry.

WILLIAM D. HERMANN, Ph.D. Washington (St. Louis) 1967. Adjustments to shortage occupations.

Research encompasses the adjustments made by hospitals in St. Louis and Chicago towards overcoming two types of labor shortages, those for licensed practical nurses and certified medical technologists. It was found that in the Chicago labor market, adjustments such as the increase in training positions, job dilution with nurses aid and substantial increases in salary, enabled the Chicago hospitals to function without apparent LPN shortages. In St. Louis, hospitals were unwilling to make the above adjustments to as great a degree, while probably because of the very high proportion of Negroes on the waiting lists, the number of training posts was not increased. Thus, the St. Louis hospitals were unable to adjust to the LPN shortage which increased in intensity during the period of study. For medical technologists, the shortage and labor markets are nationwide and very little could be done in the local labor market area to rectify the shortage.

JOSEPH A. HILL, Ph.D. Florida 1966. The economic implications for management of the Manpower Development and Training Act of 1962.

The impact of the MDTA of 1962, as amended in 1963 and 1965, has been shown to be both direct and indirect. Indirectly, the MDTA will alter the general business environment, modifying the market projections of the firm; directly, it will change the

supply and demand of labor. The MDTA will appease some of labor's demands and augment others. The MDTA will also increase the supply of skilled workers. The provisions and operations of the act are examined, as well as causes and cures of unemployment. Different types of programs, i.e., institutional vs. OJT, are compared and results evaluated. The recent shift in emphasis from providing skilled workers to training the especially disadvantaged is reviewed.

WILLIAM A. HOWARD, Ph.D. Cornell 1966. Money wage changes in U.S. manufacturing industry: a statistical model based on an analysis of the variables involved in the Eckstein-Wilson wage determination model.

CHONG C. HU, Ph.D. Saint Louis 1967. Interstate differences in insured unemployment and their implications for a recession period extended benefit program.

The two approaches to unemployment compensation extended benefits are examined—the Federal approach to begin and end payments at the same time in all states according to the condition of the economy and the state approach permitting each state to determine eligibility and qualification. The approaches are examined in thirteen different states to determine what results would have been in the 1957-58 and 1960-61 recessions.

NORMAN S. HUBBARD, Ph.D. Yale 1967. Short-run changes in labor productivity in United States manufacturing, 1954-59.

GORDON C. INSKEEP, Ph.D. Columbia 1967. An analysis of productivity and turnover among women garment workers as related to criteria used in employee selection.

GEORGE E. JOHNSON, Ph.D. California (Berkeley) 1966. The theory of labor market equilibrium.

THOMAS M. JOHNSON, Ph.D. Columbia 1967. Some aspects of the economics of minimum wages.

The theory of adverse employment effects from the minimum wage is restated and previous empirical studies are criticized. New measurements of effects on average hourly earnings are developed by use of BLS industry statistics. Adverse effects on employment are shown by observing trend changes at times of minimum wage increases, and by regressions using ratio of minimum wage to average hourly earnings. The appropriateness of the minimum wage as an anti-poverty policy is considered.

ROBERT E. JONES, Ph.D. Colorado 1966. A resource allocation model of man-hour and work load relationships in a non-market setting.

GEORGE F. LEAMING, Ph.D. Arizona (Tucson) 1967. Changes in the composition of the labor force in the primary copper industry.

The dissertation analyzes in detail changes in the relative pay structure, skill level composition, and type of work performed by the work force in the primary copper industry of the United States from 1947 to 1963. The separate analyses find significant positive trends in relative pay and skill levels and in the proportion of workers operating and maintaining capital equipment but not in the proportion of maintenance workers as compared to production workers.

ALLEN J. LENZ, Ph.D. Stanford 1966. Military retirement and income maximization: an examination of the economic incentives to extended military service.

MILDRED B. LEVY, Ph.D. Northwestern 1966. Interregional labor migration in Ghana. Interregional labor migration in Ghana is estimated in an econometric model which includes income, distance, education, urbanization and population size as explanatory variables. Migration data are from the 1960 Census. Rural to urban migration is examined within the context of the regression model and on the basis of other data.

DAVID B. LIPSKY, Ph.D. Mass. Inst. Technology 1967. The labor market experience of workers displaced and relocated by plant shutdowns: the General Foods case.

HAROLD I. LUNDE, Ph.D. Minnesota 1966. The prediction of occupational labor demand in the United States.

GRAEME MCKECHNIE, Ph.D. Wisconsin 1966. Retraining and geographic mobility: an evaluation.

This dissertation focuses on two methods of alleviating unemployment caused by

structural changes—retraining and relocation. One of the hypotheses tested was that retrained and relocated workers would have more success in the labor market in terms of increased earnings than workers who did not retrain or relocate. A major conclusion reached from data collected in West Virginia was that retrained men who relocated enjoyed a higher rate of return on the cost-investment than groups who did not combine retraining and relocation.

JOHN H. NORTON, Ph.D. Pennsylvania 1966. Studies in the measurement, analysis and projection of metropolitan area employment and unemployment.

The dissertation consists of a series of related studies of the uses of and requirements for data in metropolitan area manpower analysis, the nature and quality of the available statistics, and the problems encountered when small-area manpower data are employed in the construction of economic models. Emphasis is placed on economic base and labor market analyses, on models and methods of forecasting, and on procedures for improved inference.

DENNIS J. O'CONNOR, Ph.D. New School 1967. Fixed employment cost and wage rigidity.

The fixed employment cost hypothesis is offered as an explanation of wage rigidity. The hypothesis asserts that when the firm invests in employees, it is encouraged to maintain wages in order to minimize labor turnover. The hypothesis was tested in a number of ways and the results suggest that wage rigidity is partially explained by the hypothesis. It is concluded that profit maximizing behavior contributes to wage rigidity.

NEIL A. PALOMBA, Ph.D. Minnesota 1966. A measure of the stabilizing effect of the unemployment compensation program—with emphasis on the experience rating controversy.

The author examines the unemployment compensation program literature with special emphasis on the experience rating controversy. Then the author devises a Variance Measure, which can be altered to use different estimates of the multiplier, to test the stabilizing-destabilizing argument concerning experience rating. The measure reveals that experience rating has not been destabilizing during 1948-I to 1964-IV, but it also reveals that a non-experience rating tax rate would have achieved the same stabilizing results.

MICHAEL J. PIORE, Ph.D. Harvard 1967. Technological change and structural adjustment in the labor market.

The study investigated the manner in which the relative costs of different types of blue-collar workers influence the selection of new technology in the manufacturing sector. The investigation was conducted through interviews with managerial officials in 17 plants. The findings imply that relative costs of different types of labor are largely ignored in selecting technology. Structural bottlenecks, however, appear to be avoided through the process of on-the-job training. Several other implications of this process for labor market analysis are explored.

ROBERT PIRON, Ph.D. Northwestern 1966. Two essays on human capital and its relation to changing patterns of employment and technological change.

DENNIS F. RAY, Ph.D. Florida 1966. Automation, unemployment, retraining and a theory of retraining effectiveness.

The thesis of this study asserted that retraining programs would be considerably more effective if they were conducted prior to the unemployment of the individual. Based upon the findings presented in the study and the criterion of "retraining completions," the writer found that the experience of both public and private retraining programs supported the thesis. It was therefore concluded that the hypothesis was correct.

JAMES ROBINSON, Ph.D. Duke 1967. Labor education in Britain and the United States.

JAMES R. ROCHA, JR., Ph.D. Iowa 1966. The different impact of an urban labor market upon the mobility of white and Negro potentially skilled workers.

PAUL D. ROMAN, Ph.D. Saint Louis 1967. An analysis of the effectiveness of the reporting and disclosure provisions of Sections 202 and 203 of the Labor-Management Reporting and Disclosure Act of 1959.

The purpose of this study is to evaluate, in the light of congressional intent, the effec-

tiveness of the reporting and disclosure provisions contained in Sections 202 and 203 of the Labor-Management Reporting and Disclosure Act of 1959. The LMRDA evolved in direct response to the public concern caused by the disclosures of the Senate Select Committee on Improper Activities in the labor or management field.

BEVERLY K. SCHAEFFER, Ph.D. Duke 1967. Supplementary unemployment benefit plans: a case study and evaluation.

STUART O. SCHWEITZER, Ph.D. California (Berkeley) 1966. The relative importance of personal factors affecting re-employment.

MURRAY A. TUCKER, Ph.D. Pittsburgh 1966. The effects of three unions on wage and employment structure.

FRANK E. WAGNER, Ph.D. Syracuse 1966. Female employment in developing countries.

RODNEY F. WEIHER, Ph.D. Virginia 1966. The union as a monopoly: an empirical view.

JEFFREY H. WEISS, Ph.D. Harvard 1967. The changing job structure of health manpower.

Previous studies dealing with health manpower shortages have focused on specific health care jobs. As such, they have tended to ignore questions pertaining to the utilization of health care personnel. In this study, a job classification scheme for health care jobs was developed. Changes in the structure of employment among health care jobs were analyzed. The technological and market factors underlying changes in the structure of employment in several "health care job families" were explored in detail.

THOMAS D. WILLIAMS, Ph.D. Mass. Inst. Technology 1967. Labor and education in Ghana.

JACK C. WIMBERLY, Ph.D. Louisiana (Baton Rouge) 1967. Private industrial pension plans in the United States: a comparative analysis.

This study is a comparative analysis of pension plans in the automobile, steel, rubber, and petroleum refining industries. The analysis is based upon detailed examination of pension plan provisions as they appear in current pension agreements. Pension benefits for normal, early, special early, and disability retirement are computed with several hypothetical age, service, and earnings combinations. Special attention is given to the problems associated with vesting provisions as they appear in contemporary pension plans.

JOHN C. WINFREY, Ph.D. Duke 1965. The appropriate bargaining unit decisions of the National Labor Relations Board.

Population; Welfare Programs; Consumer Economics

ERNEST J. BARTELL, Ph.D. Princeton 1967. Catholic elementary and secondary education: a study of costs and benefits in selected areas.

This study examines questions of efficiency and equity raised by operation of Roman Catholic elementary and secondary schools in the dioceses of Youngstown, Ohio and San Francisco, California. Resource costs of school operation were measured by adding to accounting costs imputed values of services contributed by Church teachers and property. Allocation of contributed services of teachers was examined according to economic criteria. Returns to Church investment in religious teachers were measured, and incidence of school revenues was examined.

EILEEN M. BEYDA, D.B.A. Indiana 1967. Employee economic education programs by business firms.

BARRY R. CHISWICK, Ph.D. Columbia 1967. Human capital and the distribution of personal income.

A model is developed to facilitate the study of the effect of human capital on the inequality in, and the skewness of, the distribution of personal income across regions. Due to data limitations, the empirical analyses are for years of schooling. The analyses are performed for adult males in the states and regions of the United States (white and all), Canada, Great Britain, Israel, Mexico, the Netherlands and Puerto Rico.

ARTHUR J. CORAZZINI, Ph.D. Princeton 1967. Vocational education: an analysis of costs and benefits (a case study of Worcester, Massachusetts).

The goal of this study is to measure the economic benefits of the vocational-technical school to the individual graduate and to the local community and to compare these benefits with the economic costs of maintaining the school. To this end, current and capital direct and implicit costs of a community high school system are calculated and compared with direct income benefits and external social benefits.

RAYMOND S. FRANKLIN, Ph.D. California (Berkeley) 1966. The relative economic status of the Negro male: an econometric study.

CHARLES S. FRIEDMAN, Ph.D. Columbia 1967. The size, value and distribution of postwar automobile stocks in the United States.

This study presents newly developed estimates of size and/or value of automobile stocks in the United States on a national basis for 1946-1964, by states for 1962 and by households for 1960 and 1964. The household estimates are based on samples of more than 15,000 observations. Regressions are used to relate the alternative measures of per capita auto stocks by state and probabilities of auto ownership by households to economic, social and demographic explanatory variables.

WALTER P. HETTICH, Ph.D. Yale 1967. Equalization grants, minimum standards and unit cost differences in education.

MELVIN E. HORTON, Ph.D. Washington 1966. An economic analysis of progress in the medical care of the United States Navy and Marine Corps personnel.

In this study the output of the medical industry is measured in terms of the value of man-days saved from disease due to medical effort. Output series for the last three decades are constructed for the U.S. Navy. When compared with the corresponding inputs, a rate of productivity advance of 5 to 7 per cent per year is obtained. This high rate indicates that this service industry differs from manufacturing not in its proneness to progress, but in the ability of the economist to appropriately evaluate the rate of progress.

CASWELL L. JOHNSON, Ph.D. Columbia 1967. The structure of immigration and the labour force: an enquiry into the economic characteristics of Canada's postwar immigration 1946-1962.

The study explores in detail the hypothesis that the economic characteristics of the immigrant population and the pattern of postwar population movement to Canada indicate that immigration in Canada is effectively utilized to supplement and complement manpower needs in the national economy at various stages of the country's industrial transformation along lines consistent with a "classical selectivity" policy or limited classical internationalism. The scope of the study covers the employment, demographic, educational, occupational and mobility aspects of immigrant relative to native labour force characteristics.

NANDINI U. JOSHI, Ph.D. Harvard 1967. The efficient allocation of resources in higher education.

The planning model for higher education, involving input-output analysis, deals with optimum investment requirements for different kinds of skilled personnel in different sectors of the economy, considering choices among educational as well as production technologies. The input-output structure of labor force, factors affecting it, shortages and surpluses of skilled personnel arising from its disequilibrium, and its educational implications are analyzed. The model is applied to India.

MARTIN T. KATZMAN, Ph.D. Yale 1967. Distribution and production in a big city elementary school system.

DEMETRIOS KOULOURIANOS, Ph.D. California (Berkeley) 1967. Educational planning for economic growth.

RICHARD A. MALT, Ph.D. Princeton 1967. Population and the fisc: the effect of demographic change on public expenditures and revenues.

This thesis explores the proposition that demographic change does significantly affect some components of public expenditures and revenues. Theoretically, an approach to age-selective expenditures is set out. Empirically, fiscs and population of the United States are chosen for study. Changes in age composition are found to significantly affect expenditures on public education and welfare, and revenue lost through age-selective exemp-

tion provisions of the U.S. personal income tax. Demographic variables are found useful in regressions of state and local expenditures.

ALEXANDER R. MAURIZI, Ph.D. Stanford 1967. Economics of the dental profession.

We have endeavored to use some of the tools of economic analysis to understand the dental profession in this country. We have examined internal rates of return to training, explored inter-state relocation patterns of established dentists, examined the slope of the short-run supply curve of labor, and constructed a Cobb-Douglas production function for the dental service industry. This application of economic reasoning proved to be rather fruitful.

JOHN R. MCKEAN, Ph.D. Washington 1967. Regional disaggregation of rates of return to educational investment.

This dissertation extends the previous studies on the rate of return from investment in formal education in three directions. First, the author estimates the rates of return from the most recent data compiled in the 1960 Population Census. Second, he provides a more detailed analysis of the costs of education, thereby allowing for further disaggregation in the profile of rates of return. Finally, he estimates the rates of return for each of the four census regions as well as a separate estimate of the rate of return to education for non-whites in the South.

THOMAS W. MERRICK, Ph.D. Pennsylvania 1967. Population redistribution in economic growth: an international comparison.

WILLIAM B. NEENAN, Ph.D. Michigan 1966. Normative evaluation of a public health program.

ARTHUR J. O'NEAL, JR., Ph.D. Rutgers 1967. The demand for vocational rehabilitation services: a statistical analysis.

An analysis of economic and demographic factors underlying the demand for public vocational rehabilitation services. Hypotheses that demand would be a decreasing function of community income levels and an increasing function of the rate of unemployment received support, and there was some evidence of an inverse relationship between demand and urbanization and a direct relationship between demand and labor force participation rates. Results for other variables tested were negative or inconclusive.

URBAN B. OZANNE, JR., D.B.A. Indiana 1967. Family decision-making processes and the degree of innovativeness among Scottish central-heating.

HAROLD R. PHILLIPS, Ph.D. Florida 1966. An investigation of some aspects of the prices and costs of hospital care.

THOMAS I. RIBICH, Ph.D. Princeton 1967. Education and poverty.

This dissertation attempts to measure and interpret the income gains resulting from the improved education of the poor. These gains are then compared with costs as a measure of program efficiency. Comparable measurements are derived for several varieties of education. For most of the changes examined it is estimated that the earnings gains are not as large as the dollar costs of making the changes, though some notable exceptions occur.

ENRIQUE A. ROLDAN, Ph.D. Minnesota 1966. Education and income distribution: an empirical study.

T. J. SAMUEL, Ph.D. Toronto 1965. Population control in India.

PINHAS SHWINGER, Ph.D. Pittsburgh 1966. The timing of births and the business cycle.

DOUGLAS WILSON, Ph.D. Syracuse 1967. The allocation of health resources in New York State with emphasis on the role of state agencies.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois 60201.

Vacancies

Economists, mathematicians, statisticians, operations analysts: Needed by the Research Analysis Corporation to participate in defense-related studies of resource allocation techniques and applications. Areas of interest include systems analysis of defense forces, weapon systems, and manpower and material resources, and its constituent elements of cost-effectiveness analysis and military cost analysis. A strong capability is maintained in computer-assisted model building. Staff members are assisted in acquiring a working knowledge of computer techniques if they do not already have it. Candidates should have at least a master's degree. For additional information, send résumé to Mr. John G. Burke, Supervisor of Professional Staffing, Research Analysis Corporation, McLean, Virginia 22101.

Economist: September, 1967. Principles and advanced course to be selected. Twelve-hour load. Assistant or associate. Ph.D. required. Small college of about 2,000 enrollment where emphasis is placed on teaching. Write to: O. A. Grant, Head, Department of Social Science, Tarleton State College, Stephenville, Texas 76402.

Teaching opportunities abroad: University positions, instructor to professor level, in the developing nations of Asia, Latin America, and primarily Africa. Two-year contracts. Salary dependent on level and location; transportation provided. Write to: Overseas Educational Service, 522 Fifth Avenue, New York, New York 10036. An equal opportunity employer.

Accountants: With continued growth and expanding academic programs, the Accounting Department, Texas A. and M. University has openings for two additional staff members. One, an outstanding tax and auditing specialist with a capability in computers as related to accounting in general and auditing in particular. Applicants should possess terminal degree and preferably be in mid-career and interested in a balanced program of teaching, research, and publications. Salary to \$16,500 for nine months with opportunity for additional summer term teaching at a comparable rate. Second, an assistant professor interested in principles and theory, cost, and computer applications in accounting. Applicant should possess terminal degree. Salary to \$10,750 for nine months with opportunity for additional summer term teaching. Applicants for both positions may send credentials to: Dr. Jack W. Coleman, Head, Accounting Department, School of Business, Texas A. and M. University, College Station, Texas 77843.

Transportation economist or transportation specialist: U.S. Army Corps of Engineers, Civil Works Directorate, requires several transportation economics or transportation specialists to fill staff positions in division and district offices in various parts of the country. Positions are from grade level GS-5 (\$5,331) to GS-13 (\$12,873) and are a part of the federal Civil Service program. Basic requirements are a degree in economics or business administration and appropriate experience or training for performing the following duties: supervises or produces economic evaluation studies of proposed waterway or harbor improvements; structures potential area of influence of proposed improvement, determines movements of commodities in area of influence, and assesses economic efficiencies of moving these commodities by various carrier modes—water, rail, road, and pipeline. Should be familiar with cost and rate structures of these carrier modes as well as evolving technologic changes affecting their interrelationships in an overall transportation system for a region and for the entire nation. Applicants should apply to: Mr. N. A. Back, Chief, Economics and Evaluation Branch, Civil Works Directorate, Office, Chief of Engineers, Washington, D.C. 20315.

Economic statistician: Under direction of chief, Research and Analysis Division, conducts research and prepares analyses of economic conditions and trends and statistical indicators relevant to the planning and development programs of the state. Requires master's degree in economics or economic statistics and four years of progressively responsible experience in economic research involving collecting, manipulating, and analyzing economic data. Salary range, \$9,840-\$12,552 per year. File applications with: State of Hawaii, Department of Planning and Economic Development, 426 Queen Street, Honolulu, Hawaii 96813.

Economist I: Under direction of chief, Research and Analysis Division, conducts research and prepares analyses and reports on macro- and microeconomic conditions relevant to the planning and development programs of the state. Requires four years of responsible professional experience involving work in economics and a master's degree in economics or business administration. Specialization in regional economics, econometrics, or statistics helpful. Salary range, \$9,840-\$12,552 per year. File applications with: State of Hawaii, Department of Planning and Economic Development, 426 Queen Street, Honolulu, Hawaii 96813.

Economics: Assistant dean, College of Business, Ph.D. or D.B.A. in business or economics required. Three to 5 years of full-time teaching experience in college or university. Administrative experience given special consideration. Limited teaching opportunity. Initial annual salary \$17,000-\$18,000. Effective date September 1, 1967. Contact: Dean Earl A. Roth, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Economists: The state of Virginia has recently established a Division of Planning with broad research responsibilities. Opportunities are open for original work in state and local finances, regional economics, and operations research. The work is particularly challenging for young men wishing to gain research experience in an organization with few bureaucratic restraints and good facilities and financing. Positions are open for persons with training in economics at the M.A., near-Ph.D., or Ph.D. level. Salary range fairly close to federal GS-9 through GS-12. For additional information, send résumé to: Chief, Research Section, Division of Planning, 805 State Office Building, Richmond, Virginia 23219.

Economist: For one- or two-year teaching assignment in economics and/or business administration at one of the most progressing international universities. All lectures are in English. Ph.D. or D.B.A. and some teaching experience are required. Salary depends on qualifications, present rank, and experiences. The position may be permanent for Iranian citizens who meet the above qualifications. Contact: Dr. Manuchehr Vessal, Provost, Pahlavi University, Shiraz, Iran.

International economist: Westinghouse Electric Corporation, advanced studies group. Located in Cambridge, Massachusetts, consisting of senior political scientists, systems and operations analysts and specialists in strategic intelligence, psychology and engineering who seek the services of an international economist. Duties are to provide economic inputs to international and corporate political and strategic studies conducted for federal agencies. Though studies are often broad, conceptual, or long range, conclusions regarding problems examined are usually stated in as specific and policy-oriented a manner as possible. Some knowledge of economic development

desirable. Typical contributions: projection of national capabilities and resource allocations for various countries; role of economic aid and military assistance in U.S. foreign policy; gross order of magnitude costing of required U.S. military forces and facilities; contribution of economic planning measures to political and economic modernization in a specific environment; and influence of blockade on a nation's economy and military capabilities. Master or doctorate with experience in mathematics. Security or strategic studies, international economics, comparative economics and economic development. Write to: R. H. Lence, Director, 545 Technology Square, Cambridge, Massachusetts.

Visiting professorship in China (Taiwan): Requirements are a graduate degree with ability to teach one or more of the following courses: business administration; electronic data processing; marketing, statistics, quantitative methods, economics and related fields. A four-month up to two-year contract can be arranged to suit individual preference. Round trip air expenses paid. Retired professors and businessmen are also welcomed. Write to: Professor Kenneth K. Fan, Utah State University, Logan, Utah 84321.

Foreign service: The government of Jamaica has vacant posts in the Economic and Commercial Branches of its Foreign Service. Graduates (citizens of Jamaica) in economics, commerce or international law are invited to apply. Send applications to: Permanent Mission of Jamaica to the United Nations, 235 East 42nd Street, New York, New York 10017.

Principal economist: The Aerospace Division of Westinghouse Electric Corporation has a unique opening for an economist specializing in the determination of the cost elements of cost/effectiveness analyses of advanced military and nonmilitary aerospace systems, and in the analysis of relationships between specific defense program budget requirements and overall DOD objectives. Objective: to establish, and recommend to management, actions which will maintain a proper relationship between company new business proposal actions and government fiscal budget cycle operation. Basic requirements are Ph.D. (preferred) or master's (minimum) in economics plus three to five years of experience in Department of Defense, preferably in the Comptroller's or Systems Analysis areas of OSD. Equivalent experience in the aerospace industry will be considered. Salary open. Write to: Mr. M. J. Frank, Westinghouse Electric Corporation, Defense and Space Center, Friendship International Airport, Box 746, Baltimore, Maryland 21203.

Economist: Firm of consultants in economics and statistics in Berkeley, California, is seeking an economist to help meet the demands of an expanding practice. This is a fulltime, career position with unusual potential in the largest practice of its kind in the United States. The work involves the application of econometric techniques to a broad variety of appraisal problems, including present value of life-time earning capacity of persons, future expenses of persons, future losses of businesses, and future income of property. The work requires the preparation of appraisals and reports and their presentation in court as an independent expert witness. The minimum requirement is a Ph.D. in economics, preferably labor economics, and a good working knowledge of statistics and the mathematics of finance. Starting salary depends on qualifications and experience. Send full résumé to: Philip Eden, 1303 Walnut Street, Berkeley, California 94709.

Economists: Positions available with TVA's Regional Studies Staff in Knoxville, Tennessee, for candidates interested in regional economic development and economic planning. Duties involved are: To assist in formulation of a regional growth simulation model in program evaluation particularly with respect to waste management systems; to test and evaluate results. To analyze problems in corporate planning of regional development programs; to assist in evaluating research problems submitted by line divisions; to analyze program functions and suggest procedures for evaluating program contributions to regional economic development. Candidates should have at least a master's degree. Salary \$9,000 to \$14,225. Send résumé or write to: Chief, Employment Branch (AER), Tennessee Valley Authority, Knoxville, Tennessee 37902.

Economists: The United Nations needs economists for challenging research and operational posts at Headquarters, New York, in Geneva, Vienna, and in many of the developing countries throughout the world. These posts are in various fields of specialization (development, econometrics, fiscal policy, foreign trade, international finance, industrial management, transportation, agriculture, mining, water resources, urban planning), at all levels of responsibility and of varying duration—permanent

or short term. Candidates with an M.A. or Ph.D. and experience in these fields are invited to contact for additional information: Office of International Organization Recruitment, Department of State, Washington, D.C. 20520.

Economists: Opportunity for participation in original and meaningful research in regional economic studies, industry studies, human resource analysis and state and local finance as part of a continuing state-wide planning function. Requirements include training in economics at the M.A., near Ph.D., or Ph.D. level. Positions can be filled at salary ranges of up to \$12,000, depending upon qualifications. Please send résumé to: James J. O'Donnell, Director, Maryland State Planning Department, 301 West Preston Street, Baltimore, Maryland 21201.

Economists: General Electric's Center for Advanced Studies has openings for economists who are interested in solving problems of government and industry. Preferred training and background will include experience in operations research and a Ph.D. in economics. Offices are in Santa Barbara, California, and Washington, D.C. Write to: Dr. Stephen Enke, P.O. Box QQ, Santa Barbara, California 93102.

Accounting and marketing: Applications are requested for a one-year appointment, effective September 1, 1968, with the possibility of appointment for a second year (1969-70), to replace a Department member on leave of absence. The teaching load of twelve hours each semester will consist of one section of principles of marketing and three sections of elementary accounting. Applicants with the master's degree and no experience will be considered for appointment at the rank of instructor. Further graduate work and experience may qualify an applicant for appointment at the rank of assistant professor. Write to: Dr. William F. Railing, Chairman, Department of Economics and Business Administration, Gettysburg College, Gettysburg, Pennsylvania 17325.

Economist: U.S. Army Corps of Engineers in Huntington, West Virginia, requires an economist in the Planning Branch. Positions are from grade level GS-5 (\$5,331-\$6,915) to GS-9 (\$7,696-\$10,045) and are part of the federal civil service program. Basic requirements are degree in economics or business administration, appropriate experience or training, and the ability to write well. The duties to be performed are: conduct and participate in the coordination of economic studies of river basins, apply professional techniques and principles of economics in formulating water resource investments, determine the potential effects of water resource investments upon the regional and national economy, develop data sources, develop and maintain contacts with specialists in the field. Limited travel and opportunity for additional education are available. Applicants for this position should apply to: Chief, Planning Branch, Huntington District, Corps of Engineers, P.O. Box 2127, Huntington, West Virginia 25721.

Economist: February, 1968. Ph.D. preferred; doctoral candidates and those with master's degree will be considered. Rank and salary dependent on advanced degree, business and/or teaching experience. Principles and advanced courses to be selected from money and banking, public finance, growth and development, labor economics, business and government and international economics. Twelve-hour teaching assignment. Three classes per semester; three or four courses per year. Liberal fringe benefits, including TIAA. Millikin University is coeducational with an enrollment of 1,400. It is affiliated with the United Presbyterian Church (U.S.A.). The school offers undergraduate majors in economics, accounting, marketing, and industrial engineering. Opportunities for teaching at additional compensation are present in both Evening School and Summer Session. Please send résumé and names and addresses of three references to: Dean James R. G. Olson, School of Business and Industrial Management, Millikin University, Decatur, Illinois 62522.

Economists: With continued growth and a new M.A. program in economics, the Department of Economics, California State College at Fullerton will have openings in September, 1968, for several additional staff members at the junior level, Ph.D. or near Ph.D. Although the particular areas of specialization in economics are open, we are interested in persons with strong fundamental grounding in economic theory. In general, the Department seeks persons with a high degree of technical economic competence, dedicated to teaching, and devoted to continuing professional activity and development. The College, 8 years old and rapidly expanding, is located on the fringe of the Los Angeles metropolitan area. For further details, write (enclosing detailed résumé): Dr. Levern F. Graves, Department of Economics, California State College, 800 North State College Boulevard, Fullerton, California 92631.

Labor economists and statisticians: Attorneys have discovered that expert economists and statisticians can provide valuable assistance in determining the extent of monetary losses suffered because of wrongful injury of death. Qualifications required to engage in this highly rewarding activity include faculty status of associate professor and above, an interest in human capital analysis, and located near a major city. Our economic consulting organization has several prime markets open. Write to: D. Carl Deiter, President, Economic and Statistical Consultants, Inc., Box 17184, Tampa, Florida 33612.

Economist, chairman of department: California State College at Fullerton is seeking a qualified economist to fill the position of chairman of its Department of Economics. The position requires a Ph.D. degree from a major university and evidence of scholarly writing, leadership, and administrative ability. The approximate salary range is \$11,000 to \$17,000, depending upon experience and qualifications. The Department of Economics is in the School of Business Administration and Economics. Major interest is directed toward the bachelor and master of arts programs in economics. In addition, the department offers undergraduate service courses for various majors in the college and provides graduate courses for the master of arts in social science and the master of business administration programs. The undergraduate program of the School of Business Administration and Economics is fully accredited by the American Association of Collegiate Schools of Business. California State College is a rapidly growing, eight-year old institution, located on the fringe of Los Angeles, 25 minutes from Newport Beach, an hour from the mountains, and within easy driving distance of the University of California at Irvine. The position of department chairman under growth conditions such as the college is experiencing, provides ample opportunity for innovation, dynamic leadership, and professional development. Please address inquiries to: Paul T. Kinney, Dean, School of Business Administration and Economics, California State College at Fullerton, Fullerton, California 92631.

International economist: The U.S. Arms Control and Disarmament Agency has a Career Civil Service position for an international economist, grade GS-12 (\$10,927) or GS-13 (\$12,873), to perform research on the economic impact of defense and disarmament. Nondiscrimination in employment. Basic requirements are for a degree in economics and a minimum of 3 years of professional experience which includes the specialized fields of designing, monitoring, and evaluating sponsored research projects and performing in-house research. Work involves considerable contact with the academic community, nonprofit and profit institutions, and economic policy officials of other agencies of government. Position provides opportunity for use of economic background in very broad scope. Call Mr. Henry Wyner, Area Code 202, DUDley 3-7755, for further information or mail SF-57, Application for Federal Employment, or résumé to: Economics Bureau, U.S. Arms Control and Disarmament Agency, Room 4666, State Department Building, Washington, D.C. 20451.

Economists: Office of International Operations, Internal Revenue Service, has unique and intellectually challenging career opportunities for economists in Washington, D.C. Nature of work is primarily concerned with applying economic analysis to intercorporate activities between related corporations to ascertain whether they reflect competitive arm's length standards. Basic function is to examine and analyze transactions involving goods and services flowing between related corporations to determine whether the intercorporate transfer prices meet the arm's length standard. Conclusions are then drawn as to whether the corporation has clearly reflected its true taxable income. These economic analyses cover the entire spectrum of business activity and will have an important bearing on the development of tax law guidelines and precedents involving intercorporate pricing in the expanding area of international commerce. Positions are available at entrance salaries ranging from \$10,000 to \$15,000. Position and starting salary will depend upon educational background and professional experience. For further details, write to: Director of International Operations, attention, Chief, Economic Advisory Branch, Internal Revenue Service, Washington, D.C. 20225.

Economists: January, 1968. Ph.D. preferred. Teaching positions open in the areas of micro economic theory, macro economic theory, public finance, comparative economic systems, business cycles, statistics, and principles. Rank and salary appropriate to education and teaching experience. Write to: Dr. J. C. Pogue, Acting Chairman, Department of Social Science, University of Missouri at Rolla, Rolla, Missouri 65401.

Economics and department head: Opening for Ph.D. in growing economics department. Teaching assignments open, but prefer experience in statistics and quantitative methods. College located ideally in rural community, but close to metropolitan centers. Available beginning spring term or fall term, 1968. Send résumé to: Robert N. Ford, Dean of Social Studies, Millersville State College, Millersville, Pennsylvania 17551.

Economics: February or September, 1968. Ph.D. preferred; doctoral candidates with master's degree will be considered. Teaching assignment flexible, including range from elementary to advanced undergraduate courses and/or graduate courses. Rank and salary commensurate with qualifications. T.I.A.A., life insurance, major medical, other fringe benefits. Please send résumé to: Dr. Charles L. Kraemer, Chairman, Department of Economics and Business Administration, Wagner College, Staten Island, New York 10301.

Economists and econometricians: Abt Associates, Inc., a small research firm in Cambridge, Massachusetts, desires to expand its staff of economists and econometricians. The company is young, vital and growing rapidly. The firm offers unlimited opportunities for independent and creative research in a wide range of areas, management of contracts and research studies, and contact with professionals on the staffs of numerous universities. Abt Associates is presently working with many branches of the U.S. government, private companies, foreign governments and international institutions. Present work includes economic development and planning, transportation, economics, economics of education, urban development, the industrialization of rural areas, and the design of information systems for regional development. Staff members are either social scientists or engineers and mathematicians, and many are both. Abt Associates emphasizes an inter-disciplinary approach to economic and social questions and seeks creative operational answers to today's urgent problems. Highly qualified, young and dynamic Ph.D. holders or doctoral candidates are required. Salary open and depends on qualifications. Applicants should apply to: Dr. Clark C. Abt, President, Abt Associates, Inc., 55 Wheeler Street, Cambridge, Massachusetts 02138.

Dean: Dean of the College, spring, 1963. Community college in its tenth year needs replacement for retiring dean. Teaching experience at college level required. Administrative experience desired. Doctor's degree strongly preferred. The College has become the focal point of a nationally unique and significant health education complex. Communicate with: Dr. Moses S. Koch, President, Essex Community College, Baltimore, Maryland 21221. Area 301, Mu 6-1200.

Accounting: Two assistant or associate professorships in accounting. Rapidly expanding department located in state university in Greater Detroit Metropolitan Area. Ph.D. or D.B.A. with specialization in accounting theory. C.P.A. given special consideration. Salary \$11,000 and up. Summer teaching and consulting available. Contact: Dr. Emerson C. Erb, Jr., Head, Department of Accounting and Finance, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Finance: Assistant or associate professorship in finance and banking. Rapidly expanding department located in state university in Greater Detroit Metropolitan Area. Ph.D. or D.B.A. with specialization in capital structure, capital investment and financial theory of the firm. Salary \$11,000 and up. February or September, 1968. Summer teaching and consulting available. Contact: Dr. Emerson C. Erb, Jr., Head, Department of Accounting and Finance, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Business education: Instructor with ability to teach courses in office education. Must have master's degree. Man with plans for higher education preferred, but all applications are invited. Rank and salary open. September, 1968. Contact: Dr. Charles H. Duncan, Head, Department of Business Education, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Business economics: Associate professor to teach courses in managerial economics, business conditions, and related areas at the graduate level. Ph.D. or D.B.A. in economics and/or business administration. College teaching experience preferred. Salary \$11,000-\$13,000, depending on qualifications and experience. Additional stipend for teaching summer school. September, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Data processing: Assistant professor to teach undergraduate and graduate courses in data processing. Opportunity to teach at least one course in another area such as business statistics or introduction to business. Ph.D., D.B.A., or M.B.A. plus. Business experience in data processing required. Salary \$10,600-\$12,000, depending on qualifications and experience. Additional stipend for teaching summer school. September, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Insurance: Assistant or associate professor to teach undergraduate and graduate courses in insurance. Ph.D. or D.B.A. in insurance and related fields. College teaching and business experience in insurance desirable. Salary \$11,000-\$13,000, depending on qualifications and experience. Additional stipend for teaching summer school. September, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Introduction to business: Assistant professor to teach introductory course in business at freshman level. Ph.D., D.B.A., or M.B.A. plus in business administration, with college experience teaching introduction to business. Opportunity to teach at least one course in area of specialization at undergraduate or graduate level. Salary \$10,000-\$12,000, depending on qualifications and experience. Additional stipend for summer teaching. September, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Research and statistics: Associate professor to teach graduate courses in business research, qualitative analysis, and statistics, and undergraduate course in statistics. Ph.D. or D.B.A. in business administration with work in research and quantitative areas. Teaching experience at graduate level desirable. Salary \$11,000-\$13,000, depending on qualifications and experience. Additional stipend for teaching summer school. September, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, Eastern Michigan University, College of Business, Ypsilanti, Michigan 48197.

Management: Two assistant or associate professorships, one in industrial relations and one in management systems or production management, in rapidly expanding department. Greater Detroit Metropolitan Area. Salary \$11,000-plus for academic year. Summer teaching available. September, 1968. Contact: Dr. Claude I. Shell, Head, Department of Management, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Marketing: Assistant or associate professor. Teaching position in an expanding marketing department located in state university in the Greater Metropolitan Detroit Area. Ph.D. or D.B.A. in business or economics required. Training in behavioral sciences, international marketing and/or quantitative methods helpful. Undergraduate and graduate programs. Salary \$11,000 and up for academic year. February or September, 1968. Contact: Dr. Robert J. Williams, Head, Department of Marketing, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Economist (micro and macro), statisticians: A nonprofit economic research organization in Washington, D.C., is seeking several professional economists and statisticians to participate in a study of regional growth and regional planning. Economic studies will focus on labor force requirements and demographic characteristics of regional populations, on problems of industrial structure and location, on issues of public finance and fiscal policy, and on projecting economic activity. Salary is commensurate with education and experience.

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Economists Available for Positions

(Italics indicate fields of specialization)

International finance, public finance and fiscal policy, national income accounting, economic development, economics of welfare, regional economics: Man, 38, married; M.A., additional work towards Ph.D. Fellowships; languages; 9 years of progressive research experience; publications. Seeks research position. Willing to relocate. E1625

Money and banking, international finance, economic analysis, economic policy: Man, 44, married; Ph.D. Five years of university teaching experience; 15 years of economic research in business; 2 years as government economist; modest publication record, including two books. Seeks stimulating position in teaching or research. Available at end of 1967. E1649

Principles, labor, consumer comparative economics, history of economic thought: Woman, retired after 20 years of college teaching in one institution; German Dr. rer. pol., M.P.A. (U.S.). Seeks position with college in East on a year-to-year basis. Available in September, 1967. E1657

Economic theory, economic history, labor economics, money and banking: Man, 32, Indian; M.A., working towards Ph.D. at Calcutta University. Trained in labor welfare; 7 years of graduate and undergraduate teaching experience; several publications. Desires teaching or research position. Available in September, 1967. E1658

Managerial economics, corporation finance, economic and financial analysis: Man, 33; B.S., Columbia, M.B.A., Ph.D. candidate. President of textile manufacturing company, former New York banker; several languages; experience in research for publication; teaching experience as army weapons instructor and platoon leader; tutor in economics. Desires teaching position in New York City area, part time or full time, undergraduate business and economics. Available on reasonable notice. E1659

Industrial relations and personnel management, economic planning, economic history: Man, 32, married; M.A. Assistant director of compensation service; 7 years of federal civil service; 3 years of teaching experience. Many articles in foreign field; fluency in Italian and other Romance languages; wide contacts, including business executives, labor representatives, and federal officials. Desires writing or college teaching position. E1666

Economic policy, agricultural policy, economic development, agricultural economics, administration: Man, 53, married; U.S. citizen by birth; Ph.D. from major U.S. university. Eighteen years of undergraduate and graduate level teaching and research; 17 years as professor; 7 years as head of large department, plus other university administrative experience. Three years as head of a U.S. government agency with overseas posts and programs with relationships with international organizations. Also private consulting and military command, staff, and teaching experience. Able public speaker and writer. Currently employed on successful private project overseas with U.S. university affiliations. Prefers challenging administrative post, professorship, or consulting assignment. Willing and able to pioneer. Available in late 1967 or early 1968. E1667

Land and area development: Man; A.B., M.S., all college and professional honors. Nationally recognized; 35 years of high-level professional experience. Independent consulting economist in Washington, D.C., with own office for 25 years. Over 300 studies made in most economic fields, both in U.S. and abroad. Knowledge of Spanish, Italian; some knowledge of French and German. Desires additional assignments on contract, per diem, or retainer basis. Résumé furnished on request. E1669

Labor economics, international labor, international economics and finance, comparative economic systems, law, international relations: Man, 37, married; B.S.F.S. (Diplomatic Corps), LL.B., M.A. (international economics-development). Over 5 years of experience with major labor union representing it in their political action program; 7 years with nonprofit and profit organizations in economic research and analysis. Desires challenging position in legislative area (D.C.) or overseas in development or labor. E1670

Economic principles, international trade and development, business administration: Man, 44, married; European A.B., M.A. Unusual experience overseas; dynamic and resourceful; fluent in several European and oriental languages. Seeks teaching or research position. E1672

Management, business law, business organization: Man, 28, married; B.S. (industrial administration), M.B.A. (marketing), J.D. University teaching and industrial experience. Desires teaching position with increased opportunity for growth. Opportunities in connection with doctoral study of particular interest. Available in September, 1968. E1674

Economics, marketing, international business administration: Man, 30, Indian; M.A., M.B.A. (economics), Indiana University, LL.B., Bombay University. About 8 years of experience in business. Desires research and/or teaching or administrative position with college, university, or research organization. Also interested in working for private business or international agency in the U.S. or Canada. Available in September, 1967. E1676

Microeconomics, economic development, international economics, money and banking, economic planning and public administration: Man, 27; B.A., M.A., Dr.rer.pol. in 1967 (West Germany). Doctoral research carried out in the pure theory of international trade with special reference to development problems. Desires research, academic, or teaching position in United States or Canada. Available on short notice. E1677

Economic principles, macro and micro theory, statistics, business literature, regional studies, management, marketing: Man, 30, married; B.B.A., M.B.A. (economics). Experience in business, research, teaching, including chairmanship of small department. Has publications and other recognitions of abilities. Now teaches 9-12 hours, 4 days per week as full-time faculty member. Seeks additional duties or courses in New York metropolitan area. Available in fall, 1967. E1679

Labor economics, management, principles of economics, money and banking, statistics: Man, 53, married; Ph.D. Eighteen years of college and university teaching experience, including 3 years as department chairman; 9 years of personnel work in industry and government. Midwest only; prefers Missouri or Illinois. Available in September, 1968. E1681

Economic theory, economic history: Man, 42; Ph.D., Phi Beta Kappa. Fulbright Scholar. Ten years of teaching macro- and microeconomics and economic history at graduate level and monetary theory and development at undergraduate level. Publications. Special interest in economics of health and education or welfare economics. Present salary \$11,000 with benefits, associate level. Prefers post with at least M.A. program. Ten years of work in federal government and Foreign Service. E1684

Economics, finance, accounting, information systems: Man, 40, married; M.A. (economics), Ph.D. (business administration). Five years of diversified teaching experience; 10 years of business and management experience in data processing, accounting, and financial planning and control. Currently employed in consulting. Desires research, consulting, administrative position with university or private firm. E1686

Macroeconomics, mathematical economics, comparative economic systems, Soviet economics, microeconomics: Man, 28; Ph.D. University teaching and research experience. Seeks academic position with time allowance for research or research position. Willing to go abroad. Available in June or September, 1968. E1687

Economic problems of underdeveloped areas, international public and business finance, investment and accounting: Man, 44, Indian (Goan), married, with Western education and background; B.S.C. (Econ.) from European university, Ph.D. dissertation in underdeveloped country economics in progress, preparing for final professional accountancy examination. Twenty years of extensive experience in senior financial position in national transport undertaking in East Africa, 8 of which as officer in charge; employed since May, 1965, as auditor in U.S. government agency; business experience includes administrative as finance and accounts department head of moderate sized undertaking. Paper on population growth in underdeveloped country published. Extensive travel in Europe, Asia, and Africa. Excellent references. Seeks research, operational or administrative position with research organization, financial institution or business undertaking in U.S., Canada, or Europe. Prefers position offering creative challenge. Available in September, 1968. E1688

Micro- and macroeconomics, economic development, history of economic thought, international economics: Man, 55; Ph.D. U.S. government and international agency experience; two-time Fulbright to Asia plus research fellowship in Asia; professor and department chairman. Publications. Desires relocation. E1689

Principles, price theory, international economics, econometrics, economic development, economic doctrines, money and banking: Man, 33, married, Indian with permanent resident status; M.A., M.Litt., M.S., and Ph.D. from Purdue University; prize winner. Several papers; presently associate professor. Four years of graduate and

undergraduate teaching experience; 6 years of research experience; widely traveled. Seeks teaching, research, administrative, consulting position with universities, colleges or international agencies, preferably in Northeast, West, or Midwest. Available on short notice. E1690

Industrial organization, public finance, economic thought: Man, 45, married; Ph.D. Twenty years of college teaching experience at excellent liberal arts college; departmental chairman; extensive academic experience in course development, including honors programs; several postdoctoral grants and research experience; modest publications record. Prefers permanent position in liberal arts college in Middlewest or Rocky Mountain area. E1691

Mathematical economics, mathematical programming and computer programming: Man, 23, Indian citizen; B.Sc., M.A. (Delhi). Was engaged in research work in mathematical economics in India; at present employed as a research fellow and lecturer in mathematical economics and computer programming at an African university. Has experience of working on computers, especially I.B.M. 1620, 1401, and 360 systems in statistical work. Seeks change preferably in industry as a programmer and analyst. Ready to work in any part of the world. E1692

Economics: Man, 33; M.A. Hons. (economic science), B.Com., B.Sc., A.C.C.S. Several years of teaching experience at various levels; chairman, department of commerce at a school. Desires teaching and/or research position at school or college level, preferably in a state with warm climate. Available any time at six-month notice. E1693

Principles of economics, theory of economic integration, planning and economic development in South East Asia, money, banking, public finance, statistics: Man, 28; M.A. (1st class). Six years of college and university teaching experience; presently professor and head of a postgraduate department of economics in India; research experience; various publications; books on regional economic integration, capital-output analysis, etc. Seeks teaching and/or research position anywhere in the world. E1694

Public finance, money and banking, macroeconomics, principles: Man, 24, married, Canadian; Honours B.A. in Economics and Political Science, M.Sc. (Econ.) from London School of Economics, Ph.D. thesis under way. Two summers with government and 2 years of teaching principles course. Seeks teaching position with research possibilities. Prefers Canada. Available in 1968. E1695

Macroeconomics, microeconomics, international trade, economics of underdeveloped countries, money and banking: Man 28, married, Canadian; M.A., Ph.D. candidacy examinations completed at major Canadian university, degree expected in November, 1968. Scholarships; 3 years as government economist; 1 year of university teaching as graduate teaching assistant. Seeks university teaching position in Canada with possibilities for research. Available July-September, 1968. E1696

Mathematical economics, econometrics, quantitative methods, micro- and macroeconomic theory, statistics, international economics, economic development and growth: Man, 34, married; M.S. (statistics), Ph.D. (economics). Graduate and undergraduate teaching and research experience. Publications. Willing to relocate. Prefers teaching position with graduate program. Also interested in research position in a leading research institution. E1697

Economic theory, money and banking, quantitative methods, marketing, labor: Man, 27, married; A.B. (economics), M.B.A. (economics and marketing), Ph.D. candidate at leading eastern university. Background includes college teaching and government and industrial research. Desires teaching position on East Coast. Available summer or fall, 1968. E1698

History of economic thought, European economy, comparative economic systems: Man, 30, single, German; Doctor in Economics. Desires university teaching position, especially in European economic history; also able to lecture in above fields; book, articles, reviews in quality journals. Currently Oxford Research Scholar. Six years of research and teaching experience. Geographic location open. Available in September, 1968. E1699

Economic development, monetary and fiscal policy, international trade and finance, macro and micro theory: Man, 40, married; Ph.D. in economics from Yale, 1957. Desires to join university faculty with time for writing and research as well as teaching. Ten years of experience in economic policy adviser and research roles plus 2 years of full-time and considerable additional part-time teaching experience; last 5 years with Agency for International Development; currently in charge of economics branch of one of largest A.I.D. overseas missions. Available in September, 1968.

E1700

International economics, economic development, economic history, economics of Asia, economic theory: Man, 36, married; B.A., M.S., Ph.D. Five years of college teaching experience; currently associate professor of economics; publications. Seeks teaching position with opportunity for research. Available in September, 1968.

E1701

International economics, principles of economics, history of economic thought, public finance, money and banking, economics of transport and public utilities, labor economics, Indian, American, and Soviet economic development, commercial geography: Man, 26, Indian, holding a permanent residence visa to the U.S. valid until November 21, 1967; M.A. (economics), Distinction, LL.B. First Class, both from Nagpur University, plus 2 semesters of graduate work at Institute of International Studies, Geneva. One year of teaching experience; 3 years in Reserve Bank of India. Seeks suitable post, teaching, research, business, or other in U.S. or Canada. Available immediately. P. L. Rao, C-4, 132 Rue de Lausanne, Geneva, Switzerland.

Business statistics, probability and statistical inference, regression and correlation analysis, business and economic mathematics, managerial and business economics, financial management, corporate finance, money and banking, international economics, economic principles: Man, 49, married; Ph.D. in economics from one of the top ten eastern schools. Twelve years of diversified teaching experience at the graduate and upper levels, last 9 years as an associate professor; other industry and government experience at the senior level in economics and statistics; member of various professional and honorary organizations; fluent in Spanish; presently associate professor in a large Midwest university. Desires graduate or undergraduate teaching position in megalopolis with preference to the New York area. Will consider top employment with research organization or the United Nations in the New York area, or with a governmental agency in New York City, Philadelphia, Washington, Baltimore, or Boston. Available in September, 1968.

E1702

Macro and micro theory, monetary economics, public finance, statistics: Man, 31, married; B.A., B.S., M.A., currently working on Ph.D. dissertation. Five years of college teaching experience. Research and writing experience with large commercial bank and federal government. Will consider teaching and/or research position in Southeast, including Washington, D.C. Available in January, 1968.

E1703

History of thought, micro, macro, economic development: Man, 28, married; B.S. (mathematics), 1961, M.S. (economics), 1963, Ph.D. (economics), 1966, from a big ten university. Good mathematical background; thesis contains mathematical model describing the Corn-Law Controversy. Good potential for future publications. Three years of college economics teaching experience; also experience in military systems analysis. Desires university teaching and/or research position in U.S. Available in April, 1968, or June, 1968.

E1704

General business, marketing, economics: Man, 31, Indian; M.B.A., Wayne State University, B.A. (Economics), Karnatak University, India. Five years of experience of which 2½ years in university research administration in U.S. and 2½ years as training officer in India. Desires research and/or teaching or administrative position with college, university, or research organization. Also interested in working for private business or international agency in the U.S. or Canada. Available in October, 1967.

E1705

Macro- and microeconomics, history of economic thought, economic development, international economics, comparative economic systems: Man, 37; Ph.D. Six years of teaching experience; 3 years of economic research; publications; grants. Presently with one of the leading eastern universities. Available in September, 1968.

E1706

Economic aspects of R and D, Soviet economy, economic development: Man, 34, single; B.A. plus 24 units of graduate work in economics at the University of California, Berkeley, M.A., University of Toronto, Ph.D., University of Cambridge. Seeks teaching or research position, preferably in his specialized field—economic aspects of R and D. Otherwise, in other two secondary fields. E1707

Statistics, quantitative business analysis, data processing, managerial economics, business and government: Man, 43, married; D.B.A., Indiana University. Seventeen years of teaching and administrative and research experience; currently on two-year contract with foundation as economic adviser in Middle East; formerly department chairman and full professor at major West Coast university. Can be interviewed at annual meeting. Available in fall, 1968. Richard W. Graves, P.O. Box 5008, Amman, Jordan.

Mathematical economics, microeconomics, quantitative methods, econometrics, operations research, welfare economics: Man, 29, single; B.A. (mathematics), M.S. (economics), Ph.D. (economics), 1964, from big ten university. Three and a half years of experience in research and teaching; publications in top journals. Desires teaching position as associate professor. East or West Coast location preferred. Available in summer, 1968, and possibly earlier. E1708

Statistics, econometrics, economics, operations research: Man, 26, married, French national; French B.S. in Mathematics (Sorbonne), French M.S. in Statistics and Operations Research (Ecole Nationale de la Statistique et de l'Administration Economique, Paris), M.A. in Economics, University of Southern California, Fulbright grant. Experience in teaching and research; good knowledge in European economies; languages; willing to relocate in the U.S. (American wife), preferably in New York, Washington, Chicago, or California, in the private business, universities, or research centers. Available October 1, 1967, in the U.S. E1709

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Interest Rates and Demand for Consumer Durables

M. J. Hamburger
Growth in Soviet Transport and Communications *N. M. Kaplan*
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MEASURES OF TECHNOLOGICAL PROGRESS IN THE LIGHT OF RECENT GROWTH THEORIES

By WILLIAM FELLNER¹

I. *Preview of Concepts, Symbols, and Propositions*²

The behavior of relative income shares ($wL/r\bar{K}$), profit rates (r), and of output-capital ratios (Q/\bar{K}), along the growth paths of Western economies has stimulated interest in a concept of dynamic equilibrium which the authoress of [22] named a Golden Age. This is a state of growth in *observable* Harrod-neutrality: a growth path of which it is possible to say not only that it is Harrod-neutral in the sense that Q/\bar{K} will remain constant *if* there takes place just enough investment to keep r constant, but also that there does in fact continuously accrue this particular amount of investment. In other words, in a Golden Age we actually have simultaneous constancy of r and of Q/\bar{K} , hence also of $wL/r\bar{K}$.

If the elasticity of substitution (σ) is unitary, then Hicks-neutrality of innovations—expressing itself in unchanging post-innovation values of the $wL/r\bar{K}$ ratio for the given pre-innovation macroeconomic inputs of L and K^2 —logically implies Harrod-neutrality in the sense that it takes the same amount of new investment to keep r constant as to keep Q/\bar{K} and thus also $wL/r\bar{K}$ constant. This is true only for $\sigma=1$ and Hicks-neutral innovations. But not even on these assumptions are we in a state of Golden Age—i.e., directly observable Harrod-neutrality on the growth path—*unless* the amount of new investment which actually takes place along with innovating activity keeps both r and Q/\bar{K} , hence also $wL/r\bar{K}$, constant in fact. If $\sigma < 1$, and if K/L is rising, then innovations need to be Hicks-labor-saving—they must reduce

¹ The author is professor of economics at Yale University. He expresses his sincere thanks to Craig E. Swan for valuable assistance in the work that led to this article, and he acknowledges his indebtedness also to his colleague Henry C. Wallich for very helpful suggestions.

² On the micro-level Hicks-neutrality means that individual firms, to which w and r are given, adopt innovations that leave the K/L ratio of each firm unchanged. Consequently, macroeconomic adoption leads to no change of $wL/r\bar{K}$ for the initial K/L ratio. (The symbol K relates to current capital input, while \bar{K} relates to the stock.)

$wL/r\bar{K}$ for the pre-innovation macroeconomic inputs of L and K^3 —to be consistent with Harrod-neutrality, since aside from innovation the actual amount of investment would raise $wL/r\bar{K}$.

The observed behavior of the relevant variables does indeed suggest that the concept of Golden Ages has significance, though the American economy should not be interpreted as actually having settled down in such a state. In various phases of development, the American economy, and possibly other economies too, may have been approaching Golden Age conditions from "different sides," that is, from states deviating from Golden Ages in different ways.

Until recently it was nearly universal practice to base interpretations of macroeconomic trends on Cobb-Douglas production functions (C-D) and on Hicks-neutrality of innovations (*as if* Hicks-neutrality, and perhaps even $\sigma=1$, followed from a convincing application of the Principle of Insufficient Reason!). Even on these assumptions it could be established that with a constant saving ratio (s), and a constant proportionate growth rate of the labor input (G_L), systems would tend toward a Golden Age, hence toward a constant G_Q and a constant G_K , where $G_Q=G_K$. More recently the authors of [24], [2] and many followers have made good use of linear-homogeneous production functions which have a *constant but not unitary*—usually less than unitary—elasticity of substitution (CES with $\sigma<1$). Given a constant s and a constant G_L , the Golden Age tendency can be established in this framework too, and here it is possible to build on the new and important concept of the innovation possibility frontier, and on the hypothesis of an *induced* Hicks-labor-saving bias by which distributive-shares equilibrium becomes established. Moreover, in the CES framework—in contrast to the C-D—use can be made of the concept of factor-augmenting innovations which leave the form of the production function unchanged *as if* simply more L and/or more K had been fed into the system. Such innovations may of course be said to raise the factor inputs *in efficiency units*, though it must be remembered that the productivity of each input depends not only on its own size in "efficiency units" but also on that of the complementary input. Relatively L -augmenting innovations—innovations that raise the L input in efficiency units in a higher proportion than the K input—are Hicks-labor-saving if, and only if, $\sigma<1$. In this case relatively K -augmenting innovations are Hicks-capital-saving.

³ On the micro-level this means that individual firms, to which w and r are given, adopt innovations that increase the K/L ratio of each firm. But macroeconomic adoption restores the initial K/L for the economy as a whole, with the result that $wL/r\bar{K}$ is reduced.

The present article will explain the meaning of an approach to Golden Age conditions "from one or the other of two sides"; it will make a case for interpreting American data as relating to an economy which is moving toward Golden Age conditions but, as a result of parametric shifts, is always at some distance (probably not a great distance) from these conditions; and the article will examine the appropriateness of alternative measures of technological progress for an economy moving toward a Golden Age. With respect to states of Golden Age, it will also be suggested that those economists who try to avoid the use of production functions involving "real" capital inputs (K) should be able to agree with the rest of us on the meaning of the distinction between output growth via technological progress and output growth via increased inputs (for this Golden Bridge see particularly V, 3 and 4, below). Our own analysis *will* make use of production functions with current K inputs, and it will be assumed that the long-run rate of growth of K inputs equals the rate of growth of the capital stock (\bar{K}). Along our trend lines these two growth rates will be considered equal even for very short, representative unit periods.

Throughout the paper we shall denote proportionate (instantaneous) rates of growth per unit of time by G with appropriate subscripts. On the other hand, subscripts under Q —i.e., under "output"—will express partial derivatives (e.g. $Q_K \equiv \partial Q / \partial K$). In the CES model which we shall formulate, E with a subscript—such as E_L and E_K —will express the augmentation terms by which L and K need to be multiplied to give us these inputs in efficiency units. This explains also symbols such as $G_{E_L} \equiv \dot{E}_L / E_L$, and $G_{E_{LL}} \equiv$ instantaneous growth rate of E_{LL} , etc. By w we shall always mean the *real* wage rate.

All items listed in the bibliography were found very helpful, though not all are referred to in the article itself. Still, it should be mentioned that our point of departure here will be provided by Samuelson's recent growth-theoretical contributions which their author rightly describes as extensions of the Kennedy-Weizsäcker line of approach [23]. The present writer's [9] was a product of the immediately preceding stage of development of this theory.⁴ We regard Chapter 6 of Hicks's [14] as the pioneering achievement, despite the fact that Hicks did not implement his fruitful conjecture by any discussion of mechanisms that could

⁴ In his [9] and earlier writings on the subject the present author was comparing merely two discrete points of what in the hands of Kennedy-v. Weizsäcker-Samuelson became a continuous innovation possibility frontier. In [9] we also assumed that the changes of variables which induce the L -saving or K -saving character of innovations (in Hicks's sense) were anticipated by the innovators but we showed awareness of the fact that past changes of the variables can also induce these biases.

"direct" the character of innovations. With good reason, Samuelson refers back to older authors too: in particular to Wicksell, but also to Marx and Ricardo.

II. *Golden-Age Tendency from the Left and from the Right*

For net output assume the dynamic *C-D* production function

$$(1) \quad Q(t) = e^{\lambda t} L(t)^{\alpha} K(t)^{1-\alpha}$$

The function involves unitary elasticity of substitution ($\sigma=1$), and it involves Hicks-neutrality of innovations that have a *constant* proportionate product-raising effect per period (λ). The exponents too remain constant ($0 < \alpha < 1$). Hicks-neutrality and $\sigma=1$ assure constancy of relative shares, even if both marginal products and both average products change. Furthermore, as a result of $\sigma=1$ and Hicks-neutrality, the assumed progress is also Harrod-neutral in the sense that the amount of investment which would keep the marginal product of capital (Q_K), and thus r , constant would also lead to an unchanging Q/\bar{K} . Directly observable Harrod-neutrality—i.e., Golden Age equilibrium with actually constant r and Q/\bar{K} —is not a *definitional* property, but can be derived from the assumptions underlying Figure 1 (essentially the same graph as is found in [13]). The demonstration assumes a constant s and a constant G_L , and it builds on two relations that follow from these assumptions and from (1). Remembering that K and L are current inputs while \bar{K} is a stock, and treating the right-hand side of (2) for very short periods as an "instantaneous" growth-rate, we obtain:

$$(2) \quad G_K = sQ/\bar{K}$$

$$(3) \quad G_Q = \lambda + \alpha G_L + (1 - \alpha)sQ/\bar{K}$$

Given constancy of λ , of G_L and of s , the intersection of G_K with G_Q must be stable, because G_Q has a positive intercept on the left and its slope falls short of that of G_K everywhere. The intersection marks a state of Golden Age with the following definitional properties: $G_K = G_Q$; hence Q/\bar{K} remains constant; also, in view of $\sigma=1$ and the assumed Hicks-neutrality, Q_K remains constant (note that here and in what follows $Q_K=r$). Approach to the Golden Age is asymptotic from either side, because the driving force toward the growth equilibrium is generated by a discrepancy between G_Q and G_K —a discrepancy which raises Q/\bar{K} if we come from the left and diminishes it if we come from the right—and because the fact that G_K is steeper than G_Q continually diminishes the discrepancy between these two growth rates with the result that the time rate of change of Q/\bar{K} also diminishes.

Starring all symbols that express Golden Age growth rates, we find

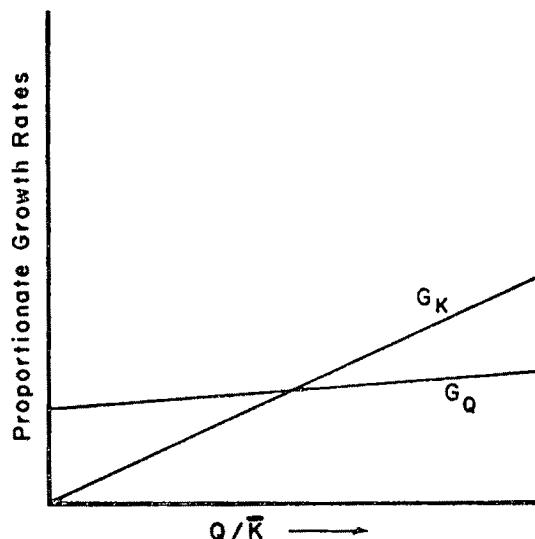


FIGURE 1. GROWTH RATE OF K AND Q IN THE EVENT OF C-D AND HICKS-NEUTRALITY.

(Income-distribution constant throughout the analysis)

Legend. "Realism" would require drawing G_K with a slope of about 0.10, and G_Q with a slope of about 0.025. For the sake of visual convenience the slopes were made larger in the graph.

At the origin, on the left-hand side, G_Q and G_K have the values shown in the graph; the value of Q/\bar{K} there should be interpreted not as zero but as negligibly small. Whether the process starts on the ordinate at the left or at the right-hand vertical line, we may think of it as being started by a sudden rise of s from zero to the value expressed by the slope of G_K . Just prior to the start G_Q had the value shown on the vertical line from which we take off. The G_Q intercept on the ordinate is $\lambda + \alpha G_L$.

$$(4) \quad G_Q^* = \frac{\lambda}{\alpha} + G_L^*$$

The Golden Age value of Q/\bar{K} is

$$(5) \quad (Q/\bar{K})^* = \frac{G_Q^*}{s}$$

Note that a change in s leaves G_Q unaltered, but that a transition from a Golden Age with a lower to one with a higher s will of course nevertheless increase Q in relation to the contemporaneous Q of the initial Golden Age.

The conventional model so far discussed—cf. equations (1) through (5)—has the disadvantage of *postulating* Hicks-neutrality, a conceptual construct the empirical importance of which cannot be made plausible

by any convincing application of the Principle of Insufficient Reason, because the underlying partitioning of the set of all possible innovations is highly unconvincing. The CES innovation-frontier model, to which we now turn—cf. equations (6) through (11) in the following pages and (12) in Section V—postulates neither Hicks- nor Harrod-neutrality. It derives a tendency toward Harrod-neutrality, coupled with Hicks-labor-saving bias, from a reasonable maximization postulate.

In the CES innovation-frontier model we assume $\sigma < 1$, which in the formulation appearing below implies $\rho < 0$, since $\sigma = 1/1 - \rho$. We shall assume also that innovations are of the factor-augmenting variety.⁵ Noting that all terms of (6) with the only exception of ρ are functions of time, we write the linear-homogeneous production function

$$(6) \quad Q = [(E_L L)^\rho + (E_K K)^\rho]^{1/\rho}$$

L and K are the inputs in natural units; $E_L L$ and $E_K K$ the inputs in efficiency units; $G_{E_L} \equiv \dot{E}_L/E_L$ is the (new or additional) L -augmentation per period; $G_{E_K} \equiv \dot{E}_K/E_K$ the corresponding term for K . Equation (6) can be written also as

$$(6a) \quad Q = \left[(E_L L)^\rho \left(1 + \left(\frac{E_K K}{E_L L} \right)^\rho \right) \right]^{1/\rho}$$

Equation (2) for G_K holds in this framework too in unmodified form. As for G_Q , we note that (6a) leads to

$$(7) \quad \log Q = \log E_L L + \frac{1}{\rho} \log \left[1 + \left(\frac{E_K K}{E_L L} \right)^\rho \right]$$

We use the notation $1 + (E_K K/E_L L)^\rho \equiv \gamma$, and we denote the proportionate rate of change per period of γ by the symbol G_γ . Then,

$$(8) \quad G_Q = G_{E_L L} + \frac{1}{\rho} G_\gamma$$

In any state of Golden Age that may be reached $G_\gamma = 0$. This is because *in efficiency units* the ratio of the two inputs must remain constant once the Golden Age path is reached, to keep $wL/r\bar{K}$ constant with $\sigma < 1$. Therefore the Golden Age growth rate may be written as

$$(9) \quad G_Q^* = G_{E_L L}^*$$

It follows that along a Golden Age path the instantaneous rate of increase of the real wage rate (w) equals $G_{E_L L}^*$. The last of these terms,

⁵ The most general form of factor-augmenting functions is $Q(t) = F[g(t) L(t), h(t) K(t)]$, where g and h are the augmentation terms applicable to L and K , respectively. The new (or additional) augmentation per period is $G_g \equiv \dot{g}/g$ for L , and $G_h \equiv \dot{h}/h$ for K .

which differs from the Golden Age growth rate G_Q^* only by the constant G_L , is the abscissa intercept of the innovation possibility frontier. This is why it was more revealing to carry out the operations leading from (6) through (6a) to (9) the way in which they were presented rather than to have them result in $G_Q^* = G_{EK}^* = G_K^*$, though these equations too are valid, and an obvious alternative to (6a) would have landed us in these.

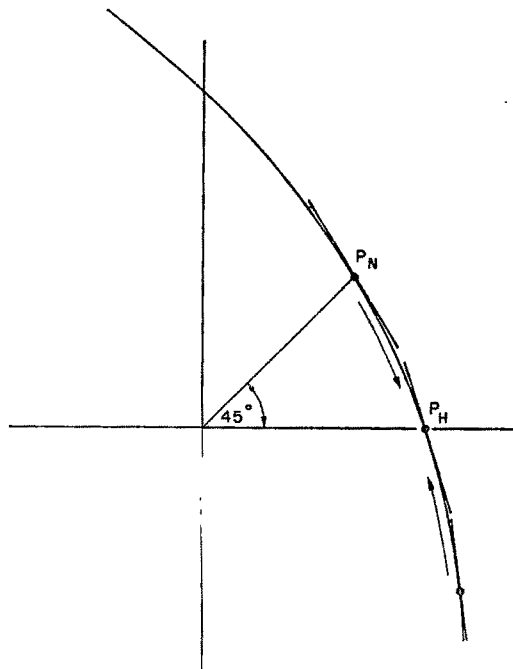


FIGURE 2. INNOVATION POSSIBILITY FRONTIER

Legend. Abscissa: Proportionate rate of new (or additional) L -augmentation per period (G_{EL}).

Ordinate: Proportionate rate of new (additional) K -augmentation per period (G_{EK}).

For this function: $G_{EK} = f(G_{EL})$, we assume $f' < 0, f'' < 0$.

For explanation of the frontier and of tangencies, see footnote 6.

As for the behavior of E_L and of E_K , given ρ , we rely on the theory of induced inventions, see [9] [1] [16] [23], and we make use of the very fruitful concept of the innovation possibility frontier. The apparatus is graphed in Figure 2, the relevant equation

$$(10) \quad G_{EK} = f(G_{EL})$$

and its significance are explained in the legend and in a footnote.⁶

⁶ The frontier implies a specific—presumably optimized—cost (or effort) of inducing *inventions* which can be put into effect as industrial *innovations*. The abscissa values of the frontier

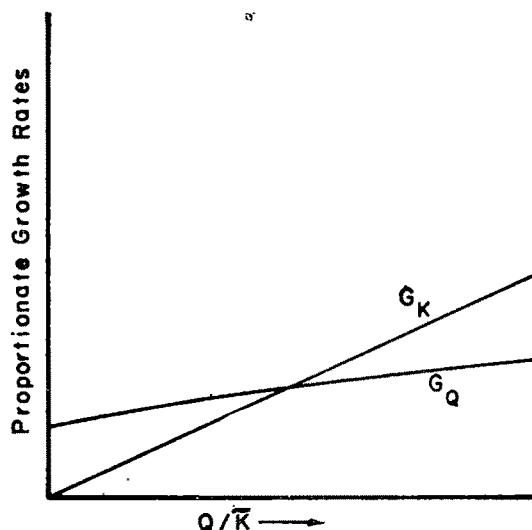


FIGURE 3. GROWTH RATE OF K AND Q IN CES INNOVATION-FRONTIER MODEL WITH FACTOR-AUGMENTATION

(Temporary shifts of G_Q associated with changes of income-distribution are disregarded in graph; intersection-point not shifting.)

Legend: Here, as in Figure 1, G_L and s are assumed to be constants. Maximization along innovation frontier is assumed.

The graph corresponding to Figure 1 may be drawn as in Figure 3.

The following considerations justify giving the G_Q curve in this model the shape it has in Figure 3. It may be seen from equation (8) that G_Q is uniquely determined by $G_{E_L L}$ if $E_K K / E_L L$ remains unchanged, regardless of the value of $E_K K / E_L L$ at which this ratio is stabilized. Thus, for any unchanging value of this ratio (for any unchanging income distribution), $(1/\rho)G_\gamma = 0$. It follows that if we draw G_Q in relation to Q/\bar{K} on the assumption $(1/\rho)G_\gamma = 0$, as we have done in Figure 3, then we do indeed obtain the correct G_Q point for the Golden Age intersection of G_Q with G_K , i.e., for $(Q/\bar{K})^*$. At this point $(1/\rho)G_\gamma = 0$ in actual fact. That such a point *exists* follows from the last paragraph of footnote 6, because there it was shown that the system tends to P_H in Figure 2, a point at which $E_K K / E_L L$ remains constant, and at which therefore income distribution too remains constant in actual fact (despite $\sigma < 1$). To P_H in Figure 2 there corresponds the intersection point with abscissa $(Q/\bar{K})^*$ in Figure 3.

To the left of this point in Figure 3, i.e., to the northwest of P_H in

show the maximum possible $G_{E_L L} = \dot{E}_L / E_L$ given any feasible value of $G_{E_K K} = \dot{E}_K / E_K$; this statement contains also a valid definition of the ordinates if in the statement $G_{E_K K}$ and $G_{E_L L}$ are interchanged. The numerical value of the frontier's slope is a marginal trade-off ratio, expressing the

Figure 2, we have a rising labor share, that is, a rising $E_K K/E_L L$ with $(1/\rho)G_\gamma > 0$;⁷ on the other side of Golden Age we have a declining labor share, that is, a declining $E_K K/E_L L$ with $(1/\rho)G_\gamma < 0$. Consequently, Figure 3, which neglects the $(1/\rho)G_\gamma$ term across the board, does not strictly speaking associate the "correct" values of G_Q with the Q/\bar{K} values, except that it does associate the "correct" G_Q value with the intersection abscissa $(Q/\bar{K})^*$. But once we take it for granted that, given $\sigma < 1$, all shares-equilibrium tangencies through which the system passes would be stable and viable for *some* given G_K (and thus for *some* given $G_{K/L}$),⁸ neglecting the $(1/\rho)G_\gamma$ term across the board merely means neglecting a term of transitory importance outside the intersection point in Figure 3, and of no importance at all at the intersection point itself. Differently expressed: even left and right of the intersection, Figure 3 associates with Q/\bar{K} those values of G_Q which correspond to the successive shares-equilibrium tangencies at the successive levels of $E_K K/E_L L$, though the G_Q curve of the figure does not show the temporary shifts

marginal opportunity cost (m.o.c.) of acquiring a higher G_{EL} , where m.o.c. is expressed in terms of the G_{EK} which is being sacrificed. Firms maximize profits by equating this m.o.c. with the marginal advantage of G_{EL} in terms of G_{EK} , i.e., by moving to the point of the frontier where the numerical value of its slope ($-f'$) equals that of the slope of a straight line such as any of the three lines in the graph. The numerical value of the slope of any such line is by *definition* equal to the ratio of factor costs, i.e., to $wL/r\bar{K}$. Thus maximization involves moving to a tangency position.

Given any $G_{K/L}$, hence given any G_K , the system settles down in distributive shares equilibrium, by the profit-maximizing tangency's becoming a *specific* tangency position. At this specific tangency point the factor-augmenting (factor-saving) properties of the innovations are so adjusted that shares will cease to change. As long as this *shares-equilibrium tangency* is not yet reached, the slope of the straight line will change because the joint effect of current changes in K/L and of the factor-saving properties of the innovations does not yet stabilize the shares. Stability of such shares-equilibrium tangencies requires $\sigma < 1$ in which case innovations with $G_{EL} > G_{EK}$ —i.e., "relatively L -augmenting" innovations—are Hicks-labor-saving, and vice versa. Only if $G_{K/L}$ remains unchanged—i.e., given a constant G_L , only if G_K remains unchanged—is the corresponding shares-equilibrium tangency *viable*. Changes in G_K alter $wL/r\bar{K}$ and they call forth further adjustments of the relationship between the augmentation terms (i.e., these changes deprive any previous tangency of its shares-equilibrium character).

Assuming a constant s as well as a constant G_L we arrive at the conclusion that G_K and $G_{K/L}$ will indeed go on changing until the Harrod-neutral Golden Age point P_H is reached. Only at this point is the shares-equilibrium tangency viable, because only at this point does that tangency involve $G_{EL} = G_K = G_Q$ (in view of $G_{EK} = 0$), and hence only at this point does Q/\bar{K} remain unchanged with the result that $sQ/\bar{K} = G_K$ also remains unchanged. At P_H income distribution is uniquely determined by the slope of the frontier on the abscissa, and G_{EL} by the abscissa intercept of the frontier. The point is Harrod-neutral but, given $\sigma < 1$, it is Hicks-labor-saving (indeed, *purely* L -augmenting). The Hicks-neutral point is P_N . Only if K/L remained unchanged, i.e., if $G_{K/L} = 0$, would the system settle down at P_N . All points southeast of P_N are Hicks-labor-saving; they become shares-equilibrium tangencies for positive values of $G_{K/L}$ (for increasingly high values of G_K and of $G_{K/L}$ as we move downward on the frontier).

⁷ Considering that $\rho < 0$ (a condition conveniently illustrated with $\rho = -1$), a rising $E_K K/E_L L$ means $G_\gamma < 0$, hence $(1/\rho)G_\gamma > 0$.

⁸ For this see the second paragraph of footnote 6; and for a detailed demonstration see [23].

of the G_Q points brought about during the Golden Age approach by movement of $E_K K/E_L L$ from one nonviable level to the next. This "distortion" would gradually disappear at any level of $E_K K/E_L L$ (at any level of income-distribution) if the system settled down at any one of these levels, since $G_Q = G_{E_L L}$ —i.e., $(1/\rho)G_\gamma = 0$ —unless $E_K K/E_L L$ changes. At the intersection point the "distortion" disappears in actual fact.

Let us now briefly trace the G_Q curve of Figure 3, remembering that it implies $(1/\rho)G_\gamma = 0$, $G_Q = G_{E_L L}$. Starting in Figure 2 from high up in the positive quadrant, i.e., in Figure 3 from the left, the intercept of G_Q in this last figure is located at a value of $G_{E_L L}$ which is not much greater than the constant G_L , since the region in question in Figure 3 corresponds in Figure 2 to the region with low G_{E_L} and high G_{E_K} . As we move to the right along the abscissa of Figure 3, and move down in the first quadrant of Figure 2, $G_{E_L L}$ is increasing for successive values of Q/\bar{K} because for these successive values the rate of new L augmentation per period (G_{E_L}) is increasing. The increase is proceeding at a decreasing rate for rising values of Q/\bar{K} , since the marginal tradeoff is changing in such a way that increasingly more of the adjustment of the mix of the two augmentation terms is being brought about by the reduction of G_{E_K} , and less by a further increase of G_{E_L} . This is how the behavior of G_{E_L} produces an increase of G_Q at a decreasing rate, as in Figure 3.

It is worth adding that while in the second of our two models the growth rate and the income distribution in Golden Age depend exclusively on the characteristics of the frontier—on its abscissa intercept and on its slope at that intercept—the size of the inputs, in efficiency units, which the system will generate to arrive at Golden Age, depends also on the value of ρ . This follows from

$$(11) \quad \frac{Q_L L}{Q_K K} = \frac{wL}{r\bar{K}} = \left(\frac{E_K K}{E_L L} \right)^{-\rho}$$

For any initial inputs, the distance at which the system *would be* from shares equilibrium *were it not for the augmentation process*, depends on the value of ρ . Hence the degrees of augmentation during the Golden Age approach also depend on this parameter.

We have described a Golden Age approach from the left. The graphs and equations show also the nature of the approach from the right. In Figure 2, an "approach from the left" is represented by a movement downward in the first quadrant, while an "approach from the right" is a movement upward from below, that is from the quadrant expressing negative K augmentation coupled with high L augmentation. K disaugmentation is not the strange phenomenon it sounds, because through complementarity the productivity of K benefits at any event from

L augmentation. Figure 2 suggests correctly, that given a sufficiently high labor share, the optimum innovations of various successive periods may involve negative K augmentation, though it is true also that for any K/L it is possible to specify on the frontier a maximum degree of negative K augmentation beyond which the "innovation" would be a "disimprovement."

In our graphs G_Q and G_K were plotted in relation to Q/\bar{K} , rather than to time. As was pointed out in the text, the movement is slowing in time, regardless of whether we approach the Golden Age point from the left or the right. The approach is asymptotic in either case. The constant rate of increase of G_K in Figures 1 and 3 is a slowing rate of increase *in time*, because the movement along the Q/\bar{K} axis is slowing as the discrepancy between G_Q and G_K is diminishing. Lagged dynamic models, for tracing time sequences in detail, will not be formulated in this paper.

III. *What is Hidden Behind the Simplifying Assumptions?*

The type of macroeconomic theory which we are using here has a number of semiarticulate implications about matters that remain in the background.

1. As concerns advanced Western economies, *land* is regarded as a factor for which capital, in the sense of reproducible tangible wealth, can be gradually substituted with sufficient ease to make a two-factor model applicable. Such a conception implies that the relevant trend in Q/\bar{K} is more "upward" than is the trend we observe when following the usual practice of regarding reproducible tangible wealth (say, other than consumer durables) as a measure of \bar{K} . This is because on any given level of technological-organizational knowledge the natural resources of a modern Western economy should presumably be viewed as fixed in quantity.

2. Both our models disregard *economies of scale*, since both our functions are linear-homogeneous. It does make sense to assume that the long-run trend of events is not dominated by those economies of scale which are observable on any given level of technological knowledge. Nevertheless, if the average size of firms is growing, then the unexploited economies of one period become partially exploited when, in the next period, firms move to lower cost curves. Thus, what we capture in the technological-progress terms of such models includes economies of scale.

3. In the C-D Hicks-neutral model, which does not distinguish between the augmentation of L and that of K , we capture in the progress term also increments of *human capital* per person. Furthermore, in the C-D Hicks-neutral model progress not merely *appears to be* disembodied but there is absolutely nothing in that model that could take account

of the fact that *technological advance requires new capital goods*. Fortunately, this is not quite true of the CES innovation-frontier model, since here we at least take into account that significant L -augmentation—the prevalent form of technological advance in a modern economy—requires a significant rate of increase of K/L .

4. The practice of interpreting w as a measure of Q_L , and r as a measure of Q_K , implies *purely competitive* conditions in the hiring (or acquisition) of inputs and in the sale of goods. Purely competitive supply of labor is not implied. If the deviations from pure competition do not change much over time, they will not mislead the economist making intertemporal comparisons. Yet even in this case the employment of distributive-shares weights to L and to K in production function analysis, which would call for elasticity weighting, must of course be taken with a grain of salt for any one period.

In my opinion monopsonistic imperfections deserve in this context more attention than they have received in the current discussion [8] [9] [10].⁹

5. As for *shifts in the product mix*, our models can give only crude indications concerning the presumptive direction of their effects. We may perhaps derive some such indications from an analysis of the consequences of letting the Q term of our models stand for a type of product calling for more K -intensive methods of production. By this we mean methods—production functions—which at the time of their adoption would raise r and would lower w for the pre-shift quantities of input, or, differently expressed, initially make an unchanging r compatible with a higher K/L and a lower Q/K . A once-and-for-all adjustment of the value of the coefficient, or of the coefficients, by which the input terms are multiplied in the production functions may also be involved in a change so conceived, and thus we may avoid making the change equivalent to technological progress. In the C-D system the conclusion emerges that such a move toward greater K intensity, that is, here

⁹ The theory of induced innovation has not only the merits discussed in II, since it may explain also why the kind of monopsonistic imperfection which develops in particularly tight factor markets tends to call forth offsetting forces. If for example acute shortages develop from the rapidly rising demand for a factor, this may lead to the narrowing of the markets in which the individual employer can do his hiring at roughly "given" input-prices and without much deterioration of quality. He may become aware of being faced with a rising supply curve for the input in question. The rising marginal cost of the factor will then intersect the marginal product curve of the same factor at a higher point of the latter, than would be the case in non-monopsonistic circumstances; and $\sigma < 1$ implies that under these conditions the cost of the input in question rises relative to the cost of the not-so-scarce factor, per unit of output. This will favor a wave of innovations that is particularly factor-saving in the scarcer factor. Even if the monopsonistic imperfection survives this wave, the system will gradually move back to the pre-monopsony point of the innovation frontier, but the factor-augmentation that takes place in the meantime has the effect of reducing the acute scarcity which created the imperfection, and thus of reducing, or perhaps even eliminating, the imperfection itself.

toward a reduced α and hence toward an increased K share in output, will in due time result in a *higher* actual Q/\bar{K} —i.e., will result in a *higher* $(Q/\bar{K})^*$ —though with a detour over *lower* temporary values of Q/\bar{K} . This long-run conclusion concerning the increase of $(Q/\bar{K})^*$ as a result of shifts toward greater K intensity does not impose itself on us in the CES innovation-frontier system. The reason why in the C-D framework we do arrive at this conclusion is that given a lower value for α the value of $G_Q^* = G_K^*$ increases, provided we are faced with merely a temporary “accommodating” change of s during the transition, but with a permanent change of α .¹⁰ For a given λ , equations (4) and (5) show this clearly, and the result ceases to be astonishing if we take into account that a higher K share means a higher K elasticity of output, thus a greater G_Q -stimulating effect of any given amount of investment. This long-run conclusion has no analogy in the CES innovation-frontier model, because here $G_Q^* = G_{E_L}^*$ which for a given G_L is uniquely determined by the abscissa intercept of the innovation frontier.

In the CES framework we may conceive of the result of a K -intensive shift as expressing itself in a reduction of the numerical value of the (negative) ρ exponent. We may conceive of the result of the contrary shift as expressing itself in a higher numerical value of ρ . While in the state of Golden Age the distribution of income will be the same as before, a shift toward greater K intensity (or greater L intensity) will *reduce (or increase) the L -share for the initial pre-shift macroeconomic inputs*, and the gradual return to the initial distribution will result from more (less) K augmentation relative to L augmentation on the way to the new Golden Age. This statement ties in with equation (11) in II, and with our discussion of the role of ρ in that same context. There it was explained that the ratio of efficiency inputs needed to make the relative shares consistent with the slope of the frontier at the Golden Age point depends on the numerical value of ρ .

If the intersector shifts were sufficiently sustained and intense, they could render the whole approach—its C-D version as well as its CES version—useless, because the tendency to move toward the Golden Age

¹⁰ To illustrate with a K -intensive C-D shift, it would be possible to describe the change on the assumption that s remains unchanged *even during the transition*, in which case the “greater K intensity” would express itself exclusively in a higher K share and in a rise of $(Q/\bar{K})^*$, but in no temporary decline of Q/\bar{K} and in no effect on the K/L ratio (because r/w would rise sufficiently to be consistent with this result). But if into our simplified framework we wish to carry over some of the essential facts of life, it is preferable to assume a temporary, accommodating change in s . Given the identical, constant equilibrium value of s for all variants of our model, the *transition* from one variant to another (in C-D characterized by a change in the value of α) will then nevertheless be associated with a *temporary* change of s , namely, with more “reinvestment” while Q is changing toward higher K intensity (and with less in the contrary case). So Q/\bar{K} will be assumed to decline temporarily in the former case (and to increase temporarily in the latter), and r will be assumed to remain unchanged.

point could become vitiated (instead of merely becoming modified) by transitions from initial values of α or of ρ to successive other values. At any rate, in the models we are using an appraisal of the direction of the effect of changes in the product mix can be merely suggestive. Ours are not multisector models.

6. Even aside from changes in the product mix, it would be unrealistic to assume that our graphs would remain fixed, as an economy was moving toward the Golden Age intersection. The economy would at any event be chasing an intersection point that would alter its place in the passage of time, and the usefulness of models of this kind would at any event require that the tendency toward the point of intersection should not be wholly offset or outweighed by movements of the intersection point itself.

In this connection changes in s and in G_L deserve emphasis, and so do shifts of the innovation possibility frontier. Indeed, these will play a crucial role in our argument. Legitimately, because these problems are not of inherently multisectoral character.

(a) A reduction (increase) of s reduces (increases) the slope of the G_K curve, and it leads to a higher (lower) $(Q/\bar{K})^*$ leaving $G_K^* = G_Q^*$ at its initial value. For C-D and Hicks-neutrality the result that G_K^* and G_Q^* do not change follows directly from equation (4). For the CES innovation-frontier model the same result follows from equations (8) and (9), if we take into account that in any state of Golden Age $G_\gamma = 0$, and that for a constant G_L the value of $G_{E_L L}$ is wholly determined by the abscissa intercept of the innovation frontier. Furthermore, it is true of Figure 3 as well as of Figure 1 that if the value of the ordinate at the intersection (G_Q^*) is to remain the same then a flatter G_K curve must intersect with a flatter G_Q curve at a higher Q/\bar{K} , and a steeper G_K curve must intersect with a steeper G_Q curve at a lower Q/\bar{K} . We conclude that along the horizontal axis of Figure 1, and also of Figure 3, a reduction (increase) of s places a system that was initially in a state of Golden Age to the left (right) of a shifted Golden Age point; hence in Figure 2 such a change places the system northwest (southeast) of the purely L -augmenting Harrod-neutral tangency along the abscissa. A new approach to the Golden Age point starts from there.

(b) A rise (reduction) of G_L shifts the G_Q curve and raises (lowers) $G_Q^* = G_K^*$ as well as $(Q/\bar{K})^*$ in both models; hence it places an economy which initially is in a Golden Age to the left (right) of a new Golden Age. This too can be read directly from equations (4) and (5) for Hicks neutrality. In the CES innovation-frontier model the same follows from equation (9) and from the fact that $G_{E_L}^*$ is fixed by the abscissa intercept of the frontier at the point of pure L augmentation, and hence $G_{E_L L}^*$ must be greater (smaller) for a greater (smaller) G_L . In Figure 2 a rise of G_L moves the economy northwest, a decline southeast along the

frontier; and an approach to the purely L -augmenting tangency starts from there anew.

(c) An outward shift of the frontier places an economy which initially is in a state of Golden Age to the left of a new Golden Age. This then is followed by an approach to the new Golden Age point. A contrary shift of the frontier has the contrary consequence, but here too with a subsequent approach to the new point of Harrod-neutrality. In the new purely L -augmenting equilibrium the rate of continuous additional L -augmentation $G_{E_L}^*$ will be greater or smaller than was the case in the initial equilibrium, depending on whether the frontier has shifted outward or inward. Whether the distribution of income will have changed depends on whether the slope of the shifted frontier at its intersection with the abscissa differs from that of the old.

IV. *Diagnostic Suggestions*

At the "start"—in some reasonable sense of that hazy concept—were we "launched" from the left, with a high G_Q and a low G_K or from the right with the opposite combination? Differently expressed: were we launched with a low Q/\bar{K} , and were we subsequently moving toward higher values of Q/\bar{K} or the other way around?

No opinion will be ventured on the question as formulated above, partly because for early periods the question is rendered obscure by the obvious inadequacy of dispensing with the difficulty created by land in the particular fashion described in III, 1. We are, however, inclined to interpret the American trends of the late nineteenth and of the twentieth century as reflecting movements in the neighborhood of the Golden Age conditions, with a shifting Golden Age point usually located somewhat to the right of the actual position of the economy.

Data presented and discussed in [11] [12] [15] [17] and [18] point to the following tendencies in the behavior of the Q/\bar{K} ratio, when abnormally low depression ratios and abnormally high wartime ratios are disregarded. From the 1870s on, the ratio of annual NNP to \bar{K} —the former in the "Commerce" sense, the latter in the sense of reproducible tangible wealth net of depreciation and excluding consumer durables—has shown a sometimes constant, sometimes rising trend, with the probable exception of the closing decades of the past century and the turn of the present, at which time there seems to have been some degree of decline. If the findings are expressed with this generality, they apply both to price-corrected data (which are conceptually suspect because of the lack of intertemporal comparability of capital goods) and also to current-dollars data (which remain subject to limitations of a different kind); yet, as concerns the current-dollar ratio, the emphasis should be placed on the mildness of its movements except in depression and war.

At present the current-dollars ratio is in the neighborhood of 0.35.¹¹ The probable decline of Q/\bar{K} at the end of the nineteenth century—lasting perhaps into the very early part of the twentieth—does not qualify the suggested conclusion importantly. This is because any reasonable attempt to make allowances for land (III, 1) would change the findings in the direction of “more rise” or “less decline” for all periods under consideration, and because in the late part of the nineteenth century important shifts toward K -intensive sectors may well have moved the system temporarily to the left on the Q/\bar{K} axis even if the Golden Age point was located to the right (III, 4). Nor is it of course suggested that during three-quarters of a century the Golden Age point was *always* located to the right of the system’s actual position.

The rate of growth of “real” net capital input—expressed in [15] by an index that is subject to the conceptual difficulties already mentioned and that here includes an estimate of land in addition to reproducible wealth—had been declining gradually until about twenty years ago.¹² Subsequently it rose again to the pre-1929 level of the present century. The rate of growth of total man-hour input in the economy had been declining sharply until after World War II, and only quite recently has it picked up considerably. The annual instantaneous growth rate of L -input was 2.43 per cent for 1884–1900; 1.61 per cent for 1900–1929; 0.45 per cent for 1929–1948; and 0.70 per cent for 1948–1957. The comparable price-corrected NNP growth rates for these four periods are 3.62 per cent; 3.50 per cent; 2.69 per cent, and 3.39 per cent.

The saving ratio—interpreted for the present purpose as “capital formation as a percentage of NNP ”—declined from the late part of the nineteenth century to the present, and this seems to be a valid interpretation regardless of whether we do or do not engage in inevitably questionable price corrections, and also regardless of whether we do or do not include capital formation in the public sector. Whether we should conclude that the decline has continued over the period from 1929 to the present or was halted does, however, depend on how we handle the data in these regards. In current prices, and including all additions to

¹¹ If Q and \bar{K} are expressed in 1929 prices, then the present ratio is higher than roughly 0.35 (it is about 0.40), and in this case the rise from the level of the turn of the century to the present—a rise from somewhat less than 0.30 to about 0.40—becomes substantial. Yet price correction for capital goods whose prices appear to have risen sharply but whose identity has in fact changed is not a very revealing operation (the difficulty here is even greater than in the case of consumer goods).

¹² In the 27 years from 1874 to 1901 the K input rose by about 180 per cent; in the following 27 years by about 130 per cent. This is for the economy as a whole. The slowing is somewhat more accentuated for data limited to the private domestic economy. The data for the sixteen years following 1929 are strongly influenced by depression and war; they show a very small average rate of increase. As a result of a decline of G_L , the rate of increase in K/L was, however, rather stable until 1929; it was exceedingly small or negative during various subperiods of the span 1929–1945; and in the immediate postwar period it became even larger than it had been in the early periods.

reproducible tangible wealth other than military and consumer durables, the data in [16] point to a decline of the ratio from about 0.13 to about 0.09 or 0.10. This is from the turn of the century to the period after World War II.

Over the past sixty years the distribution of income has probably changed very slowly and gradually in favor of labor. This seems to be true even if we correct for the immediate consequences of shifts in the product mix [3], but in that event only to a very small extent. The extent of the change for the period as a whole depends quite a bit on the procedure used for estimation [3] [15]. The change seems to be somewhere *between* five and ten percentage points in terms of wL/Q , when correction is made for certain spurious sources of increase (e.g., for the rising weight of the public sector) but not for private intersector shifts. The wL/Q ratio for the private sectors is now somewhat in excess of 0.75. In the late part of the past century the private intersector shifts favored the relatively K -intensive industries, while more recently the contrary trend has shown to a small extent. Given these tendencies in the behavior of Q/\bar{K} and of $wL/r\bar{K}$, it seems reasonable to assume that r has not changed much. Other indications—all of these somewhat vague—also point to this being a reasonable assumption. As for the rate of technological progress, this has been increasing, as will be argued below.

It seems right to suggest that the approach to Golden Age intersection points has usually been from the region of somewhat lower-than-equilibrium Q/\bar{K} values. Much of the time a reduction of G_L has tended to shift the Golden Age point to the left; a reduction of s , as well as the acceleration of technological progress, have tended to shift it to the right (see III, 6).

V. *Preliminaries on Technological Advance: the Neighborhood of the Golden Bridge*

The clue to a reasonable interpretation of the data is provided by the rate of technological advance.

Were it not for the fact that from about 1890 on G_Q has shown a *very much* smaller tendency to decline than G_L , the observer *might* be tempted to explain the near-stability of Q/\bar{K} , or its tendency mildly to rise, by arguing that the Q/\bar{K} lowering effect of the sharp reduction of G_L was offset or possibly more than offset by the Q/\bar{K} raising effect of a reduction of s . A closer look at the matter leads to the conclusion that this interpretation of the behavior of Q/\bar{K} would at any event imply a much sharper reduction of s than it is reasonable to assume; still, with all the uncertainties of such calculations, one might say: it *could* have happened this way.

Yet, we do know better than that. If in our speculations for the period

ending in 1957—the outlook *as of now* we shall consider only in VI—we compensate the undeniably significant decrease in G_L by a decline of s , then we have found a strained explanation for the observed behavior of Q/\bar{K} but have found no explanation whatever for the failure of G_Q to decline to any appreciable extent. Both our models lead to the conclusion that *other things equal* G_Q^* should have declined by the same number of percentage points as G_L , and *in this regard* a decline of s does not serve as a compensating factor. In either of the two models—and also in less highly formalized frameworks—the investigator will turn to technological advance as the probable cause of the discrepancy we have pointed out.

We shall soon refer to direct evidence of an acceleration of technological progress. A piece of introductory reasoning is needed to explain that in a state of Golden Age—*only* in that state—two well-known macro-economic measures of technological advance should lead to identical results. It should not matter whether we use the algebraic product of the proportionate rate of increase of man-hour output with the L elasticity of output (i.e., in our models with wL/Q), *or* use the proportionate rate of increase in total factor productivity.¹³ The latter attempts to give us that part of the proportionate growth of output which is not explained by the growth of physical inputs on the basis of the elasticity of output with respect to these. In principle, the total factor-productivity method should give us the “correct” measure, though its computation inevitably involves a great many arbitrary assumptions and decisions [15]. But in a state of Golden Age the two measures should yield identical results. In either case we should obtain that output-growth which in the CES innovation-frontier model is attributed to the equilibrium rate of pure L augmentation, and which in the C-D Hicks-neutral model is attributed to λ in circumstances where λ enables a system with $G_L < G_K$ to function *as if* $G_L = G_K^* = G_Q^*$ were satisfied.¹⁴ These two things are

¹³ In [15] total factor-productivity is said to increase if output increases per unit of “total factor-input.” The latter is the weighted sum of physical inputs, where the base-period marginal products (factor prices) are used as weights. Along a static, linear-homogeneous production function total factor-productivity would remain unchanged. (Strictly speaking, the shorter the intervals in which the base-period weights are adjusted along the growth path, the more valid does the preceding sentence of this footnote become.)

¹⁴ In this regard the general logical implications of the C-D Hicks-neutral model are not identical with those of the CES innovation-frontier model, but the difference has no practical significance from the present point of view. In our CES model the only valid way of getting at the Q equivalent of Golden Age progress is to take the route through its L equivalent, because here Golden Age progress is purely L -augmenting. In the C-D model the route through the K equivalent or through some combination of the K equivalent with the L equivalent would *logically* be just as valid as is the route through the L equivalent. The reason for this is the same as which makes L augmentation indistinguishable from K augmentation in the C-D framework. However, when it comes to comparing *output per man hour* with total factor-productivity as measures of Golden Age progress then, even in the C-D model, the only *helpful* way of getting at the Q equivalent is to take the route through the L equivalent.

identical. In a state of Golden Age the proportionate rate of increase of output per man hour, before we multiply the increase by wL/Q , gives us that *deficiency* of G_L , in relation to G_K and to G_Q , which is made up by technological progress, so that prior to any weighting the L -input equivalent of progress is obtained (see the excellent discussion in [5]). If we then multiply this L equivalent by the L elasticity of output, we arrive at the Q equivalent of progress, and this is the same Q equivalent which we are trying to obtain by the total factor-productivity method.

However, by the standards of total factor productivity the proportionate increase in output per man hour multiplied by the L elasticity of output *understates* technological advance during an approach from the northwest in Figure 2 (i.e., during an approach from the left in Figure 3 or in Figure 1). The same measure *overstates* the advance during an approach from the other side. This is because the measure we are now considering is appropriate to circumstances in which $G_K = G_Q$, and in which therefore the role of technological advance can be described as filling in the gap between G_L and $G_K = G_Q$. If during the approach $G_K < G_Q$, as is the case coming from the left (i.e., from the northwest), then the man-hour output method implies a higher G_K than is the true value of *this* growth rate, and the method therefore gives K more than its due, hence underrates technological advance. The contrary is true of an approach from the other side.

In the CES innovation-frontier framework, with factor-augmenting progress, we arrive at the conclusion that approaches to the Golden Age point along a given innovation frontier will usually be associated with a change in the rate of technological progress, in the sense in which the total factor-productivity method attempts to measure progress.¹⁵ Denote here by \hat{X} the share of labor (wL/Q) at some initial point along the frontier, either northwest ("left") or southeast ("right") of Golden Age; and the share of labor at the Golden Age point by X^* ; the rate of new K augmentation per period at the initial point by \hat{G}_{EK} ; the rate of new L augmentation per period at the initial point by \hat{G}_{EL} ; the same at the Golden Age point by G_{EL}^* ; then for the transition from the initial point to the Golden Age point, the change in the instantaneous rate of technological advance—($A^* - \hat{A}$) *along the frontier*—will be

$$(12) \quad A^* - \hat{A} = X^* G_{EL}^* - [\hat{X} \hat{G}_{EL} + (1 - \hat{X}) \hat{G}_{EK}]$$

Whether $A^* - \hat{A}$ is positive or negative (or conceivably zero) depends therefore partly on the relationship between G_{EL}^* and $\hat{G}_{EL} + \hat{G}_{EK}$ (i.e., on the slope of an arc expressing the tradeoff between augmentation

¹⁵ In the CES innovation-frontier model, with factor-augmenting progress, the increase in total factor-productivity results from the summation of the Q -raising effects of the two individual factor-augmentations.

rates during the transition as a whole), and partly on the change in the relevant X weights as a result of the curvature of the frontier.

We conclude:

1. When proportionate rates of increase of a total factor-productivity index are used to measure technological advance, a change in the rate of advance should be expected during approaches to Golden Age conditions. This conclusion presupposes the CES innovation-frontier model, with factor-augmenting progress. In the C-D Hicks-neutral system the rate of technological advance remains constant, as long as the production function remains unchanged.

2. If the measure of technological advance is constructed by way of multiplying the proportionate rate of increase of output per man hour by the L elasticity of output (say by wL/Q), we should observe a lower rate of technological advance than is obtained by the total factor-productivity method while the system is moving toward Golden Age from the left, and vice versa for a movement from the right, but the difference should gradually diminish as Golden Age is approached. The conclusion is valid for both our models, and in our CES innovation-frontier model it is possible to add that what we called an approach from the left (right) can alternatively be described as a Figure 2 approach from the northwest (southeast). In either model the conclusion derives its justification from the fact that the man-hour output method understates technological progress when Q/K is rising and overstates progress when Q/K is declining, but this understatement or overstatement is gradually decreasing as we get nearer the Golden Age point.

3. As was explained on p. 1090, in Golden Age the two measures should yield identical results. This conclusion too is valid for both our production-function models. Furthermore, since man-hour output measures are acceptable also to those economists who try to avoid production functions involving "real" capital inputs, we have here, at least at one point, a Golden Bridge between two groups of economists with very different methodological preferences. In a state of Golden Age the two groups should be able to agree on the proposition that the effect of technological progress is that of making up the gap between G_L and G_Q .¹⁶ A judgment concerning the existence of Golden Age conditions need not necessarily be interpreted as an explicit judgment on the stability of Q/K —hence as an explicit judgment on K inputs—because it may be possible to arrive at the same diagnosis indirectly via the constancy of rates of return and of wL/Q . As for weighting of the increase of the man-hour output index by the L elasticity of output, it seems to be generally accepted that multiplication by wL/Q gives at least a crude approximation to the correct weighting, but we should note also that

¹⁶ See footnote 14.

obtaining merely the "golden" L equivalent of progress (rather than its "golden" Q equivalent) does not even call for such weighting.

4. The man-hour output measure (advocated in [5]) avoids some of the worst difficulties that are inevitably connected with total factor-productivity estimation, and it therefore has obvious advantages whenever a general appraisal of the data suggests the existence of near-Golden Age conditions. This is true in spite of the fact that the idea underlying the total factor-productivity method incorporates the theoretically correct principle, practical application of which, however, requires very far-reaching simplifying assumptions.

5. If a disturbance moves the system in the direction opposite that in which the Golden Age point is located, and if for a while the direction of the movement is determined predominantly by the disturbance itself, rather than by the forces leading to a subsequent Golden Age approach, then, temporarily, the measures of technological advance should be expected to behave in a fashion contrary to that in which they would behave if the system had continued to move in the direction of the Golden Age point (see III, 5). Example: An economy approaching Golden Age from the left may be moved back, further left, by a sufficiently pronounced shift of the product mix toward K -intensive industries, and the subsequent Golden Age tendency may not come through for a while; during the interim period when Q/K is declining, man-hour output measures will tend to overstate the rate of technological advance even if in the circumstances here envisaged the Golden Age point is located to the right of the system's actual position. The conclusion is valid for both models. The man-hour output measure necessarily gives technological progress too much credit whenever K/Q is increasing (and vice versa), irrespective of the reasons why K/Q changes.

VI. *The Rate of Technological Advance: Conclusions from Empirical Observations*

We now turn to the data.

1. With reliance on [15] we calculate the following instantaneous growth rates p.a. for the index of total factor-productivity (NNP-productivity of inputs) in the private domestic economy. From the year 1884 to the year 1900: 0.55 per cent; from 1900 to 1929: 1.42 per cent; from 1929 to 1948: 2.01 per cent; from 1948 to 1957: 2.21 per cent.

Using the same source we calculate, for the private domestic economy, the following instantaneous growth rates p.a. in the (net) output per man-hour index, *not yet multiplied by the share of labor*. From 1884 to 1900: 1.22 per cent; from 1900 to 1929: 2.06 per cent; from 1929 to 1948: 2.39 per cent; from 1948 to 1957: 3.25 per cent. Rounded to the

first decimal, the simple annual compound rates of increase would in all cases be the same as the instantaneous rates.

2. For the entire period 1884–1957 we find that the (net) output per man-hour index for the private domestic economy rose at a 2.11 per cent instantaneous rate p.a., and that the total factor-productivity index rose at a 1.50 per cent instantaneous rate. Thus we find that, for the *period as a whole*, weighting the proportionate increase in man-hour output by about 0.71 yields the same result as using the proportionate increase in the total factor-productivity index as one's measure. The discussion in V3, comes to mind: this is very nearly the relationship one would expect to find for an economy in a state of Golden Age. However, this same statement concerning the empirical confirmation of the Golden Age relationship between the two compound rates is not valid for the individual subperiods with different rates of progress. Nor did of course G_Q , G_K , or s show Golden Age constancy, as was explained in V.

3. It follows from VI, 1 that, if G_F stands for the proportionate rate of increase of the total factor-productivity index, and G_M for the proportionate rate of increase of the man-hour output index from one year to the next, then regressing G_F or G_M on time (t) will give not only positive constant terms ($a > 0$), but also positive regression coefficients ($b > 0$). We first used three-year moving averages, and fitted by least squares for 1891–1956, a period for which yearly estimates are found in [15]. The value of the regression coefficient b —i.e., of the coefficient of t when the latter is measured in years—is somewhat in excess of 0.02 in both linear regressions.¹⁷ In the G_F regression this coefficient is significantly different from zero at the 5 per cent level, but just barely so; in the G_M regression it misses that particular mark by a shade.

Subsequently, we used ten-year moving averages (first decade: 1890–1899, last: 1948–1957), and for G_F this resulted in

$$(13) \quad \begin{aligned} y &= G_F = 1.087 + 0.025t \\ n &= 59; \quad S_y = 0.8153; \quad \sigma_b = 0.0069 \end{aligned}$$

For G_M we obtained:

$$(14) \quad \begin{aligned} y &= G_M = 1.662 + 0.025t \\ n &= 59; \quad S_y = 0.9082; \quad \sigma_b = 0.0062 \end{aligned}$$

Here both b coefficients—pointing to a one percentage point increase of the growth rates of the indexes in 40 years—are significantly different from zero at the 1 per cent level. The deviations show a pattern that is distinctly cyclical, i.e., they reflect so-called long swings. Considering

¹⁷ Here again the indexes we have used relate to the private domestic economy. The proportionate rate of increase of man-hour output is *not* weighted by wL/Q .

the methodological problems hidden behind techniques of this sort, we should remind ourselves of the fact that VI, 1 seemed rather convincing without much further elaboration.

4. In VI, 1 and 3, the trend of G_F was compared with the trend of G_M but it *should* have been compared with the trend of $(wL/Q)G_M$. Finer shadings in labor-share estimates cannot be trusted sufficiently to make this rewarding. However, it is likely that, over our long period as a whole, $(wL/Q)G_M$ rose by slightly less, in terms of percentage points, than did G_M . This is very likely in spite of the fact that, as a result of the rise in the labor-share, $(wL/Q)G_M$ must have risen in a somewhat higher *proportion* than G_M . Weighting G_M by L elasticity would presumably have reduced the resulting estimate of the trend-slope of $(wL/Q)G_M$ somewhat below the values suggested for G_F by equation (13), since the percentage point difference between, say, $G_M=3$ per cent and $G_M=1.5$ per cent is a little greater than the difference that results after weighting 3 per cent by say, 0.75 or even 0.80 and 1.5 per cent by say, 0.70. The discrepancy between weighted and unweighted results does not remain constant as we move on in time; and if he had found such a difference it would at any event have been too small to serve as a basis for the interpretation of facts.

In view of VI, 1 and 3, we suggest that the rate of technological advance has been subject to a consistent uptrend, though with large variations about the trend. This leaves the question open whether in the CES innovation-frontier model the increase should be interpreted as a movement along a given frontier, down from the northwest toward a Golden Age point (V, 1), or as an outward shift of the frontier itself, with a small steepening of the outward-shifting frontiers in the neighborhood of pure L augmentation. We suggest placing more emphasis on the second than on the first of these two interpretations, i.e., on the outward shift. A look at V, 3, and at VI, 2 shows that, as a result of the mutual cancellation of the effects of changing parameters, our long period as a whole had *pretty nearly* the technological-progress characteristics of a Golden Age. To be sure, the subperiod data in VI, 1 show, as do other indicators, that this long-period result should be attributed to the mutual offsetting of the effects of deviations observable in successive intervals. These subperiods had somewhat differing traits, partly no doubt because of the product-shift phenomenon explained in III, 5, and V, 4. But if, as concerns measures of technological advance, the long period points to near-Golden Age conditions via the mutual offsetting of successive deviations, it becomes unconvincing to attribute the consistent increase of the rate of technological advance, in the sense of a rising G_F and of a rising G_M , *mainly* to a sustained movement of the economy toward the Golden Age point *along* any given frontier.

A significant decline of G_L , as described in IV and V, seems to have been the most important Q/\bar{K} -diminishing force, but it seems to have been balanced by two Q/\bar{K} -raising forces, namely, by a diminution of s and by a rise in the rate of technological advance (particularly through an outward shift of the frontier). While the last two of these forces do indeed seem to have *jointly* balanced or even overbalanced the first in their effect on Q/\bar{K} , the outcome is somewhat different when we consider the effect of these forces on G_Q . In this regard too the reduction of G_L remains an important diminishing force—i.e., it is a force diminishing G_Q as well as Q/\bar{K} —but the increase of the rate of technological progress is the only important G_Q -raising force. The decline in s does not work in that direction (III, 6). Hence on balance we do obtain a small decline in G_Q (as observed in IV and V). Most of the time the economy has been moving in the direction of a Golden Age point which along the abscissa of Figures 1 and 3 was probably shifting somewhat to the right (see pp. 1087–88). Changes in the product mix tended to push the economy temporarily away from the Golden Age point mainly at a time when these changes favored the K intensive industries and thus exerted a temporary push toward lower Q/\bar{K} ratios.

If the near future should bring a high G_L , and no further decline of s , it will be reasonable to expect a more pronounced tendency toward higher Q/\bar{K} values and, in contrast to the past, also a tendency toward rising values of G_Q and of G_K in the post-1957 period.

In I we explained why we prefer the CES innovation-frontier model to the C–D Hicks-neutral framework, though the relative advantages of the two approaches have so far remained controversial (see [20] and the reference on its p. 577 to other writings of the same author). The model we prefer has the considerable advantage of deriving fairly stable distributive shares from reasonable maximization assumptions. Shares-stability is a matter of great importance, because in market economies with a high and rising rate of technological advance it is likely to lead to a steep uptrend in real wages along with favorable trends in the rate of return on investment. Any such statement obviously has implications concerning the adequacy of economic policies, the forces of competition, etc., but on these we shall not elaborate here. The mild increase in the L share, which seems to have occurred even if correction is made for the effects of intersector shifts, is easily explained in the CES innovation-frontier model, particularly if we take account of a gradual outward shift of the frontier. The increasingly intense and systematic research-and-development effort has doubtless played a role in sustaining these outward shifts; but in L -augmenting economies the obstacles that need to be overcome are likely to be somewhat greater in the region surrounding pure L augmentation than in other regions of the frontier. A small degree of steepening of the frontier in that region, if coupled

with a gradual rise of $(Q/\bar{K})^*$ and of Q/K , should not lead the observer to expect a downtrend in r .

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PRODUCTIVITY GROWTH IN LATIN AMERICA

By HENRY J. BRUTON*

Most observers are now convinced that long-run increases in national product cannot be fully explained in terms of increasing inputs of capital and labor as these factors are conventionally measured. Although there are severe difficulties of measurement, the accumulated evidence, covering numerous countries and a variety of time spans, indicates clearly that more capital and more labor of unchanging quality can rarely explain more than one-half the estimated growth of Gross National (or Domestic) Product. The obvious consequence of this evidence is a search for other sources of growth, for the "residual" component of growth theory [5] [21]. The purpose of the present paper is to examine the growth of Gross Domestic Product in five Latin American countries (Argentina, Brazil, Chile, Colombia, and Mexico) in the period 1940-1964 in terms of hypotheses about possible explanations of the "residual" source of growth in these countries. The development and appraisal of the various hypotheses considered is based on a comparison of the rate of growth of productivity (the residual) among the several Latin American countries and a comparison between these countries and a group of more economically advanced countries.

Part I outlines the approach employed and describes the data. Part II presents the principal results, Part III offers an interpretation of these results, and Part IV is a short summing-up.

I

A. *The Approach*

The arguments are built around an aggregate production function, and the residual is isolated in the manner originated by Robert Solow [13]. That this approach is both useful and treacherous is now well established, and little is gained from continued debate as to its conceptual and theoretical basis. The contention here is that its use enables us to learn a great deal about Latin American growth, and, in the present context, this is defense enough. Richard Nelson [12] has recently provided an admirable review of all aspects of the model, and here we need

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only define symbols and note points of special relevance to later discussion.

The Cobb-Douglas production function may be written as

$$(1) \quad P_t = A_t K_t^a L_t^b$$

where P is "potential" (i.e., full employment) GDP in year t ; K and L are quantities of capital and labor available in the same period, A an index of productivity, and a and b are elasticities of output with respect to capital and labor respectively. Assume further that the sum of a and b is unity, and that their respective values are not affected by changes in A . Take logarithms and differentiate with respect to time and get

$$(2) \quad r_P = r_A + ar_K + br_L$$

and

$$(3) \quad r_A = r_P - (ar_K + br_L)$$

where r_P , r_A , etc., refer to annual proportionate rates of growth of GDP, productivity, etc. The task is to explain r_A , the rate of growth of productivity of capital and labor.¹ Given the production function the explanation is necessarily in terms of improved quality of the two inputs and improvements in their utilization. Nelson, following Solow, Denison [14] [4], and others, seeks to break down r_A into components of improved quality of labor and technical change embodied in newly created capital. He then derives an expanded version of (2) in which these sources of growth are explicitly included, and leaves a residual explicable chiefly in terms of improved allocation.² For the present investigation only one modification of this approach is necessary.

The use of potential output washes out the effect on r_A of changes in the extent of underutilization of existing capacity. This procedure is justified on the grounds that underutilization is largely due to problems of aggregate demand, and as such has nothing to do with the productivity of the inputs. Estimates of the contributions to productivity growth of embodied technology, improved education, and better allocation must

¹ The derivation given by (3) shows why the term "residual" is suggested for r_A . It refers to that part of r_P not accounted for by increased capital and labor, and is obtained by simple subtraction. Since a positive r_A results in capital and labor becoming more productive, it is convenient to refer to it as the rate of growth of (total) productivity.

² Let λ_K refer to the rate of growth of capital productivity embodied in machines, λ_L to the rate of growth of the quality of labor (both constant over time), and $\bar{\alpha}$ as the average age of capital. Then the Nelson expansion of (2) is

$$r_P = r'_A + a\lambda_K + b\lambda_L + a\lambda_K\Delta\bar{\alpha} + br_L + ar_K$$

where r'_A is increased productivity not embodied in capital or due to improved quality of the labor force. Nelson [12] derives this equation rigorously, but once derived it is intuitively obvious.

then be made directly. For the Latin American countries, however, the evidence (cited below) is convincing that the underutilization is not due to inadequate demand. It is therefore more fruitful to assume that the ability to exploit capacity is an important factor in potential output, and that changing utilization is a key variable explaining productivity growth in the Latin American countries (hereafter LAC). Indeed the principal empirical result of the investigation is that virtually all of the variance of r_A for LAC can be explained by variation in the degree of utilization. Given this result we can then deduce something about the rate of growth of "pure" (i.e., that not explained by changing utilization) productivity: namely, that it has been virtually zero over the time period covered. The task of Parts II and III is to derive and defend this conclusion, and to offer an explanation for the failure of r_A for LAC to grow independently of changes in the degree of utilization.

B. *The Data*

The data for both the advanced group (AG) of countries and for LAC are open to many questions. I have not constructed any new series, but have pieced together data from a variety of sources and have modified and adjusted a number of existing series to arrive at estimates of the variables called for in equation (2). Despite the questionableness of many of the individual observations, the series in general appear consistent with other available evidence, and, in general, their conceptual bases are acceptable. We work exclusively with rates of growth, rather than absolute numbers or ratios of absolute numbers, and in general rates of change are more meaningful than the absolutes from which they are drawn. In view of all this we have concluded that the data are worthy of analysis, and should not be relegated merely to filling yearbooks.

The findings which we will seek to analyze are presented in Tables 1 and 2. The logic of the time periods chosen for the Latin American group is indicated as we proceed. For the advanced countries the dates selected were those for which diversity in the value of r_A was most marked.

The values for a and b used in estimating r_A are the relative shares of output accruing to the two inputs. In the case of the advanced countries capital share is given as .30 in all cases except for the United States where .25 is used. For the five Latin American countries capital's share was taken to be the following:

| | |
|-----------|-----|
| Argentina | .40 |
| Brazil | .45 |
| Chile | .50 |
| Colombia | .45 |
| Mexico | .50 |

While these percentages for LAC are based on incomplete data, they are consistent with a substantial body of evidence, and can be taken as a close approximation to what complete data would indicate. In the manufacturing sector alone capital's share will exceed 60 per cent in almost all the Latin American countries.

II

The simplest point to make about the data of Tables I and II has to do with the mean values of r_A and r_A/r_P of the two groups of countries. For the Latin American countries, r_A averages 1.4 and that of r_A/r_P is .26. For the advanced group of countries the averages are 2.8 and .58 respectively. If the extreme cases of Mexico for 1940-45 and Canada were excluded, the differences between the two groups would be even more marked.

This result is contrary to the frequently encountered notion that less developed countries with a somewhat primitive technological base may reap large windfalls by exploiting recently developed knowledge. If this were a valid hypothesis, one would expect that during years when modernization of the economies was actively promoted and the rate of capital formation comparatively high, LAC should have experienced an r_A considerably higher than that achieved by those countries already technologically advanced. That this did not occur requires some explanation.

A. *The Role of Capital*

If productivity growth played a smaller role in the growth of output in LAC than in AG, then obviously inputs must have played a larger role. That growth of capital should be more important in LAC is suggested by the estimates of relative shares given above. But then why should capital's share be greater in LAC? The answer seems to be partly a matter of technology and partly of market structure. Available data suggest that the variance of capital output ratios (aggregate and sectoral) is considerably less than the variation in labor output ratios. Further, the observed differences in the capital output ratios—unlike those of the labor output ratios—are not all in one direction, i.e., one cannot say that the capital output ratios in LAC are systematically higher or lower than in AG, while of course the labor output ratios are much lower across the board in AG than in LAC.³ Given this evidence the assumption that the capital output ratio in LAC is about equal to that in AG is an acceptable approximation.⁴

³ This point has been noted by several people. See especially Balassa [2] and Leontief [10].

⁴ If the argument were limited to the fixed-capital full capacity output ratio in the manufacturing sector, the evidence is even more convincing.

TABLE 1—GROWTH RATES OF INPUTS, OUTPUT, AND PRODUCTIVITY IN
LATIN AMERICAN COUNTRIES

| Country and Period | r_P | r_K | r_L | r_A | r_A/r_P |
|--------------------|-------|-------|-------|-------|-----------|
| Argentina | | | | | |
| 1940-45 | 2.9 | 0 | 2.1 | 1.6 | .55 |
| 1946-51 | 3.4 | 3.9 | 2.4 | .4 | .12 |
| 1955-59 | 1.7 | 3.4 | 1.5 | -.6 | -.35 |
| 1960-64 | 1.2 | 4.6 | 0 | -.6 | -.50 |
| Brazil | | | | | |
| 1940-45 | 3.2 | 2.2 | 1.7 | 1.3 | .41 |
| 1947-53 | 5.6 | 5.9 | 2.4 | 1.6 | .29 |
| 1955-59 | 5.6 | 5.2 | 2.8 | 1.7 | .30 |
| 1960-63 | 5.0 | 5.1 | 2.8 | 1.2 | .24 |
| Chile | | | | | |
| 1940-45 | 2.7 | .9 | 1.8 | 1.4 | .52 |
| 1946-53 | 3.9 | 3.0 | 2.1 | 1.4 | .36 |
| 1955-59 | 3.0 | 3.4 | 2.5 | .1 | .03 |
| 1960-64 | 4.0 | 4.8 | 1.4 | .9 | .23 |
| Colombia | | | | | |
| 1940-45 | 2.8 | 1.5 | 1.8 | 1.1 | .39 |
| 1946-53 | 5.2 | 4.0 | 2.1 | 2.3 | .44 |
| 1955-59 | 4.0 | 4.9 | 2.6 | .4 | .10 |
| 1960-64 | 4.5 | 4.3 | 2.0 | 1.5 | .33 |
| Mexico | | | | | |
| 1940-45 | 9.0 | 1.7 | 2.8 | 6.7 | .74 |
| 1946-53 | 5.0 | 4.9 | 2.6 | 1.2 | .24 |
| 1955-59 | 5.7 | 4.2 | 3.1 | 2.0 | .35 |
| 1960-64 | 6.2 | 4.2 | 2.5 | 2.8 | .45 |

Source: Col. 1 (r_P)—computed from the published data of the national accounts of the various countries. Rates are for Gross Domestic Product in 1950 prices. Data after about 1950 are more satisfactory than those for the 'forties, and in some cases pre-1950 are quite rough. Also data for GDP 1963 and 1964 are preliminary and subject to revision. A description of the national accounts data for Latin American countries is given in [17].

Col. 2 (r_K)—in all cases the capital stock figures on which the r_K 's are based were obtained from bench mark capital stock estimates and accumulations from gross investment and depreciation estimates provided by the national accounts. A general survey of capital stock estimates in Latin America with additional references to specific country studies is Alexander Ganz [7].

Col. 3 (r_L)—computed from estimates of employment made by ECLA and presented in [16, p. 3]. Data for certain years were obtained by logarithmic interpolation.

If the capital output ratio is broadly similar for both groups of countries while capital's share is significantly greater in LAC, then the marginal product of capital must be greater in LAC than in AG. Why should this be? If the marginal product of capital in LAC exceeds that in AG because capital is combined with more labor in the former, then the capital output ratio in AG must exceed that in LAC. This latter

TABLE 2—GROWTH RATES OF INPUTS, OUTPUT AND PRODUCTIVITY
IN ADVANCED COUNTRIES

| Country and Period | r_P | r_K | r_L | r_A | r_A/r_P |
|--------------------|-------|-------|-------|-------|-----------|
| Belgium | | | | | |
| 1949-54 | 3.6 | 2.4 | .6 | 2.5 | .69 |
| 1954-59 | 2.3 | 2.7 | — .1 | 1.6 | .70 |
| Canada | | | | | |
| 1949-59 | 4.3 | 7.1 | 2.1 | .7 | .16 |
| Netherlands | | | | | |
| 1949-54 | 4.9 | 4.0 | 1.4 | 2.7 | .55 |
| 1954-59 | 4.1 | 5.5 | 1.1 | 1.6 | .39 |
| Norway | | | | | |
| 1949-59 | 3.7 | 4.4 | .2 | 2.3 | .62 |
| Sweden | | | | | |
| 1949-59 | 3.4 | 2.0 | .5 | 2.5 | .73 |
| United Kingdom | | | | | |
| 1949-59 | 2.5 | 3.1 | .6 | 1.2 | .48 |
| France | | | | | |
| 1949-54 | 4.8 | 2.9 | .1 | 3.8 | .79 |
| 1954-59 | 4.1 | 3.9 | .2 | 2.8 | .68 |
| Italy | | | | | |
| 1949-54 | 6.4 | 3.0 | 1.5 | 4.4 | .69 |
| 1954-59 | 5.7 | 3.4 | .8 | 4.1 | .72 |
| West Germany | | | | | |
| 1950-54 | 8.3 | 4.8 | 1.8 | 5.6 | .67 |
| 1954-59 | 6.6 | 6.9 | 1.4 | 3.5 | .53 |
| Israel | | | | | |
| 1952-58 | 9.8 | 11.8 | 3.3 | 3.9 | .40 |
| Japan | | | | | |
| 1950-58 | 7.9 | 10.6 | 2.4 | 3.0 | .38 |
| United States | | | | | |
| 1947-54 | 4.4 | 4.0 | .7 | 2.9 | .66 |
| 1954-60 | 3.5 | 3.1 | .8 | 2.1 | .60 |

Source: Data for the United States from Nelson [12], for the European countries [15], and for Israel and Japan from Aukrust [1]. The Aukrust paper contains a summary report of productivity growth in all the countries included in this table except the United States.

inequality, as already noted, does not exist, and hence a conventional variable proportion argument will not explain the deduced differences in the marginal product of capital. Neither can one appeal to technological factors as the source of the difference, since technical progress is surely more rapid in AG than in LAC.

The similarity of capital output ratios can be explained in terms of the preponderance of imported physical capital in the more rapidly growing sectors of the developing economies. This equipment is usually designed for a high-wage economy, and rarely are modifications made in this imported equipment. Wage rates in LAC are much lower than in AG, and despite the fact that LAC has a much lower capital labor ratio her wage bill is a smaller proportion of total output than in AG. This could not be the case, however, if the marginal product of capital were very low in LAC. Two factors seem to account for the higher marginal product of capital in LAC. In the first place there are in LAC numerous and obvious "gaps" in the capital structure to be filled. To a considerable degree investment represents efforts to fill in these gaps, i.e., to add to the extensiveness of the capital structure. In the richer countries with an already extensive capital structure, investment was much more in the form of replacing and duplicating existing capital. In this latter case, new capital as such is expected to add less to the capacity of the economy since it is replacing or duplicating capital capacity already there. Secondly, the import substitution policy—the policy of curtailing or eliminating entirely the importation of certain products to encourage their domestic production—also creates gaps that in turn provide possibilities for profitable investments. The monopolistic position of most plants then permits the maintenance of prices at levels sufficient to assure relatively high returns on the capital. In the context of the present discussion, it is this latter explanation of the high marginal product of capital that is particularly relevant.

B. The Role of Productivity Growth

The preceding argument suggests why capital's role in LAC is more important than it is in AG. It does not, however, tell us why productivity growth is generally lower in the former country than in the latter. It is this question that we now consider.

It may be asserted that at least part of r_A is explained in terms of the flow of new technical knowledge from research and development activity. Such activity is carried on to a much larger extent in the advanced countries than in LAC. This new technical knowledge is rarely easily and costlessly transferred to countries with different factor endowments and different organizational arrangements. Thus whatever opportunities for the exploitation of new technology existed, there did not exist the capacity to adapt and modify this technology to use it effectively in Latin America. It is also probably correct to assume that the rate of growth and of improvement in the educational establishments was greater in the former group than in the latter. In terms then of the customarily identified sources of r_A , technical advance and education, the Latin American countries lagged. This argument is intuitively appeal-

ing, but its validity depends very much on the assumption as to the sources of productivity growth. We need then to investigate the extent to which the data in Tables 1 and 2 support this generalized notion of the sources of productivity growth.

The production function states that the growth of output is explained by the growth of inputs and an increase in their productivity. If then we calculate for a group of similar countries a simple regression of r_P on the rate of growth of inputs, r_n , ($r_n = ar_K + br_L = r_P - r_A$), the resulting equation should indicate, by its intercept, the average value of r_A and the regression coefficient of r_n should of course be about unity. The estimates of r_P and r_n for the 18 observations of the advanced group of countries yields the following equation:

$$(4a) \quad AG \ r_P = 2.47 + 1.17 \ r_n \quad \bar{r}^2 = .64$$

(.21)

Equation 4a conforms to expectations reasonably well. The regression coefficient is not significantly different from unity, and the equation accounts for almost two-thirds of the variance in r_P .

For LAC, for the 1940-64 period, the corresponding equation is

$$(4b) \quad LAC \ r_P = 1.26 + 1.06 \ r_n \quad \bar{r}^2 = .23$$

(.40)

Equation 4b shows LAC's smaller r_A (the lower value of the intercept compared to that in 4a) and a regression coefficient not significantly different from one. The equation however explains such a small proportion of the variance of r_P that it is not appropriate to interpret it as describing the relationship between r_P and r_n for LAC in the same manner that equation 4a does for AG.

Data in Table 1 suggest that the relative magnitude of the sources of growth in LAC was in fact substantially different during the war years from that of later years. If the observations for the war years are eliminated, equation 4b becomes

$$(4c) \quad LAC \ (1946-64) \ r_P = -1.73 + 1.89 \ r_n \quad \bar{r}^2 = .66$$

(.35)

and if the observations for the immediate postwar period are also eliminated, the regression becomes

$$(4d) \quad LAC \ (1955-64) \ r_P = -2.87 + 2.21 \ r_n \quad \bar{r}^2 = .76$$

(.39)

The changes in the regression equations all move in a similar direction, the values of r_A (the vertical intercepts) fall sharply and those for

the regression coefficient and \bar{r}^2 rise. The rising values of the regression coefficients and the \bar{r}^2 's indicate an increasing dependence of r_P on r_n . However, both the negative intercept and the regression coefficient in excess of unity imply that this increased dependence is due to the effects of changes in the degree of utilization. Thus a negative rate of growth of productivity can be most satisfactorily explained in terms of output falling more rapidly than it is possible to reduce the quantity of inputs. Similarly the regression coefficients of equations 4c and 4d, both significantly greater than one, imply that the growth of output exceeds the growth of inputs, not because of a "pure" productivity effect, but because a positive r_n reduces the extent of utilization.

These regressions suggest the following conclusion: for AG equation 4a provides an economically and statistically meaningful summary of the data of Table 2 and of the relationship between the rates of growth of output, inputs, and productivity. The same regression for LAC is revealing in a different way. Equation 4b takes a form consistent with Equation 4a but the low value of \bar{r}^2 suggests that the relationship changes over the time period considered. Equation 4c and 4d verify this result. These latter equations also indicate that r_n becomes increasingly important in the explanation of r_P as the period progresses. Given the observed change in r_A , we may conclude that this rising importance is due both to the role of inputs on the supply side and their role in preventing or reducing underutilization.

Another way of looking at the data is in terms of a direct explanation of r_A . The notion that increases in productivity result from improvements embodied in capital equipment and from better educated, better trained workers suggests that r_A and r_n should move together, i.e., that r_n is a carrier of r_A . Thus for countries with similar technology and similarities in rate of technical progress and comparable improvements in education a positive and significant relationship between r_A and r_n is expected. Equation 5a is this regression for the eighteen observations for AG.⁵

$$(5a) \quad AG \ r_A = 2.47 + .17 \ r_n \quad \bar{r}^2 = .02$$

(.21)

This equation shows, contrary to expectations, that inputs are not carriers of the sources of productivity growth. From 4a and 5a we may conclude that for AG the rate of growth of productivity was in fact simply added to whatever growth is produced by increased inputs. In particular the evidence is consistent with the hypothesis that r_A is autonomous with respect to the growth of inputs.

For the Latin American countries the regression of r_A or r_n with all

⁵ It should be recognized that equations (5a)–(5d) are derivable from equations (4a)–(4d).

twenty observations yields

$$(5b) \quad \text{LAC } r_A = 1.26 + .06 r_n \quad \bar{r}^2 = 0$$

(.40)

The form and interpretation of this regression are similar to 5a. Again, however, eliminate the war years and then the first postwar period to get equations 5c and 5d.

$$(5c) \quad \text{LAC (1946-64) } r_A = -1.73 + .89 r_n \quad \bar{r}^2 = .29$$

(.35)

$$(5d) \quad \text{LAC (1955-64) } r_A = -2.87 + 1.21 r_n \quad \bar{r}^2 = .50$$

(.39)

The regression coefficients and adjusted coefficients of determination rise through time, but this change cannot mean that in the later periods r_n began to carry r_A for if this were the case, the vertical intercept would not turn negative. The negative intercept implies that the increasing strength of the relationship is due not to r_n 's role as a possible carrier of increasing productivity, but as a source of demand, i.e., r_A becomes increasingly dependent on the growth of demand. More specifically, the rate of growth of productivity seems increasingly dependent on changes in the degree of utilization, and the evidence shows little or no independent growth of "pure" productivity.

The preceding discussion suggests a final way to consider the data, namely a regression of r_A on r_P . For AG this regression is

$$(6a) \quad \text{AG } r_A = .64 + .44 r_P \quad \bar{r}^2 = .51$$

(.10)

This equation indicates that if r_P were zero r_A is still about .64, and hence implies that if r_P were zero (or slightly above) r_n must be negative (e.g., capital not replaced, increased unemployment, etc.). This result is also consistent with the notion that productivity growth occurred in AG more or less independently of the growth of inputs. That the equation explains one-half the variation in r_A is due simply to the fact that, for AG, r_A is a very large proportion of r_P .

The picture for LAC emerging from the regression of r_A on r_P is again different from that for AG and again changes over the time period considered.

$$(6b) \quad \text{LAC} = -1.71 + .74 r_P \quad \bar{r}^2 = .75$$

(.10)

$$(6c) \quad \text{LAC (1946-64) } r_A = -1.62 + .63 r_P \quad \bar{r}^2 = .85$$

(.07)

$$(6d) \quad \text{LAC (1955-64)} \quad r_A = -1.67 + .64 r_P \quad \bar{r}^2 = .91$$

(.06)

The value of the intercepts again shows that productivity growth is negative unless r_P is substantially positive, a result directly opposite to that shown by equation 6a for AG. Similarly, the rising \bar{r}^2 indicates a heightened dependence of r_A on r_P , i.e., changes in the degree of utilization account for an increasing proportion of the variation in r_A . In the decade 1955-64 over 90 per cent of the variation of r_A is accounted for by variation in r_P . And this occurs even though r_A/r_P in this latter period is much smaller than it was in the earlier period.⁶

Summary

The results of the regression analysis may now be summarized. We have examined three relationships for AG and LAC: r_P on r_n , r_A on r_n , and r_A on r_P . For AG both r_A and r_n are important in explaining r_P , and their effect on r_P is more or less additive. Thus the evidence is consistent with the notion that r_A represents a flow of improvements that can be employed independently of the growth of inputs.

For LAC the 1941-45 period reveals a picture very similar to that summarized for AG. After 1945, this picture changes. The rate of growth of output and the rate of growth of productivity become increasingly dependent on r_n . The negative vertical intercepts in both sets of regressions suggest that this dependence is due to the effect of r_n on the rate of growth of capacity utilization. This last possibility is further supported by the rise in \bar{r}^2 between r_A and r_P over the twenty-five year period. From these results we concluded that the variation in the values of r_A is due primarily (after 1945 and especially after 1955) to the ability of LAC to exploit fully its capital and labor resources. If this is correct, then the important conclusion that "pure" productivity growth has, in the past decade or so, been about zero emerges.

The findings for LAC would be consistent with the assumption that underutilization is due to a conventional oversaving problem. The evidence that this is not the case is convincing. In the first place, there are the inflations. A simple excess demand theory of inflation is probably not adequate to explain inflation in Latin America, but equally probably such inflations could not continue if demand were not pressing against capacity in key sectors of the economies. Also the rate of capital formation (see Table 1) has generally been quite high, and this fact is difficult to reconcile with a general oversaving problem. There is evidence of a positive nature [18] [19] which indicates that the underutilization is due to bottlenecks on the input side. Finally, an oversaving argument

⁶ The regression of r_A on r_P for LAC for the ten observations 1940-53 is $r_A = -1.53 + .78r_P$ $\bar{r}^2 = .73$.

would not explain why "pure" productivity growth was negligible in recent years. It seems appropriate to rule out oversaving as the explanation of the behavior of r_A in LAC.

III

This final section seeks to explain the behavior of r_A in terms of the development strategies and policies followed in LAC and in terms of the productivity of capital discussed in Part I above.

The central hypothesis defended here is that the development policies which created the profitable opportunities for investment (especially in the years after 1955) also created conditions that had two other effects: made it extremely difficult, for technological reasons, to achieve a high r_A and secondly, created an economic environment in which the entrepreneur had little incentive to search for productivity-increasing improvements. During the war years, on the other hand, both the technological and the incentive factors worked in favor of a relatively high r_A . The war period provided protection without distortion, while the import substitution approach to development has provided protection but has also imposed severe distortions, and it is these distortions that create the two effects just enumerated. The general evidence supporting the hypothesis is easily stated, but a detailed investigation would require a country-by-country survey. Such a survey is not possible here, but the following points are generally pertinent.

A. *The War Period*

An examination of the 1940-45 period is especially helpful. In this period the rate of growth of capital was much lower than in later periods due to the curtailment of imported capital goods. During the war there existed a strong and obvious demand in both the internal markets and for exports. Consequently, there was great incentive to increase output among all firms in the five Latin American countries, but virtually no ability to obtain new plant and equipment, spare parts, and replacements. Similarly, the flow of many raw material imports was irregular and unpredictable. With foreign supplies of capital equipment difficult to obtain, firms (to capitalize on the favorable market) were forced to find ways to use their existing capital stock with increasing effectiveness. Improvisation and adaptation of existing equipment were common, and one can find many examples of ingeniously and indigenously devised machines producing various items for household and business use [14a] [20]. The war then not only provided "protection" from foreign competition, but also helped to create an environment within which entrepreneurs had incentives to use available resources with increasing effectiveness. The innovative activity observed

in this period involved not only changes in technique to fit the domestic supply of inputs complementary to capital (labor of various skills and quality, raw material imports, and managerial ability), but also included adaptation of techniques to fit market size and of product to fit market demand.

Although the growth of the labor supply was not thwarted the way capital imports were, the wartime isolation had some effect on labor's use. One of the consequences of the efforts to use physical capital more effectively was the adaptation of the tools and equipment to fit the quality of the available labor. Thus the form of the capital became increasingly appropriate for the workers and thereby *their* productivity tended to rise. We conclude then that strong and obvious demand in a situation where availability of new, imported capital was *recognized* to be almost nil is part of the conditions necessary for entrepreneurs to achieve a relatively high rate of increase in productivity. The important thing to note is that the relatively high r_A in this period was accomplished without capital goods imports. We cannot say that it was generally high export earnings permitting a high level of imports that were responsible for a strong showing of r_A .⁷

B. *The Post-1955 Period*

The wartime experience is most clearly contrasted with the post-1955 period. In this latter period a large sector of the domestic economy of LAC again was isolated from foreign competition, but this time by high tariffs and other forms of import impediments rather than by a world war. As the war had created profitable opportunities for increased output of a wide range of manufactured goods, so also did the import substitution strategies of development followed in LAC create opportunities in the post-1955 years. The response to these opportunities that produced the high r_A in 1940-45 seemed to be absent in the later period. The question now is why?

In comparing the later periods with the war period, three characteristics seem especially relevant.

1. The most obvious difference has to do with the supply conditions of imported capital goods. During the war, as already noted, they were virtually unavailable. After 1955 the almost universal and continuous overvaluation of the local currencies made capital imports cheap, relative to domestic inputs. Entrepreneurs not only knew that foreign-made capital was available, but had a major incentive to use

⁷ Strong external demand did not mean in all cases higher exports or more favorable terms of trade than prevailed later. Exports as well as imports were handicapped by transportation. The rate of growth of exports was much higher than later and all producers knew a demand existed for all that they could produce.

it intensively in their production. But the regression analysis of Part II gave us no reason to think that capital formation carried the sources of productivity growth. On the labor side, a variety of social welfare policies (minimum wage rates, paid vacations, factory infirmaries) instituted (or enforced) in the 'fifties added to the cost of employing labor.⁸ It is also probably correct to say that wage earners were better able to protect themselves from inflation in the 'fifties and 'sixties than they were in the 'forties. There is no doubt then that prevailing market prices for capital and labor reflected the real factor supply situation much more accurately in the war period than they did in the later periods. In a very general sense, it seems correct to say that the capital equipment imported from and designed for capital rich, labor scarce countries was more nearly appropriate (for the individual producer) in its unmodified state than was the case in the war years. In this sense, the entrepreneurs had less incentive to modify and adapt (and thereby raise the productivity of) their imported capital than they had in the earlier period. Indeed, their incentives worked in the opposite direction: they were encouraged to meet any demands for increased output by acquiring more capital from abroad. It is important to emphasize that the misleading factor prices arose largely from specific policy measures, not from some endemic characteristic of the economy. Similarly, note should be taken of the fact that "entrepreneurial response" did not change, i.e., entrepreneurs reacted to market signals in both periods with considerable rationality.

2. Another difference between the two periods has to do with the composition of output. Although industrialization was underway in LAC before the war, it was not until the 'fifties that an explicit import substitution policy of industrialization became effective. In the present context the most relevant characteristic of this policy is the haphazard and *ad hoc* manner in which trade barriers have been applied. There is no evidence of a careful review leading to the protection of this or that activity on the basis of expected productivity growth or infant industry considerations. Rather import limitations have been in response to immediate balance-of-payments difficulties or to pressures from specific interests wishing to expand into new activities.⁹ The result of such a policy has been not only a reduction in current income in accordance with the conventional free trade model. More importantly, an industrial

⁸ Wage rates in most Latin American countries have risen considerably less than total labor costs since 1950. The owner of a very modern textile mill in Mexico told me that his skilled labor cost the equivalent of one dollar per hour. Of this 53 cents was "fringe benefits." The fringe benefit contribution in the early 'forties—according to the same source—was negligible. See also [18].

⁹ The most thorough documentation of this point is Santiago Macario [11]. See also the papers prepared for the Latin American Symposium on Industrial Development [18] and the analysis of Raul Prebisch [22, especially the first 25 pages].

structure has tended to emerge that is so alien to factor endowments that full utilization of existing capacity came to depend more, not less, on a constant flow of imports.¹⁰

The following sequence is typical and illustrates the way the development pattern affects productivity growth. In response to balance-of-payments difficulties, a certain category of imports (almost always a consumer durable, e.g., fully assembled automobiles for general use) are prohibited. Demand for the product is strong, and a number of plants come into being to exploit this newly created investment opportunity. Initially, almost all produced inputs used are imported. The underpricing of foreign exchange means that it must be rationed by an exchange authority and that demand always exceed supply. With a strong demand for the product, with output dependent on access to foreign exchange, and with the latter allocated among the firms independently of their competitive strength there is no market test for survival, and no need to increase productivity to survive or even to make acceptable profits.

The next step in the sequence occurs when the governments require the new firms to buy a given proportion of their produced inputs from domestic manufacturers. Usually this proportion rises over time; also, the policy usually has its origin in efforts to reduce imports.¹¹ This new requirement has two consequences. It creates a new gap (in the sense of Part I above) in the capital structure, i.e., a new obvious opportunity for profitable investment. Secondly, it forces the originally protected activity to use some inputs which are more costly, of lower quality, and less reliably supplied than was the case when imported inputs were used. The costs of the first activity are thus pushed up.¹² Foreign exchange allocations to the first activity are reduced, in line with the requirement to buy domestically fabricated inputs, and excess demand for foreign exchange by this activity continues. The consequence is also

¹⁰ Several investigations have shown that the income elasticity of demand for imports has risen in recent years in LAC due to a reduction in the average propensity to import while the marginal remained about constant. See especially David Felix's study of Argentina [6].

¹¹ Whether it does in fact have this effect obviously depends on the quantity of imported materials used by the local manufacturers of the produced inputs. See [3] for a model defining the conditions necessary for such a sequence to reduce the import content of the output of the initially protected product.

¹² Unreliable supply conditions have their most obvious consequence on inventory policy. The Chilean automobile assembly activity, for example, follows practically a seasonal pattern of production as the plants must have inputs on hand in sufficient quantity to permit an assembly run. Leland Johnson [8] has a good description of the problems of the Chilean automobile industry. Irregular supply and its consequence for inventory policy is not limited to purchases from domestic manufacturers. Almost all producers import (if permitted) well in excess of current needs on the grounds that they do not know what import policies will be next month. ECLA [13] places heavy emphasis on the lack of continuity and predictability of import policy. Finally, the usually misleadingly low interest rates reduce any incentive the producer might have to find ways to avoid carrying large inventories.

a continuation of the protection of the high-cost producers by allocating to them a share of the foreign exchange essential for production. Strong internal demand pushes product prices up, the overvalued exchange rate (and other investment incentive concessions) keeps capital costs low, and even the relatively inefficient producer makes a comfortable profit. The relatively efficient producer cannot increase his share of the market because he cannot acquire the necessary inputs, especially those that are imported. Meanwhile the most recently protected sectors (supplying the domestically produced inputs to the *initially* protected activity) attract investment.

The final stage arrives when it appears unacceptably costly to try to find ways to reduce the import content of output in the initially protected activity.¹³ Then the process is begun again by levying a prohibition against the importation of another consumer durable. Investment then seems to take place over a wide range of activities as advantage is taken of the gaps in the economy created by import policies, and there is little evidence of investment in response to profit opportunities created by increased efficiency and rising productivity.¹⁴

New investments require capital goods imports, and when foreign exchange sufficient for an acceptably high rate of capital formation and full utilization of existing capacity is not available, one or the other had to give way. As profit rates on invested capital even with considerable underutilization, were acceptable, as new gaps offered opportunities for further investment, and as foreign loans and aid were more easily obtainable for increasing capacity than for using capacity, r_K remained reasonably high in these post-1955 years. Consequently, an economic structure emerged which, when subjected to strong pressure on the demand side, produced rising prices rather than rising productivity. This final result is of course exactly opposite to that described in the war period.

3. A final element in the picture has to do with the extent and nature of external competition prevailing during the war with that prevailing in the later periods. During the war direct competition between LAC and AG was virtually nil. With demand strong in AG, Latin American countries were in effect competing among themselves for the AG market as well as for their domestic markets. With the end of the war and reconstruction and the beginning of the emphasis on industrialization in LAC, competition with AG was direct. That producers in LAC tend not to respond to direct competition with AG, while they do seem to respond to competitive threats with producers in other less developed

¹³ Or when domestic demand at the going price is satisfied.

¹⁴ ECLA recognizes this point in [18, p. 53] where "development in depth" is contrasted with "development in breadth." Similarly David Felix [6] speaks of a "premature widening" of the market in Argentina.

countries is reasonably clear. The most appealing explanation of this is simply the initial difference in costs. A Chilean manufacturer of refrigerators may seek ways to undersell a Colombian manufacturer because their costs are at least comparable. The Chilean manufacturer would, however, fold up immediately if he were confronted with producers who could offer refrigerators at less than one-third his costs. Protection was thus believed necessary, and competition in large segments of the individual economies became almost nonexistent.

IV

A short summary statement of the main conclusions may be useful. Evidence has been presented that is consistent with the hypothesis that in neither LAC nor AG are inputs (capital and labor) the carrier of productivity growth. Also evidence has been presented which leads to the conclusion that the observed changes in the rate of growth of productivity in LAC could, in the periods after 1945, be accounted for largely by the ability of LAC to utilize all its available resources. From this conclusion and on the basis of comparison with data for AG, the further important conclusion was reached that "pure" productivity growth has been virtually zero in LAC in the past decade or so. The explanation of this last result rested on three points: (1) a growing inappropriateness of the input mix of production due in large part to the continued undervaluation of foreign exchange to interest and wage rate policies, etc.; (2) a growing inappropriateness of the composition of output in the sense that productive activity was not based on cost or potential cost considerations, but rather has evolved in response to the incentives generated by protectionist policies made up largely to meet balance-of-payments crises; (3) a decline in competition.

A more general conclusion is also suggested by the argument of this paper. Recently a number of studies have shown that the increased output to be expected if all deviations from a conventionally defined optimal allocation of resources were eliminated is exceedingly small. Therefore concern with the traditional allocation questions is of little interest in understanding development.¹⁵ If, however, productivity growth is an important element in development, and if it is handicapped by severe misallocations, then the solving of the allocation problem in a satisfactory way is a crucial element in development policy.

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TECHNOLOGICAL CHANGE AND THE DEMAND FOR COMPUTERS

By GREGORY C. CHOW*

To what extent has the use of electronic computers grown in the United States since 1955? What can explain the rate of growth?

We propose to measure different computers by their monthly rentals which would have been charged if the models had been introduced in the year 1960. This method of measurement requires first establishing a relationship between rental and the computer's basic characteristics for 1960 models, and then applying the relationship to estimate what each computer would have cost if it had been introduced in 1960. By this method, the stock of general-purpose, digital computers has grown from an estimated 1960 rental of about \$370 thousand per month at the end of 1954 to about \$194 million at the end of 1965. An average annual rate of growth of 78 per cent would have accomplished this.

To explain this tremendous growth, one has to isolate the natural growth which would have prevailed had there been no technological change, and the growth induced by technological change. In other words, two elements account for the increase in the use of computers. First, it takes time for a new product to reach an equilibrium level even without quality change. Second, in the meantime, the quality of the product is improving, so that the equilibrium level is being continuously raised. An explanation ought to combine these two elements and assess their roles in the growth process.

For the natural growth, two differential equations have been popular. They are the differential equations generating the Gompertz curve and the logistic curve [3, pp. 302-18]. For reasons of empirical validity as well as analytical convenience, we have chosen the former, but a test of the latter will also be presented.

A simple way of incorporating the effect of technological change into the natural growth process is to assume that the equilibrium level, which the growth curve approaches, is a function of price, given quality. A price index can be constructed once the quantity of each computer is established, by its hypothetical 1960 rental as previously indicated.

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An average of the ratios of rentals to quantities, for models introduced in a period, would serve as a price index for the period.

Our price index, deflated by the GNP deflator, shows an average rate of reduction of 20 per cent per year between 1954 and 1965. If the price elasticity of demand for equilibrium stock of computers is 1.3, say, price reduction alone would account for a 34 per cent annual growth, out of a total of 78 per cent observed.

The remainder of this paper develops more fully the ideas sketched above, provides measurements of the use of computers and of their prices, and attempts to explain the growth of computers in the United States between 1955 and 1965. The theory will be formulated in Section I. Section II describes the construction of quantity and price indexes. Section III performs statistical analyses of the time series data and draws some conclusions.

I. *Theory*

To describe the process of natural growth of a new product, it is often assumed that the rate of growth depends on two factors. First, the quantity of existing stock y , asserts a positive influence, partly rationalized by the idea that the more the product has been accepted, the more prospective buyers have come in contact with and learned about the product. Second, the difference or ratio between the equilibrium level y^* , ultimately to be reached, and the existing level y , also asserts a positive influence. Although the existing stock y , has a positive effect on the rate of growth as it shows the extent of the awareness of the product, the closer it comes to the equilibrium level y^* , the smaller will be the number of prospective buyers remaining.

One formulation of the growth process incorporating these two factors is

$$(1) \quad \frac{dy}{dt} = \alpha y (\log y^* - \log y)$$

or

$$\frac{d \log y}{dt} = \alpha (\log y^* - \log y)$$

The solution of the differential equation (1) yields the Gompertz curve. Another formulation is

$$(2) \quad \frac{dy}{dt} = \gamma y (y^* - y)$$

or

$$\frac{d \log y}{dt} = \gamma(y^* - y)$$

the solution of which is the logistic curve. Both formulations assume that the two factors affecting the rate of growth enter multiplicatively. Hence they become hypotheses explaining the percentage rate of growth. According to (1), the percentage rate of growth is a linear decreasing function of $\log y$.¹ According to (2), the percentage growth is a linear decreasing function of y . Thus, in a later stage of the growth process, or for a large value of y , any given increment in y will dampen the rate of growth more for the logistic hypothesis than for the Gompertz hypothesis. In other words, in the later stages, the Gompertz rate of growth is more nearly constant than the logistic rate of growth.

A further observation on the difference between these two growth curves is in terms of the stage at which the maximum rate of growth is reached. Setting the derivative of (1) with respect to y equal to zero, one finds $y = e^{-1}y^* = .37 y^*$. Setting the derivative of (2) equal to zero, one finds $y = .5 y^*$. Hence, the maximum rate of growth is reached when the stock is about 37 per cent of its equilibrium level by the Gompertz curve, and when the stock is 50 per cent of equilibrium by the logistic curve. Since the rate of growth given by equation (2) is the same for y equal to $.5 y^* + k$ or to $.5 y^* - k$, the logistic curve is symmetrical. On the other hand, the Gompertz curve attains its maximum rate of growth at an earlier stage, and maintains a more nearly constant rate of growth later on, than the logistic curve.

There are two a priori reasons for choosing the Gompertz differential equation (1). According to a study of the growth of television sets in the United Kingdom by A. D. Bain [1], the growth curve is not symmetric as the logistic would imply, but the growth rate is higher at an early stage and declines gradually in later stages. The Gompertz curve possesses this characteristic; yet Bain has rejected it because it is too rigid, implying the maximum rate of growth to be at 37 per cent of equilibrium. We have no reason to reject the Gompertz in favor of the equally rigid logistic curve. In fact, rigidity is the cost we have to pay in choosing an equation with only one parameter.

The second reason for preferring the Gompertz differential equation is analytical convenience. We will approximate the derivative of $\log y$ by its difference $\log y_t - \log y_{t-1}$, and the existing stock y by y_{t-1} . (1) will then become

$$(3) \quad \log y_t - \log y_{t-1} = \alpha(\log y^* - \log y_{t-1}).$$

¹ Log always refers to natural logarithm in this paper.

If the equilibrium stock y^* is assumed to be a function of certain variables with constant elasticities, the right-hand side of (3) will be a linear function of the logarithms of these variables and of $\log y_{t-1}$. Unless we assume y^* to be a linear function of these variables, a similar transformation of (2) would be nonlinear and more difficult to analyze statistically.

Equation (3), with $\log y^*$ replaced by a linear function of the logarithms of certain variables affecting the equilibrium level, is familiar in econometrics. One wonders why this model has scarcely been applied, if at all, to the study of a new product. In another occasion [2], I pointed out the need to integrate the growth process with the comparative static theory of demand in the study of a new product. Why not let this model provide such an integration?

By the comparative static theory of the demand for input, two of the most important determinants are relative price p_t and the output x_t of the firms employing the input. In the case of a durable good, the quantity of the input used can be measured by the stock. Assuming constant elasticities, one can determine the equilibrium stock y^* by

$$(4) \quad \log y_t^* = \beta_0 - \beta_1 \log p_t + \beta_2 \log x_t$$

On substituting (4) for $\log y^*$ in (3), one obtains

$$(5) \quad \log y_t - \log y_{t-1} = \alpha\beta_0 - \alpha\beta_1 \log p_t + \alpha\beta_2 \log x_t - \alpha \log y_{t-1}$$

For any new product that one can think of, its price always declined, and the output of the industries employing the product in case it is an input, or the income of the consumers in case of a consumer good, always increased. It is only by allowing for the price and output effects explicitly that one can isolate the process of natural growth.

II. *Measurement of Quantity and Price of Computers*

In this study, we assume that all general-purpose digital computers can be grouped into one commodity. The quantity of a computer is measured by an estimate of what its monthly rental would have been if it had been introduced in 1960. To form such an estimate, we first establish a relationship between rental and the computer's basic characteristics for models introduced in 1960.

Three characteristics are chosen to establish the required relationship: multiplication time, memory size, and access time. Multiplication time is the average time required to get and complete the multiplication instruction, in microseconds. It should have a negative effect on rental. Addition time was considered instead, and has been found to be a slightly inferior variable. Memory size, in thousands of binary digits or equivalents, is the product of the number of words in the main memory

(in thousands) and the number of binary digits per word, with a decimal digit counting as four, and an octal digit counting as three, binary digits. It should assert a positive influence on rental. Access time is the average time required to retrieve information from the memory. Its effect on rental should be negative.

It is recognized that the hardware of a computer has many other characteristics, and that the characteristics of software support may be important. As far as the omitted characteristics of the hardware are concerned, it is assumed that they are highly correlated with the three included ones so that our estimate of the hypothetical 1960 rental would not be too inaccurate. Insofar as the software has been improving through time, its omission may be more serious. The only defense for this omission is to include software improvement as part of the adjustments allowed in reaching long-run equilibrium in our model.

The relationship between rental and the above-mentioned characteristics for models introduced in the same year is assumed to be linear in the logarithms of all variables. Our quantity index will be independent of the base year (the year chosen to establish this relationship) if the coefficients in the linear function are the same for different years. The intercept is expected to decline through time because the price of computers with the same characteristics has been declining. To examine how similar the coefficients are for different years, we have computed a regression for each year from 1955 to 1965. For each year beginning 1960, the number of new computer models introduced during the year is at least 10. For each year up to 1959, the number of new models is less than 10, and we have included models introduced in the preceding year. The main source of data on rental and computer characteristics is the three surveys by M. H. Weik [5]. A supplementary source is the June issue of *Computers and Automation*, in which a section entitled "Buyers Guide" contains the relevant information. From October 1962 on, *Computers and Automation* has included a "monthly computer census" in which data on average monthly rental can be found. We have relied on Weik's surveys as much as possible because they provide rental figures corresponding to different memory sizes available for the same computer model whereas *Computers and Automation* has only an "average rental" and a range for memory sizes.

The estimated coefficients (with standard errors in parentheses) of the regressions of rental on the three characteristics, all variables in logarithms, are presented in Table 1. Judging from the 11 cross section regressions for the individual years, memory size has a larger coefficient (in absolute value) than either access time or multiplication time. Three of the coefficients of multiplication time, and one coefficient of access time, have wrong signs, though they are small fractions of their

TABLE 1—RELATIONSHIP BETWEEN RENTAL AND COMPUTER CHARACTERISTICS
(All variables in logarithms)

| Year | Multiplication Time | Memory Size | Access Time | Intercept | R^2 s^2 | Number _i Observations |
|---------|----------------------------|----------------------------|----------------------------|----------------------------|---------------------------|-------------------------------------|
| 1955 | .0108 (.1021) | .4297 (.1530) | -.2895 (.0618) | 2.027 | .947 .0461 | 9 |
| 1956 | -.0505 (.1911) | .4495 (.1624) | -.1991 (.1076) | 1.675 | .890 .2081 | 11 |
| 1957 | .0549 (.1596) | .5651 (.1481) | -.2187 (.0807) | .140 | .941 .1476 | 10 |
| 1958 | -.0171 (.0891) | .5311 (.0697) | -.1617 (.0565) | .542 | .976 .0972 | 10 |
| 1959 | -.2116 (.0366) | .3562 (.0395) | -.1270 (.0337) | 2.489 | .993 .0360 | 10 |
| 1960 | -.1523 (.1009) | .4234 (.1797) | -.1208 (.0783) | 1.205 | .943 .1924 | 10 |
| 1961 | -.0615 (.0729) | -.5507 (.1078) | .1755 (.0519) | .005 | .944 .1159 | 12 |
| 1962 | .0786 (.1411) | .8264 (.1525) | -.2571 (.1167) | -2.404 | .916 .2414 | 11 |
| 1963 | -.0675 (.0690) | .5750 (.0732) | -.0412 (.1228) | -.801 | .951 .0794 | 15 |
| 1964 | -.1486 (.0525) | .6867 (.0754) | .0412 (.1048) | -1.590 | .895 .0978 | 18 |
| 1965 | -.0411 (.0779) | .5778 (.0821) | -.1465 (.0999) | -1.354 | .877 .2518 | 16 |
| 1960-65 | -.0654 (.0284) | .5793 (.0354) | -.1406 (.0293) | -.1045 | .908 .1476 | 82 |
| | -.1398 d_{61} (.1665) | -.4891 d_{62} (.1738) | -.5938 d_{63} (.1661) | -.9248 d_{64} (.1663) | -1.163 d_{65} (.166) | |

standard errors. While the standard errors are large for many coefficients, as a result of the high correlations among the three explanatory variables and of the small sample sizes, the orders of magnitude of the three coefficients do not appear to have changed drastically through time. Note also that the intercept tends to be smaller for later years, but its decline is far from being uniform.

We have decided to pool the data from 1960 on, assuming unchanging coefficients but introducing a dummy variable d_i for each year beginning 1961 to allow for change in the intercept. The estimated regression is

presented in the bottom of Table 1. All three coefficients are highly significant, in spite of the strong correlations among the explanatory variables. The proportional effects of multiplication time, memory size, and access time on rental are about -7 per cent, 58 per cent, and -14 per cent respectively. The coefficient of the dummy variable is decreasing uniformly, reflecting the price decline.

If one were to test the null hypothesis that the three coefficients have remained the same from 1960 to 1965, he would compute the sum of squares A of the residuals in the pooled regression, with 73 degrees of freedom, and the sum of squares B of the residuals in the six individual regressions, with 58 degrees of freedom. Under the null hypothesis, together with the assumption that the residuals in the individual regressions are normally distributed with the same variance, the ratio of $(A-B)/15$ to $B/58$ would be distributed as the F distribution with 15 and 58 degrees of freedom. From our calculations, A is 10.77 and B is 9.04. The F statistic is only .74, strongly supporting the null hypothesis. Therefore, we will use the coefficients obtained by the pooled regression, with $-.1045$ as the intercept for 1960, in the estimation of the hypothetical 1960 rental for each computer model. The quantity index so constructed is independent of the base year chosen, at least from 1960 to 1965; it would hardly be affected even if a year in the 1950s were chosen as the base year, in view of the temporal stability of the coefficients in Table 1.

The computer models included in our quantity index are those listed in the "monthly computer census" of *Computers and Automation*; they are U.S.-made general-purpose digital computers only, and exclude certain very small models that are not internally programmed, notably IBM 604, 607-610, and CPC. The author has used IBM data, where available, on the number of IBM (domestic) installations as of December each year. For installation figures of other models, he has relied on *Computers and Automation* and the *Automatic Data Processing Newsletter*, published by the Diebold Group, Inc.² The quantity index,

² The earliest time period for which even sketchy information on numbers of computer installations can be found is December, 1954. See [6, March 1955, p. 30] [6, Feb. 1955, pp. 23-29] [6, April 1955, p. 41]. Data for approximately November 1955 can be found in the first survey of Weik [5]. Data as of September 1956 are available from the Diebold *Newsletter*, reprinted in [6, Dec. 1956, p. 27]. For February 1958, see the Diebold *Newsletter*, March 3, 1958 or in reprint in [6, May 1958, pp. 8-9]. From 1958 on, December figures can be found in Diebold's *Newsletter*, reprinted in [6], up to 1961. From October 1962 on, *Computers and Automation* has published its own "monthly computer census" giving installation figures for the preceding month, presumably including installations of American-made computers outside the United States while the Diebold figures do not.

We have estimated December figures for 1955, 1956, and 1957 by logarithmic interpolations, performed separately on individual makes, and based on the above-mentioned data. The Diebold figures were used from 1962 on, but the *Computers and Automation* figures are almost identical. We have presented the quantity index using the former source, noting that the latter source has given almost the same results.

in thousands of dollars of estimated 1960 monthly rental, is presented in Table 2.

To measure the price, per fixed quantity, for each computer model, one simply takes the ratio of its rental to its quantity, the latter being measured by its hypothetical 1960 rental as described. To obtain a price index number for a given year, one may average the prices over all models currently introduced. Some weighted average was considered, but it was not easy to decide on the weights. An obvious possibility would be to use the quantity (measured in our sense) of the installations as of December of each year, but this weight would depend too much on the date when the model was first introduced. In principle, one could

TABLE 2—QUANTITY, PRICE AND RELATED DATA ON THE DEMAND FOR COMPUTERS

| Year | Quantity (thousands of 1960 rentals) | $\frac{\text{Quantity}_t}{\text{Quantity}_{t-1}}$ | Absolute Price Index | GNP Deflator | GNP (billions of 1958 dollars) |
|------|---|---|----------------------------|-----------------|---|
| 1954 | 370.26 | | 3.2554 | .896 | |
| 1955 | 991.67 | 2.678 | 2.9610 | .909 | 438.0 |
| 1956 | 2389.9 | 2.410 | 2.5336 | .940 | 446.1 |
| 1957 | 5087.6 | 2.129 | 2.3168 | .975 | 452.5 |
| 1958 | 8362.0 | 1.644 | 2.0342 | 1.000 | 447.3 |
| 1959 | 12549. | 1.501 | 1.5884 | 1.016 | 475.9 |
| 1960 | 19072. | 1.520 | 1.0716 | 1.033 | 487.8 |
| 1961 | 38264. | 2.006 | .9042 | 1.046 | 497.3 |
| 1962 | 64349. | 1.682 | .6873 | 1.057 | 530.0 |
| 1963 | 95815. | 1.489 | .5712 | 1.071 | 550.0 |
| 1964 | 136845. | 1.428 | .4186 | 1.089 | 577.6 |
| 1965 | 194136. | 1.419 | .3416 | 1.109 | 614.4 |

use the quantity as of a fixed period after the first installation, but such quantity data are difficult to obtain. Furthermore, a weighted mean, when the weights are highly uneven, is subject to much larger sampling error than an unweighted mean, something which we can ill afford in view of the sizable errors in estimating price and the small numbers of observations revealed in the individual regressions of Table 1.

Another alternative to the simple arithmetic mean is the geometric mean. In fact, the logarithm of the geometric mean from 1961 on has already been calculated in Table 1; it is the coefficient of the dummy variable for the corresponding year. We have constructed the geometric mean price index for the entire sample period, and have found the results to be virtually identical with those obtained by employing the simple arithmetic mean presented in Table 2. Note, incidentally, that while the geometric mean equals one for the base year 1960, the arithmetic mean is slightly above one.

Now that we have explained and presented the quantity and price indexes, the magnitudes of their errors should be noted. After examining installation figures from different sources, the author would not be too surprised to find an error, in our aggregate quantity index, to be as large as 10 per cent for some year after 1956, or as large as 15 per cent for an earlier year. Of course, if our sources consistently over- or underestimate the installation figures by the same percentage, it does not matter for our analyses of the proportional rate of growth, but we cannot be sure of this.

As far as the price index is concerned, assuming that the error of observation on actual rental is unbiased, one can get an upper bound on its standard error by noting the standard errors of the logarithms of the geometric mean index, i.e., of the coefficients of the dummy variables in the pooled regression of Table 1. A standard error of .17 means that the percentage error of the geometric mean is approximately 18 per cent. This estimate of the standard error is too high for our purpose, because it has included the variations of the price-quantity ratios for the same year due to different firms' pricing policies. Conceptually our index is the (population) mean of the prices (per fixed quantity) of the models introduced in a given year. The variations in these prices, alone, should not produce an error in our index.

III. *Statistical Analyses*

With the quantity and price indexes at our disposal, we would like to raise three empirical questions. First, without explicitly accounting for the change in equilibrium stock, is the growth pattern closer to the Gompertz hypothesis or to the logistic hypothesis? Second, do the data support the notion of a moving equilibrium, due mainly to the drastic price reduction, and, if so, how large is the price elasticity of equilibrium demand? Third, can we measure the output effect on equilibrium stock?

The answer to the first question is obvious from the data. A plot of the logarithm of the ratio y_t/y_{t-1} (a ratio exhibited in Table 2) against $\log y_{t-1}$ is much closer to a linear function, as the Gompertz hypothesis implies, than against y_{t-1} , as the logistic hypothesis would imply. If one were to insist on a linear relation between $\log (y_t/y_{t-1})$ and y_{t-1} , he would have to stop in 1959. Unless one is willing to use two models, one for initial growth and another based on the theory of comparative statics, there seems to be no way of maintaining the logistic hypothesis. But it is precisely the purpose of this study to integrate both elements into one model.

To answer the second question, pursuing the Gompertz hypothesis further, we examine the partial effects of price on $\log (y_t/y_{t-1})$, given $\log y_{t-1}$. This amounts to performing a regression of $\log (y_t/y_{t-1})$ on

both $\log p_t$ and $\log y_{t-1}$. The price variable p_t used is the relative price, obtained by deflating our absolute price index by the GNP deflator.³ The result of the regression, using 11 annual observations from 1955 to 1965, is

$$(6) \quad \log (y_t/y_{t-1}) = - .3637 \log p_t - .2526 \log y_{t-1} + 2.950$$

(.1726) (.0739)

$$R^2 = .834$$

$$s^2 = .0104$$

The coefficient of $\log p$ is negative, as expected. The ratio of the estimated coefficient to its standard error is -2.107 . The null hypothesis of no price effect, against the one-sided alternative hypothesis of a negative price effect, would be rejected at the 5 per cent level where the critical value of the t ratio is -1.860 , but would not be rejected at the 2.5 per cent level, where the critical t value is -2.306 . Since there are only 8 degrees of freedom, the power of the t test is low. Under this circumstance, the rejection of the null hypothesis tends to give strong support for the negative price effect. The Durbin-Watson statistic is 1.77, consistent with no serial correlation in the regression residuals.⁴

A point estimate of the price elasticity of demand for equilibrium stock can be obtained as the ratio of the two coefficients in equation (6). It is 1.44. Partly because of our small sample size and the high correlation between the two explanatory variables, the standard error of the price coefficient is large, being almost half of the coefficient itself. However, the ratio between the two coefficients is subject to smaller error than the former coefficient by itself, because the two coefficients are positively correlated. A 60 per cent confidence interval for the ratio is $1.379 \pm .274$, and an 80 per cent confidence interval is $1.326 \pm .382$.⁵ The parameter α of the Gompertz difference equation is about .253, with a standard error of .074.

In Figure 1, $\log (y_t/y_{t-1})$ is plotted against time t , together with its

³ The GNP deflator is found in [7, Aug. 1965, p. 53, and July 1966, p. 38].

⁴ Since $\log y_{t-1}$ is a regressor, the power of the Durbin-Watson test is low; one could get a D-W statistic close to 2 even if there were positive serial correlation.

⁵ To obtain an interval estimate of the ratio between two regression coefficients b_1 and b_2 , form the linear combination $b_1 - zb_2$, where z is yet to be determined. This linear combination has a variance equal to $\text{var } b_1 + z^2 \text{ var } b_2 - 2z \text{ cov}(b_1, b_2)$, which, as estimated by our data, is $.02979 + .005463z^2 - .02484z$. If z is the true ratio, this linear combination would have mean zero. The probability is 60 per cent that the sample value of this linear combination is within .889 times its standard deviation, where .889 is obtained from the t table. Thus

$$(.3637 - .2526z)^2 < (.889)^2 (.02979 + .005463z^2 - .02484z)$$

with probability .60. Treating the above as a quadratic equation of z , we find $z = 1.379 \pm .274$. Replacing .889 by 1.397, we obtain an 80 per cent confidence interval for z , namely, $1.326 \pm .382$. This method is described in Fisher [4, pp. 143-44].

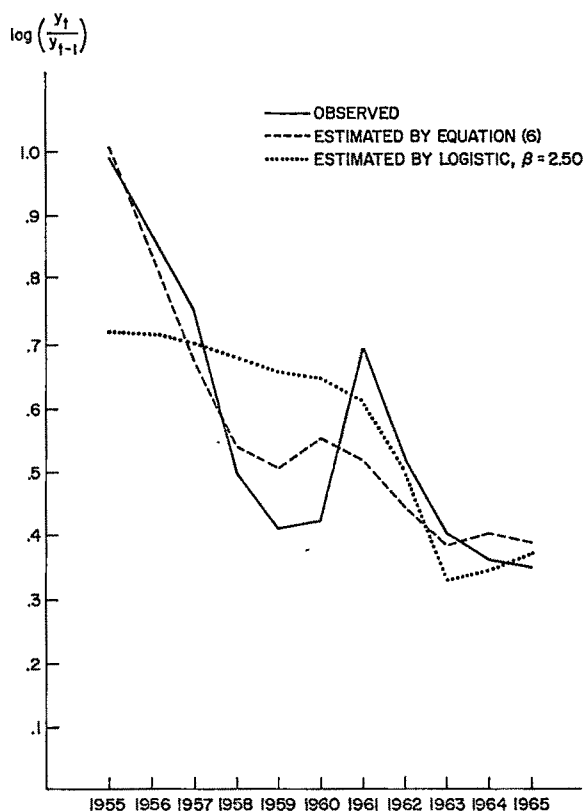


FIGURE 1

estimated value by equation (6). The general pattern of the rate of growth appears to have been explained fairly satisfactorily. However, the model underestimates, by a fairly sizable margin, the local peak in the rate of growth that occurred in 1961. That was the year when the "first-generation" computers using vacuum tubes were changed to the "second-generation" solid-state computers, a transition which had begun in 1960. Possibly, the excess of orders over actual deliveries of the new IBM 360 systems in 1965 could account for the difference between the observed and the estimated rate of growth in that year.

Just to be sure that the logistic difference equation cannot be salvaged by allowing the price effect on the equilibrium level, we have regressed $\log(y_t/y_{t-1})$ on $p_t^{-\beta}$ and y_{t-1} , assuming a constant elasticity of equilibrium demand. The regression, being not linear in p_t , can be estimated by trying different values for β to maximize the multiple correlation, or to minimize the standard error of the regression. Table 3 shows the regression results for different values of β . For small values of β , the

TABLE 3—REGRESSIONS BASED ON LOGISTIC GROWTH FOR DIFFERENT VALUES OF PRICE ELASTICITIES

| Elasticity β | Coefficient of $p_t^{-\beta}$ | Coefficient of $y_{t-1}(10^5)$ | R^2 | Durbin-Watson Statistic |
|-----------------------|----------------------------------|-----------------------------------|-------|----------------------------|
| .50 | -.7103 (.3999) | .3244 (.3736) | .563 | 1.000 |
| 1.00 | -.4259 (.3339) | .6571 (.7212) | .505 | .990 |
| 1.50 | -.0588 (.4144) | -.0669 (1.698) | .392 | .743 |
| 2.00 | .1498 (.1361) | 1.408 (1.008) | .471 | .726 |
| 2.50 | .0544 (.0459) | -1.012 (.608) | .482 | .739 |
| 3.00 | .0220 (.0193) | -.8078 (.4562) | .476 | .737 |
| 4.00 | .0047 (.0044) | -.6318 (.3348) | .464 | .730 |
| -1.00 | .1825 (.0638) | -.00087 (.1433) | .699 | 1.193 |

coefficients of both $p_t^{-\beta}$ and y_{t-1} have wrong signs, but the multiple correlation is higher than for larger values of β . If we ignore the calculations for β below 1, the best R^2 is obtained when β is approximately 2.50, in which case both coefficients have the correct signs, but the goodness of fit is much worse than equation (6) based on the Gompertz hypothesis. If one assumes the equilibrium stock to be a linear function of price, he will improve the R^2 , but find a price effect in the wrong direction and the stock effect absent, as shown in the last row of Table 3. The most acceptable fit based on the logistic model, or the least unacceptable fit, is for a constant price elasticity of 2.50. Its residuals are much larger than those of the Gompertz model (6), and it fails to trace the pattern of the rate of growth, as the dotted line in Figure 1 shows.

The remaining task is to measure the output effect on the equilibrium demand for computers. Practically all industries employ the services of some computers, although in varying degrees.⁶ Ideally, one should weigh the outputs of different industries by the corresponding output elasticities of demand. However, a simple aggregate of output, such as the Gross National Product, would suffice if the relative contributions of

⁶ For an estimate of the distribution of the value of computer installations by industry, see the market report appearing in [6, Nov. 1966, p. 16].

different industries remain approximately the same. One could even tolerate the relative gains of some industries, at the expense of others, if the former, as a group, have the same output elasticity as the latter. If the gaining industries had higher output elasticities than the losing industries, the growth of GNP would be smaller than the growth of the weighted output.

Adding GNP, in billions of 1958 dollars,⁷ as an additional variable in equation (6), we obtain

$$(7) \quad \log (y_t/y_{t-1}) = -.5924 \log p_t - 1.160 \log \text{GNP} - .2828 \log y_{t-1} + 10.45$$

(.4056) (1.845) (.0906)

with an R^2 of .843. The coefficient of $\log \text{GNP}$ has the wrong sign, but is very insignificant. The inclusion of this additional variable, which is highly correlated with price, also affects our estimate of the price coefficient. From the data, we have failed to measure the separate effect of output. Furthermore, there is the possibility that the omission of an output variable in equation (6) might seriously bias our estimation of the price effect.

To examine this possibility, we assume that the output coefficient equals two times the negative of the price coefficient, and employ the combined variable $\log p - 2 \log \text{GNP}$ to replace $\log p$ in the regression. The result is

$$(8) \quad \log (y_t/y_{t-1}) = -.2448 (\log p_t - 2 \log \text{GNP}_t) - .2306 \log y_{t-1} - .0306$$

(.1260) (.0689)

$$R^2 = .824$$

$$s^2 = .0110$$

This regression explains the dependent variable almost as well as equation (6). The ratio of the coefficients is 1.062, a little smaller than in equation (6). Since output has been increasing, while price has been declining, part of the growth attributed to the price effect in equation (6) may be due to the output effect. In other words, equation (6) may overestimate the price effect. However, the rate of price reduction has been much greater than the rate of output increase. Even if the output effect were twice as big as the price effect, as we have assumed in the calculation, it could only take away a small fraction of the effect attributed to price in equation (6). Thus, while we have not been able to measure the output elasticity, this failure would not seriously affect our estimation of the price elasticity even if the output effect were present.

Technological change in the computer industry is a very complicated phenomenon. We have not dealt with its causes. Neither have we dealt

⁷ GNP in billions of 1958 dollars is found in [7, Aug. 1965, p. 27, and July 1966, p. 11].

with many interesting aspects of its consequences, such as the adjustments of the management and production processes. Only one aspect of its consequences, namely, the effect on demand, has been studied in this paper. This limited objective is achieved by employing a univariate measurement of technological change from the viewpoint of demand, namely, the measurement of price reduction. Even with this limited objective, it has been necessary to measure the quantity of computers in constant dollars, to construct a price index, and to formulate a theory of demand applicable to a new product while preserving the elements of comparative statics.

As matters of historical facts, we have found the stock of computers growing by more than 100 per cent per year between 1954 and 1957, and by more than 40 per cent per year between 1962 and 1965, averaging to about 78 per cent from 1954 to 1965. During the same period the relative price of computers has declined by 20 per cent per year, on the average. On the theoretical side, we have found that the Gompertz difference equation, modified by a moving equilibrium due to the comparative statics of price change, can explain the rate of growth for this new commodity, and is much better than the logistic difference equation. The price elasticity of equilibrium demand is somewhat over unity, the growth parameter of the Gompertz difference equation is about one quarter, but no output elasticity has been successfully estimated.

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INTEREST RATES AND THE DEMAND FOR CONSUMER DURABLE GOODS

By MICHAEL J. HAMBURGER*

The purpose of this paper is to examine the effects of monetary variables on the demand for consumer durable goods. Since the publication of Keynes' *General Theory*, it has generally been assumed that consumption is insensitive to interest rates. Consequently, the only monetary variable that has been included in the consumption function with any regularity has been liquid assets. The view taken here is that monetary variables have a significant effect on consumer purchases of durable goods and that the most appropriate measures of these variables are interest rates. Other measures which will be considered are the monetary base, the aggregate money supply, the rate of change in the money supply, and the consumer stock of liquid assets. If the results support the hypothesis, they would (1) cast considerable doubt on the widely accepted notion that consumers do not respond to changes in interest rates,¹ and (2) provide an alternative to both the Keynesian and the Chicago views concerning the channels through which monetary policy operates. Contrary to the Keynesian income-expenditure approach, they would imply that monetary policy has a direct effect on consumer behavior. They would also suggest, however, that for the consumer sector of the economy the interest rate effects of monetary policy are more important than the "monetary" effects.

Section I presents a model to explain the quarterly movements in two major components of consumer durable goods: automobiles and parts, and all others. (The most important items included in the latter are furniture and household equipment.) In Section II the model is subjected to empirical test. The data used are quarterly observations of the variables for the period 1953-64. Section III tests for differences in the

* The author is economist, Federal Reserve Bank of New York. While he alone takes responsibility for this paper, he acknowledges debt to David M. Jones for valuable discussion and to Maurice Mann for his encouragement and helpful comments. The suggestions made by Richard G. Davis and Allan H. Meltzer and the excellent assistance of Cynthia M. Latta have contributed to the paper. Useful comments on an earlier draft were received from members of the Monetary Workshop, Columbia University, and from members of the Federal Reserve System Committee on Financial Analysis.

¹ Despite its general acceptance there is very little empirical evidence to confirm the proposition that consumption is independent of interest rates. The only studies that have come to my attention in which interest rates are considered as explanatory variables in the consumption function are Liu [27] and deLeeuw [8]. Liu's results (which are not shown in his article) tend to support the received theory; deLeeuw's suggest that interest rates are significant and that they operate with a long lag. Two recent studies in which interest rates are conspicuously absent from the consumption function are Goldfeld [17] and Evans and Kisselgoff [10].

results when alternative formulations of the equations are employed. The implications of the results for monetary and macroeconomic theory are discussed in the concluding section.

I. *Consumer Demand for Durable Goods*

Our model of the demand for consumer durable goods may be expressed as follows. First, let the desired aggregate stock of any given physical asset (\bar{A}) be a linear function of Y , aggregate income valued in constant prices, r , a vector of yields on financial assets and liabilities, and P , the price of the asset relative to the prices of all other goods and services purchased by consumers. That is,

$$(1) \quad \bar{A} = a + bY + cr + dP$$

where the parameters b , c , and d denote the long-run effects of the independent variables on the desired stock. Except for the inclusion of r , this formulation of the demand function for consumer durable goods is similar to those that have been used elsewhere.²

Second, purchases of the asset during period t (X_t) are defined as the sum of consumption (or depreciation, D_t) and net investment (ΔA_t):

$$(2) \quad X_t \equiv D_t + \Delta A_t$$

where it is assumed that

$$(3) \quad \Delta A_t = \theta(\bar{A}_t - A_{t-1}); \quad 0 \leq \theta \leq 1,$$

and

$$(4) \quad D_t = e_1 X_t + e_2 A_{t-1}.$$

A_t denotes the existing stock of the asset at the end of the t^{th} period. Equation (3) is a standard stock adjustment relationship where θ is the reaction coefficient. It measures the percentage of the adjustment that consumers make towards equating their actual and desired asset balances during any time period. It is assumed that (a) the response patterns associated with each of the explanatory variables may be described by a simple decay function, and (b) the parameters associated with these functions are identical.

² See, for example, Atkinson [3], Burstein [4], Chow [6], Fromm and Klein [16], Roos and von Szeliski [31], and Stone and Rowe [35]. Three variables that are not introduced explicitly into the model are the expected rate of change in the price level, population and nonhuman wealth. The exclusion of wealth is one of the more important limitations of this study. Its omission is almost certain to introduce some bias into the estimates of the income coefficients. The variable is not included because of the difficulties involved in measurement. For a discussion of these and other problems, see Hamburger [18] and Johnson [19]. It may also be mentioned that a more appropriate measure than the relative price variable used here would be a vector of the implicit yields on consumer durables. The typical element in the vector would be the price of the services of a unit of a durable good as a ratio to the price of a unit of the good. At the present time these variables cannot be measured—and even conceptually the absence of a market for the services of many consumer durables presents serious difficulties.

The model described above may be modified slightly by allowing the adjustment process to begin at different time periods for the various independent variables. This is done by introducing lagged values of the variables on the right-hand side of equation (1), i.e.,

$$\bar{A}_t = a + bY_{t-n_1} + cY_{t-n_2} + dP_{t-n_3}$$

or alternatively

$$(1') \quad \bar{A}_t = a + bY_{t^*} + cY_{t^*} + dP_{t^*}$$

where the n 's indicate the lags associated with the independent variables and t^* is used as a shorthand device for $(t-n_1)$, $(t-n_2)$ and $(t-n_3)$. The values of t^* will generally not be the same. For nonzero values of n_1 , n_2 , and n_3 , equations (1') and (3) imply that the adjustment of expenditures to changes in the explanatory variables is negligible for some initial time period, then rises abruptly to its maximum, and declines geometrically thereafter. Of course, actual expenditures, even if lagged, would probably not start off at their peak response but are more likely to build up gradually to the peak and then taper off. Hence, the adjustment pattern used here may be overly restrictive. However, in view of our interest in assessing the effects of monetary variables on the demand for consumer durable goods this pattern seems to provide a reasonable first approximation.³

Equation (4) suggests that depreciation is a constant percentage, e_2 , of the existing stock plus some percentage, e_1 , of current purchases. At least two views may be identified concerning the relative values of e_1 and e_2 . Stone and Rowe [35] assume that (a) the rate of depreciation for any particular asset is constant throughout its life, and (b) current purchases are distributed evenly during the period of observation. From these assumptions they derive the equation

$$(5) \quad e_1^{SR} = 1 + \frac{e_2}{\ln(1 - e_2)}.$$

Equation (5) implies that, for aggregate analysis, the rate of depreciation on current purchases is approximately one-half the rate on the existing stock.⁴ Other students of consumer durable goods have ob-

³ For more general treatments of the distributed lag model, see Almon [1], Hamburger [18], Jorgenson [21], and Solow [34].

⁴ The following table provides some illustrative values of e_1^{SR} and the annual rate of depreciation on the existing stock (e_2^A) for various values of e_2 .

| e_2 | e_2^A | e_1^{SR} |
|-------|---------|------------|
| .04 | .1507 | .0203 |
| .08 | .2836 | .0407 |
| .12 | .4003 | .0613 |
| .16 | .5021 | .0824 |

The values of e_2^A are derived from the formula $e_2^A = 1 - (1 - e_2)$.⁴

jected to the first of Stone's and Rowe's assumptions. They argue that some durable goods, particularly automobiles, depreciate much faster during the quarter in which they are purchased than they do during subsequent periods. If this is correct, e_1^{SR} would serve as a lower bound for e_1 . One of the desirable features of the approach taken here is that many of the important parameters of the model may be estimated without making any assumptions about the absolute or relative values of e_1 and e_2 . To show this, we proceed with the development of our estimating equation. The objective is to eliminate those variables that are not readily observable— A_t , \bar{A}_t , and D_t .

Substitution of equations (3) and (4) into (2) yields

$$(6) \quad X_t = \theta(\bar{A}_t - A_{t-1}) + e_1 X_t + e_2 A_{t-1}$$

which after some algebraic manipulation becomes

$$(7) \quad X_t = [\theta \bar{A}_t - (\theta - e_2) A_{t-1}] / (1 - e_1).$$

Replacement of \bar{A}_t by its determinants and the use of a transformation suggested by Nerlove [29] yields

$$(8') \quad \begin{aligned} X_t = & \frac{a\theta}{1 - e_1} + \frac{b\theta}{1 - e_1} [Y_{t*} - (1 - e_2) Y_{t*-1}] \\ & + \frac{c\theta}{1 - e_1} [r_{t*} - (1 - e_2) r_{t*-1}] \\ & + \frac{d\theta}{1 - e_1} [P_{t*} - (1 - e_2) P_{t*-1}] + (1 - \theta) X_{t-1} \end{aligned}$$

or, more simply,

$$(8) \quad \begin{aligned} X_t = & a' + b' [Y_{t*} - (1 - e_2) Y_{t*-1}] + c' [r_{t*} - (1 - e_2) r_{t*-1}] \\ & + d' [P_{t*} - (1 - e_2) P_{t*-1}] + (1 - \theta) X_{t-1} + v_t \end{aligned}$$

where the residual term v_t has been added and the relationship between the two sets of constants may be expressed as

$$(9) \quad z' = \frac{z\theta}{1 - e_1}; \quad z = a, b, c, d.$$

The parameters with prime superscripts denote the short-run effects of the exogenous variables on current purchases. According to equation (8), the latter are related to weighted differences of the exogenous variables and to lagged purchases.⁵

⁵ It is well known that the inclusion of the lagged dependent variable on the right-hand side of a regression equation may lead to biases in the parameter estimates if the residuals are serially correlated. Moreover, biases may arise as a result of the treatment of Y , P , and r as exogenous variables. It seems unlikely, however, that corrections for these biases would alter any of the major conclusions reached in this paper.

As indicated above, the model summarized by equation (8) embodies a slightly modified version of the standard "stock adjustment" assumptions. Two other differences between the specification used here and the usual formulation of the stock adjustment equations for durable goods are worth noting. First, equation (8) explicitly recognizes the rate of depreciation on current purchases, which is generally ignored in other treatments (see Evans and Kisselgoff [10], Burstein [4], and Chow [6]). Second, the use of weighted differences of the explanatory variables as opposed to the levels of the variables tends to reduce the amount of multicollinearity which might otherwise be present.

If e_2 and the lags associated with the independent variables were known, or could be assumed, least squares estimates of a' , b' , c' , d' , and θ [the coefficients in equation (8)] could be obtained. However, these parameters are not known. Consequently, an iterative estimation procedure is employed. It can be shown (Nerlove [29]) that if the residuals in equation (8) are normally and independently distributed, the technique yields maximum likelihood estimates for each of the coefficients that can be separately identified.⁶ The procedure is as follows: (a) Compute the weighted differences of the exogeneous variables for each of a number of values of e_2 and the lags associated with the independent variables. (b) For each of these values compute the least squares regression of X_t on the weighted differences of the independent variables and X_{t-1} . (c) Find the maximum of R^2 with respect to e_2 and the lags associated with the independent variables. The values of these parameters at the maximum and the values of the other coefficients, given the values of the former, are the maximum likelihood estimates.

To complete the development of the model, it is necessary to provide operational definitions of the variables. These are given in the appendix. Here we consider some of the characteristics of the interest rate variables employed in the study. The interest rate vector r is defined to include the following components: r_{SV} , the yield on savings accounts (a weighted average of the rates paid on savings and loan shares, mutual savings bank deposits, and time and savings deposits at member banks of the Federal Reserve System); and r_{FS} , the yield on financial securities, measured alternatively as r_{Aaa} , Moody's A_{aa} rate on long-term corporate bonds, and r_{FCP} , the yield on directly placed three-to-six-month finance company paper. The estimates of r_{FS} have two functions. They serve as measures of the yields available on marketable financial assets and, perhaps more important, the rates paid for borrowed funds by the suppliers of consumer credit—that is, as proxy variables for the rates that households pay for consumer credit.

⁶ Since e_1 and the long-run stock coefficients (a , b , c and d) always appear in combination [see equation (8')], it is impossible to obtain separate estimates for them, unless some assumption is made about e_1 .

TABLE 1—PARAMETER ESTIMATES FOR THE AUTOMOBILE AND OTHER DURABLES
EQUATIONS: QUARTERLY DATA, 1953-64
(standard errors in parentheses)

| Dependent Variables | Y | P | r_{Aaa} | r_{SV} | Lagged Dependent Variables | e_2^A | \bar{R}^2_S | R_P |
|---------------------|-------------------------|--------------------------|--------------------------|--------------------------|----------------------------|------------------|----------------|-------|
| C_A | .257 (.047) [1.0] | -.235 (.104) [0.0] | -4.44 (1.27) [4.5] | | .639 (.069) | .28 ^a | .9170 .9249 | .7279 |
| C_{DEA} | .123 (.016) [0.0] | -.443 (.106) [0.0] | -1.08 (0.36) [6.0] | -3.28 (0.95) [0.0] | .838 (.038) | .50 ^a | .9946 .2913 | .8126 |

Note: The data for all variables except interest rates are adjusted for seasonal variation.

The numbers in square brackets denote the lags associated with the independent variables.

The Durbin-Watson statistics for the automobile and other durables equations are 1.25 and 2.09, respectively.

^a The quarterly estimate from which this statistic is derived was estimated within an interval of $\pm .01$.

II. A Test of the Model

The empirical analysis of the demand for consumer durable goods is presented in two parts. This section examines the properties of the model developed above. In Section III we investigate a number of alternatives.

Table 1 reports the results obtained when expenditures on automobiles and parts and on other durable goods (in 1958 dollars) are regressed against lagged expenditures and weighted differences of real disposable personal income, the yield on savings accounts, the yield on *Aaa* rated long-term corporate bonds, and relative price.⁷ The price data are the implicit deflators provided by the Department of Commerce. The symbol e_2^A denotes the estimated annual rate of depreciation on the existing asset stock. The numbers in square brackets are the lags associated with the independent variables, indicating the number of quarters which elapse between changes in the independent variables and the peak adjustments in expenditures. \bar{R}^2 is the squared multiple correlation coefficient adjusted for degrees of freedom; S is the adjusted standard error of the regression; and R_P is the partial correlation between the dependent variable and the "economic" (or exogenous) variables in our model, given the prior effects of the dependent variable lagged one period.⁸

⁷ An alternative measure of income is provided by Friedman's permanent income variable (Friedman [14]). This measure is generally introduced into a regression equation to allow for a lag in the adjustment of the dependent variable in response to fluctuations in income. In the present study this lag is taken into account through the introduction of the lagged value of the dependent variable.

⁸ Partial correlation is defined as the square root of the proportion of residual variation ex-

The statistical fit of the model is obviously close. First, almost all of the regression coefficients have the proper sign and are at least twice their standard errors. The only exception is the coefficient for the yield on savings accounts in the automobile equation, which has the proper sign but is not statistically significant (at the .05 level). Second, the estimates of the partial correlation coefficients (R_P) suggest that after the prior effects of the lagged dependent variable have been taken into account, the remaining variables explain approximately 53 per cent of the variance in the expenditures on automobiles and approximately 66 per cent of the variance in other durables. Third, the independent variables in the two equations are almost identical. Previous observers have not obtained this result. Generally, the only variables that the two equations have in common are income and possibly the lagged dependent variable (or lagged stock).⁹ Thus, the inclusion of interest rates in the analysis seems to yield a more general explanation of consumer investment behavior than that provided by previous treatments.

Finally, the acceptability of the model may be evaluated by examining the annual rates of depreciation on existing asset stocks. For automobiles, the estimated rate of 28 per cent is within the range implied by the behavior of prices in the used car market (see Chow [6] and Friedman [13]). For other durables the estimated rate seems high. However, the difference between this rate and the one for automobiles is not statistically significant. The probability that the "true" rate of depreciation for other durables is the same as the rate on automobiles is greater than 30 per cent.¹⁰

We turn next to the effects of the independent variables. Of particular

plained. Thus,

$$R_P = \left[\frac{S_D^2 - S^2}{S_D^2} \right]^{1/2}$$

where S_D^2 is the sum of squared residuals for the regression of the dependent variable on its lagged value and S^2 is the sum of squared residuals for the regressions shown in Table 1.

One of the principal limitations of the iterative estimation procedure is that the parameter estimates are complicated functions of the observations on the variables; hence, the usual tests for the statistical significance of individual parameters are not strictly appropriate. Nerlove [28] has shown that it is possible to test practically any point hypothesis by means of the likelihood ratio test. This test is used to answer a number of questions concerning the values of e_2 and when there is some question about the statistical significance of the regression coefficients. For the most part, though, we rely on conditional t -statistics for the regression coefficients, given the estimated values of e_2 and the lags associated with the independent variables. These statistics are derived by adjusting the standard errors computed from the equations for the loss in degrees of freedom resulting from the estimation of e_2 . No adjustment is made for the introduction of the lags, since the estimation of these parameters was not pursued intensively.

⁹ See Atkinson [3], Liebenberg, Hirsch and Popkin [26], Fromm and Klein [16], Suits and Sparks [37], and Evans and Kisselgoff [10].

¹⁰ These statements are based on the application of the likelihood ratio test discussed in *n.8*. The probability that the true value of either of the depreciation rates is zero is less than .05.

concern are interest rates. However, before discussing them, it seems appropriate to point out two of the similarities between our income and price results and those reported in other studies. First, the estimates of the new purchase elasticities for automobiles with respect to income and relative price (see Table 2) correspond very closely to those obtained by Roos and von Szeliski [31], Atkinson [3], Chow [6], and Suits [36] (Table 3). The results suggest that income has a greater effect on automobile expenditures than relative price and that the difference between the effects has grown with time.¹¹ Second, changes in income appear to

TABLE 2—NEW PURCHASE ELASTICITIES FOR AUTOMOBILES AND OTHER DURABLES
(evaluated at sample means)

| Dependent Variable | F | P | r_{Aaa} | r_{SV} |
|--------------------|------|-------|-----------|----------|
| Autos | 4.32 | -1.17 | -.85 | |
| Other Durables | 1.65 | -1.81 | -.17 | -.40 |

TABLE 3—SUMMARY OF THE NEW PURCHASE ELASTICITIES FOR AUTOMOBILES
AS REPORTED IN SEVERAL STUDIES^a

| Study | Time Period | Price | Income |
|------------------------------------|---------------------|-------|--------|
| Suits ^b | 1929-41 and 1949-56 | -1.2 | 3.9 |
| Chow ^c | 1920-41 and 1948-53 | -1.2 | 3.0 |
| Atkinson ^d | 1925-40 | -1.4 | 2.5 |
| Roos and Von Szeliski ^e | 1919-37 | -1.5 | 2.5 |

^a Derived from Table 27 in Committee on the Judiciary, United States Senate, "The Demand for New Automobiles," *Report of the Subcommittee on Antitrust and Monopoly*, [7].

^b Suit's results are those as reworked by the subcommittee staff, see [7 pp. 130-51].

^c Presented in *Hearings on Administered Prices in the Automobile Industry*, U. S. Senate Committee on the Judiciary, Washington 1958, p. 3192.

^d Atkinson [3].

^e Roos and Von Szeliski [31].

have a greater effect on new car purchases than on the purchases of other durable goods, but with a slightly longer lag. This suggests that consumers begin the adjustment in their expenditures on other durables during the quarter in which there is a change in income, but do not begin the adjustment in their automobile expenditures until one quarter later. Thus, the results support the view that consumers spend more time planning their larger expenditures than their smaller ones.¹²

¹¹ The similarities between our results for the automobile equation and those shown in Table 3, which are based on annual observations, also provide some support for the use of seasonally adjusted data. The use of this data has recently come under question (see Jorgenson [20] and Nerlove [30]). An alternative procedure, which has been employed by Goldfeld [17] and others, is to use unadjusted data and seasonal dummy variables.

¹² Evidence derived from interview studies is presented in Ferber [11], [12] and Katona and

For interest rates, there are several points that warrant comment. To begin with, the new purchase elasticity for automobiles with respect to the yield on *Aaa* rated corporate bonds is three-fourths of the elasticity with respect to price and approximately one-fifth of the elasticity with respect to income. For other durables, the effects of interest rates are more important. The sum of the elasticities with respect to the yield on bonds and savings accounts is more than 30 per cent of the elasticity with respect to either income or relative price. This plus the statistical significance of the variables clearly indicates the importance of interest rates as determinants of the demand for consumer durable goods.

The lags associated with the yield on bonds are also of interest. A strict interpretation of the results suggests that it takes between four and six quarters for changes in bond yields to have their maximum effect on consumer expenditures on durable goods. After that, the intensity of the response declines exponentially.¹³ The length of these lags relative to those associated with the other independent variables suggests that r_{Aaa} is acting primarily as a proxy for the rates charged on consumer credit, rather than as a measure of the yields available on marketable financial assets. If this is the case, it follows that (a) marketable financial assets are relatively poor substitutes for consumer durable goods, and (b) the estimated lags may be more appropriately viewed as estimates of the reaction times of the suppliers of consumer credit than those of consumers. That is, they serve as indicators of the speed with which the rates charged on consumer credit adjust to changes in open-market rates.

Direct evidence on the relationship between open-market rates and new-auto finance rates is reported by Shay [32], [33]. He finds that

Mueller [23]. Leaving aside the question of the initial lag, we note that the coefficients of the lagged dependent variables ($1-\theta$) suggest that the adjustment of the actual stock of automobiles to its desired level is accomplished more quickly than the adjustment for other durables. In addition, if the income coefficients in the two equations are divided by the adjustment coefficients, the long-run effects of income on the desired stocks of automobiles and other durables are practically the same. The estimates of these effects are $.69(1-e_1)$ and $.76(1-e_1)$, respectively (see equation 9).

¹³ While this adjustment pattern is not what we would have expected, it is similar to those obtained in two recent studies of business investment expenditures by Jorgenson [21] and Fromm and Klein [16]. In Jorgenson's study expenditures on plant and equipment are disaggregated on the basis of four major industrial classifications: durable manufactures, nondurable manufactures, regulated industries, and all other industries included in the OBE-SEC Investment Survey. The results indicate that for three of the groups the first noticeable effects on expenditures do not occur until three to six quarters after there is a change in the independent variables. In each case, the peak response occurs one quarter later and thereafter the intensity of the response declines at something which very quickly approaches a constant rate. Fromm and Klein use the same classifications and obtain similar results. They find that for two of the classifications both the initial and the peak response to changes in interest rates occur after a lag of five quarters.

(1) the rates are positively correlated, and (2) during the period 1954–59 the yield on *Aaa* rated corporate bonds moved more promptly at business cycle turns than new-auto finance rates. Although the data are not sufficiently numerous to permit a detailed analysis, visual examination suggests that the two series are out of phase by about two or three quarters. In this respect the behavior of automobile finance rates corresponds very closely to that of bank rates on short-term business loans. During the period 1953–61 the peaks in the latter series occurred approximately six months after the peaks in the *Aaa* corporate rate, and troughs occurred about four months later.¹⁴

The substitution relations for savings accounts are somewhat different from those for bonds. In particular, savings accounts appear to be more effective substitutes for durable goods other than automobiles. The techniques used here to study the substitution relationships between liquid assets and consumer durable goods differ from those which have been used in the past. As a general rule, it is the stock of liquid assets rather than their yield which is used as an independent variable in the durables equations. Despite this difference, our conclusions are similar to those implied by the results of Liebenberg, Hirsch, and Popkin [26], and Suits and Sparks [37]. They differ, however, from those of Evans and Kisselgoff [10], and Fromm and Klein [16].

To determine the extent to which measurement of the variables is critical for the results, equation (8) was re-estimated using BLS data in place of the implicit price deflators, and using the yield on finance company paper (r_{FCP}) as a measure of r_{FS} . The parameter estimates for the first two regressions in Table 4 suggest that the two sets of price data yield the same conclusions. There are no significant differences between these estimates and those in Table 1.¹⁵ The second set of equations presents the results obtained when r_{FCP} is substituted for r_{Aaa} . Their only major difference from the results in Table 1 is that the absolute values of the coefficients for the short-term interest rate are lower than those for the long-term rate. This discrepancy can be attributed almost entirely to the difference in the variances of the rates. Since both yields are assumed to provide measures of the rates charged on consumer credit we would expect the coefficients to be inversely related to the standard deviations. This is precisely what we find. When the coeffi-

¹⁴ Cagan [5]. It is also worth noting that if (as was suggested in Section I) the response patterns used here are viewed as approximations to those where the adjustment builds up gradually to a peak and then tapers off, our estimates of the lag between changes in open-market rates and the peak adjustments in expenditures are similar to those reported by deLeeuw [8]. The latter estimates are six quarters for "other" durable goods and seven quarters for automobiles. For the results of an interview study of consumer sensitivity to finance rates see Juster and Shay [22].

¹⁵ The simple correlations between the alternative measures of relative prices are .678 for automobiles and .994 for other durables.

TABLE 4—REGRESSION COEFFICIENTS AND OTHER STATISTICS FOR ALTERNATIVE SPECIFICATIONS OF THE VARIABLES—
QUARTERLY DATA, 1953–1964
(standard errors in parentheses)

| Source of Price Data | Dependent Variables | Y | P | r_{Aaa} | r_{FCP} | r_{SV} | Lagged Dependent Variables | C_2^A | \bar{R}_p^2 R_p^2 |
|-------------------------|------------------------|-------------------------|--------------------------|----------------------------|--------------------------|----------------------------|----------------------------------|------------------|--------------------------|
| BLS | C_A | .245 (.038) [1.0] | -.254 (.110) [0.0] | -4.562 (1.126) [4.5] | | | .604 (.067) | .37 ^a | .9288 .7582 |
| BLS | C_{DEA} | .120 (.018) [0.0] | -.296 (.088) [0.0] | -.904 (.361) [6.0] | | -3.090 (1.096) [0.0] | .870 (.039) | .48 ^a | .9950 .7698 |
| DC | C_A | .279 (.058) [1.0] | -.217 (.109) [0.0] | | -.728 (.395) [4.0] | | .691 (.071) | .19 ^a | .9056 .6820 |
| DC | C_{DEA} | .111 (.015) [0.0] | -.363 (.098) [0.0] | | -.410 (.124) [5.5] | -3.760 (.832) [0.0] | .842 (.038) | .57 ^a | .9948 .8290 |

Note: The data for all variables except interest rates are adjusted for seasonal variation.

The numbers in square brackets denote the lag associated with the independent variables.

BLS . . . Bureau of Labor Statistics

DC . . . Department of Commerce

^a The quarterly estimate from which this statistic is derived was estimated within an interval of $\pm .01$.

cients are multiplied by the standard deviations of the independent variables the remaining differences are not statistically significant.¹⁶ Thus, it appears to make very little difference whether short- or long-term market rates are used as measures of the rates that consumers pay on their liabilities.

In summary, the results suggest that expenditures on consumer durable goods can be explained as a relatively simple function of income, relative price, and interest rates. In many respects, these findings are in agreement with the evidence that has been accumulated in the area. What sets this study apart from those which have preceded it is that we are able to isolate a significant relationship between expenditures on consumer durable goods and interest rates.

From our point of view these results make a considerable amount of economic sense. They imply that in the aggregate consumer purchases of durables are influenced by *both* the prices of the goods and the costs of financing them. This conclusion does not necessarily mean that an increase in interest rates will lead consumers to postpone or cancel purchases that they had already planned. All that is indicated is that consumers spend less than they otherwise would. This may be accomplished by purchasing a lower-priced model or by forgoing some marginal extras. The vast array of models and extra equipment available on automobiles is well known. But other durables, too, come in a wide variety of models, colors, sizes, shapes and materials.

III. *Alternative Formulations*

In view of the interest rate results reported above, i.e., the significant coefficients and the long lags associated with open-market rates, it seems appropriate to subject the model developed in Section I to a number of additional tests. First, there is the possibility that interest rates merely reflect the effects of other monetary variables, e.g., the stock of money. Second, it is useful to test the hypothesis that the relationship between open-market rates and purchases of "other" durables can be attributed to the combined effects of (1) the negative association between interest

¹⁶ The following table presents the beta coefficients (and the standard errors) for r_{Aaa} and r_{FCP} in the automobile and other durables equations.

| <i>Dependent Variable</i> | r_{Aaa} | r_{FCP} |
|---------------------------|-------------------|-------------------|
| C_A | -.1511 (.0426) | -.0903 (.0491) |
| C_{DEA} | -.0405 (.0133) | -.0356 (.0107) |

The beta coefficient for a variable is defined as the regression coefficient multiplied by the ratio of the standard deviation of the variable to the standard deviation of the dependent variable. It is easily seen that the differences between the beta coefficients for r_{Aaa} and r_{FCP} are not statistically significant.

rates and residential construction and (2) the positive correlation between housing expenditures, on the one hand, and expenditures on furniture and household appliances on the other. Third, we examine the effects of a number of additional variables. Finally, the explanatory power of the model developed in Section I is compared with that of several other models that have appeared in the literature during the past few years.

The monetary variables included in this study, in addition to interest rates, may be divided into two groups: those which reflect actions taken by the monetary authorities and those which measure consumer liquidity. The former include

M_B : the monetary base (defined as bank reserves plus currency in the hands of the public)

M_{T1} : the aggregate stock of demand deposits plus currency

M_{T2} : M_{T1} plus time deposits at commercial banks

\dot{M}_{T1} : The rate of change in M_{T1}

\dot{M}_{T2} : The rate of change in M_{T2} ¹⁷

These variables are included in the analysis to provide some indication of the manner in which monetary operations influence consumer behavior. More specifically, we wish to test the views expressed by Friedman and Meiselman. They argue that while the effects of monetary policy may be described as operating through interest rates, it is not very useful or illuminating to do so ([15], p. 217).

Consumer liquidity is measured as

M_{C1} : the consumer stock of demand deposits plus currency

M_{C2} : M_{C1} plus consumer holdings of time and savings accounts at commercial banks, mutual savings banks, and savings and loan associations

These variables have been used in previous studies of the demand for consumer durable goods for two reasons: (1) as measures of the ability of consumers to make large purchases or down payments [24] and (2) as proxies for total consumer wealth [25].

Let m serve as a general measure for the variables noted above. The effects of the variables on the purchases of consumer durable goods may be estimated by rewriting equation (3) as

$$(3.1) \quad \Delta A_t = \theta(\bar{A}_t - A_{t-1}) + \delta m_t$$

According to equation (3.1) investment in consumer durables is influenced both by the discrepancy between desired and actual stocks and

¹⁷ The use of the rate of change in the money supply as a measure of monetary tightness or ease has been suggested by a number of writers. See, for example, Cagan [5].

TABLE 5—REGRESSION COEFFICIENTS AND OTHER STATISTICS FOR
EQUATION (8.1): QUARTERLY DATA, 1953-64

(standard errors in parentheses)

| De- pendent Vari- ables | Y | P | r_{Aaa} | r_{SV} | \dot{M}_{T1} | M_{C3} | Lagged De- pendent Variables | e_2^A | \bar{R}^2 S |
|----------------------------------|-------------------------|--------------------------|--------------------------|--------------------------|-------------------------|-------------------------|---------------------------------------|------------------|--------------------|
| C_A | .237 (.073) [1.0] | -.207 (.105) [0.0] | -3.99 (1.36) [4.5] | | | .094 (.072) [1.0] | .646 (.073) | .19 ^a | .9165 .9273 |
| C_A | .272 (.051) [1.0] | -.205 (.104) [0.0] | -4.32 (1.32) [4.5] | | .356 (.243) [0.0] | | .674 (.065) | .22 ^a | .9187 .9153 |
| C_{DEA} | .100 (.017) [0.0] | -.315 (.100) [0.0] | -1.10 (0.35) [6.0] | -3.39 (0.79) [0.0] | | .053 (.018) [0.0] | .693 (.050) | .68 ^a | .9953 .2721 |
| C_{DEA} | .111 (.017) [0.0] | -.423 (.102) [0.0] | -.980 (.347) [6.0] | -3.17 (0.91) [0.0] | .173 (.084) [3.0] | | .859 (.037) | .50 ^a | .9949 .2805 |

Note: The data for all variables except interest rates are adjusted for seasonal variation. The numbers in square brackets denote the lags associated with the independent variables.

^a The quarterly estimate from which this statistic is derived was estimated within an interval of $\pm .01$.

by some measure of either aggregate or consumer liquidity.¹⁸ Substitution of equation (3.1) for (3) yields

$$\begin{aligned}
 (8.1) \quad X_t = & a' + b'[Y_{t^*} - (1 - e_2)Y_{t^*-1}] + c'[r_{t^*} - (1 - e_2)r_{t^*-1}] \\
 & + d'[P_{t^*} - (1 - e_2)P_{t^*-1}] + \frac{\delta}{1 - e_1}[m_{t^*} - (1 - e_2)m_{t^*-1}] \\
 & + (1 - \theta)X_{t-1} + v_t
 \end{aligned}$$

Each of the monetary variables discussed above was introduced into equation (8.1). Although the variables do not reflect the same considerations, the results obtained were fairly similar. Table 5 presents the results for the variables that yielded the highest multiple correlation coefficients: M_{C3} (the broad measure of consumer liquidity) and \dot{M}_{T1} (the rate of change in the money supply). The similarities between the interest rate coefficients in these equations and those in Table 1 provide little, if any, support for the view that interest rates serve as proxies for other monetary variables. Thus interest rates appear to have an inde-

¹⁸ The use of m_t as a determinant of the investment in durables instead of the desired stock has no effect on the form of the regression equations estimated below. What is affected is, of course, the interpretation of the parameters.

pendent effect on the purchases of consumer durables. This implies that consumer behavior is influenced not only by the supply, but also the demand for loanable funds, in particular the demand for credit by other sectors of the economy. It follows that the effects of monetary policy on the consumer sector will depend to a very important extent on what is going on elsewhere in the economy.

Some difficulty is encountered in choosing between M_{C3} and \dot{M}_{T1} as an additional monetary measure. Neither is significant in the automobile equation. In the nonauto equation both variables are significant and M_{C3} yields a slightly higher estimate of \bar{R}^2 ; however, the estimate of the annual rate of depreciation associated with M_{C3} (.68) seems excessive. This suggests that \dot{M}_{T1} is the more appropriate of the two measures, but additional evidence will be presented below.

The second hypothesis to be tested is that variations in residential construction expenditures (E_{RC}) play a major role in explaining the association between market rates of interest and the purchase of "other" durable goods. The addition of E_{RC} to the right-hand side of equations (3) and (3.1) yields the following estimation equations:

$$(8.2) \quad X_t = a' + b'[Y_{t^*} - (1 - e_2)Y_{t^*-1}] + c'[r_{t^*} - (1 - e_2)r_{t^*-1}] \\ + d'[P_{t^*} - (1 - e_2)P_{t^*-1}] \\ + \frac{\Omega}{1 - e_1} [E_{RC,t^*} - (1 - e_2)E_{RC,t^*-1}] + (1 - \theta)X_{t-1} + v_t$$

$$(8.3) \quad X_t = a' + b'[Y_{t^*} - (1 - e_2)Y_{t^*-1}] + c'[r_{t^*} - (1 - e_2)r_{t^*-1}] \\ + d'[P_{t^*} - (1 - e_2)P_{t^*-1}] + \frac{\delta}{1 - e_1} [m_{t^*} - (1 - e_2)m_{t^*-1}] \\ + \frac{\Omega}{1 - e_1} [E_{RC,t^*} - (1 - e_2)E_{RC,t^*-1}] + (1 - \theta)X_{t-1} + v_t$$

The parameter estimates and other results for these equations are shown in Table 6. Once again the interest rate coefficients are statistically significant. In addition, the coefficients for residential construction have the anticipated signs (positive) and are significantly different from zero. This finding provides some support for the view that consumers treat houses and household furnishings as complements. However, similar results are obtained when E_{RC} is included in the automobile equation. Hence, all that we may be observing is a tendency for housing expenditures to act as a leading indicator, i.e., to lead expenditures on other physical assets by one or two quarters.¹⁹

¹⁹ The appropriate way to evaluate the substitution relationships between housing and other physical assets is to use the relative price of houses as an explanatory variable in equation (3).

TABLE 6—PARAMETER ESTIMATES FOR EQUATIONS INCLUDING EXPENDITURES ON RESIDENTIAL CONSTRUCTION: QUARTERLY DATA, 1953-64
(standard errors in parentheses)

| Dependent Variables | Y | P | r_{Aco} | r_{SV} | E_{RG} | M_{CS} | \dot{M}_{T1} | Lagged Dependent Variables | C_2^A | \bar{R}_S^A |
|---------------------|-------------------------|--------------------------|--------------------------|--------------------------|-------------------------|-------------------------|-------------------------|----------------------------|------------------|----------------|
| C_{DZA} | .094 (.017) [0.0] | -.404 (.095) [0.0] | -0.82 (0.33) [6.0] | -3.16 (0.85) [0.0] | .181 (.055) [1.0] | | | .888 (.037) | .50 ^a | .9957 .2612 |
| C_{DZA} | .082 (.017) [0.0] | -.293 (.092) [0.0] | -0.83 (0.33) [6.0] | -3.09 (0.73) [0.0] | .145 (.051) [1.0] | .042 (.017) [1.0] | | .744 (.051) | .70 ^a | .9960 .2512 |
| C_{DZA} | .092 (.017) [0.0] | -.399 (.096) [0.0] | -0.80 (0.33) [6.0] | -3.13 (0.85) [0.0] | .158 (.060) [1.0] | | .081 (.086) [3.0] | .892 (.037) | .50 ^a | .9957 .2616 |
| C_A | .186 (.033) [1.0] | -.204 (.103) [0.0] | -3.80 (1.21) [4.5] | | .489 (.191) [2.0] | | | .521 (.076) | .55 ^a | .9234 .8886 |

Note: The data for all variables except interest rates are adjusted for seasonal variation. The numbers in square brackets denote the lags associated with the independent variables.

^a The quarterly estimate from which this statistic is derived was estimated within an interval of $\pm .01$.

The evidence presented in Table 6 also bears on the importance of using M_{C3} and \dot{M}_{T1} as explanatory variables in the durables equations. Once E_{RC} is added to the model, the t -ratio for \dot{M}_{T1} falls below 1.0. Thus the case for including either of these variables in the analysis appears to be rather weak.

Two other variables that were considered as determinants of the demand for durable goods are (1) the yield on equities (measured as Moody's dividend yield on industrial common stocks)²⁰ and (2) the rate of unemployment. The latter variable may be viewed as a general cyclical indicator (see Evans and Kisselgoff [10]) or as a rough means of separating unemployment benefits from other income, the assumption being that unemployment benefits are unlikely to be spent on durable goods (Suits and Sparks [37]). The results for these variables are inconclusive. Though each might be expected to have some effect, their inclusion does not yield a significant improvement in the explanatory power of the durables equations. This is particularly true when the expenditures on residential construction are incorporated in the model. The t -values for the variables never exceed 1.5. Although the apparent effects are in the expected direction, more data will be required before one can be reasonably sure of the persistence and the magnitude of their influence.

The interest rate results obtained in these tests are similar to those reported above. Thus, the case for including interest rates is strengthened considerably. Regardless of what variables are added to the analysis the interest rate effect remains significant. Moreover, the only additional variable that contributes significantly to the explanatory power of the model is the expenditures on residential construction.

One difficulty which often affects estimates from economic time series is largely absent in the present study. For the period under consideration there is little significant correlation among the weighted differences of the variables included in equation (8), the equation estimated in the previous section. For example, the highest correlation involving the yield on *Aaa* rated corporate bonds ($-.54$) is between the bond yield and the relative price of "other" durables.

Finally, let us compare the performance of equation (8) with that of three other models that have appeared in the literature during the past few years (Suits and Sparks [37]; Liebenberg, Hirsch and Popkin [26]; and Evans and Kisselgoff [10]). The time periods used to fit the regression equations in these studies and the estimates of the adjusted coefficients of determination are given in Table 7, Part A. Part B shows the results obtained when equation (8) is reestimated for the periods (1953:3–1961:4) and (1953–62).

²⁰ This specification of the yield on equities is discussed in Hamburger [18].

TABLE 7—ALTERNATIVE DEMAND FUNCTIONS FOR CONSUMER DURABLE GOODS

Part A: Estimates of the Adjusted Coefficients of Determination
and the Time Periods Used in Several Studies

| Study | Time Periods | Adjusted Coefficients of Determination | |
|----------------------------------|---------------|---|----------------|
| | | Autos | Other Durables |
| Suits and Sparks | 1953:3–1961:4 | .686 | .942 |
| Liebenberg, Hirsch and Popkin | 1953–1964 | .91 | .98 |
| Evans and Kisselgoff | 1953–1962 | .710 | .986 |

Part B: Parameter Estimates for Equation (8) for the
Periods 1953:3–1961:4 and 1953–62—Quarterly Data
(standard errors in parentheses)

| De- pendent Vari- ables | Time Period | F | P | r_{Aaa} | r_{SV} | Lagged De- pendent Variables | e_2^A | \bar{R}^2 S |
|----------------------------------|----------------|-------------------------|--------------------------|--------------------------|--------------------------|---------------------------------------|------------------|--------------------|
| C_A | 1953:3–61:4 | .266 (.066) [1.0] | –.222 (.110) [0.0] | –4.13 (1.47) [4.5] | | .630 (.090) | .19 ^a | .7712 .9576 |
| C_A | 1953–62 | .249 (.047) [1.0] | –.245 (.108) [0.0] | –4.58 (1.24) [4.5] | | .631 (.081) | .34 ^a | .8258 .9290 |
| C_{DEA} | 1953:3–61:4 | .109 (.016) [0.0] | –.468 (.129) [0.0] | –1.03 (0.37) [6.0] | –2.52 (1.78) [0.5] | .810 (.072) | .50 ^a | .9837 .2584 |
| C_{DEA} | 1953–62 | .105 (.016) [0.0] | –.465 (.100) [0.0] | –1.12 (0.34) [6.0] | –3.21 (1.55) [0.0] | .876 (.066) | .45 ^a | .9883 .2694 |

Note: The data for all variables except interest rates are adjusted for seasonal variation. The numbers in square brackets denote the lags associated with the independent variables.

^a The quarterly estimate from which this statistic is derived was estimated within an interval of $\pm .01$.

Note first that although two of the time periods differ from the one used to specify equation (8), the variance explained by it is greater than that explained by the alternatives. The only instance in which the estimates of \bar{R}^2 are fairly close is the comparison between the automobile equation presented here and the one estimated by Liebenberg, Hirsch and Popkin. However, a considerable portion of the explanatory power of the latter can be attributed to the inclusion of a dummy variable, da (see the Appendix). If da is eliminated from the equation the estimate of \bar{R}^2 is approximately .86.

Second, there is some evidence to suggest that the parameters in equation (8) are fairly stable over time. The only possible exception is the coefficient for the yield on savings accounts in the "other" durables equation. The absolute value of that coefficient and its *t*-ratio increase with time. This result may be explained in several ways. For example, it might be argued that the growth in the competition among thrift institutions in recent years has increased consumer sensitivity to interest rates. Alternatively, there is the argument that the rates offered by savings institutions may not have a significant effect on consumer expenditures until they reach some critical level. Other explanations could be provided. However, it is more important to note that the present results furnish additional confirmation for the conclusion reached earlier, namely, that interest rates are an important determinant of consumer behavior.

IV. *Conclusion*

The findings reported in the previous sections have a number of broader implications. First, they are not entirely consistent with either of the two most widely held views concerning the channels through which monetary policy operates. According to the Keynesian income-expenditure approach, changes in monetary policy affect interest rates, interest rates affect the level of investment, and the level of investment in turn affects income and consumption through the multiplier process. The evidence presented here suggests that there is a much more direct link between monetary operations and consumer expenditures on durable goods. This more direct link is recognized in the alternative approach expounded by Friedman and Meiselman, but they argue that it is neither very useful nor illuminating to view the effects of monetary policy as operating through interest rates. While their view may be appropriate for aggregate analysis, it does not appear to be consistent with the evidence for the consumer sector. This finding should not surprise those who favor either the Chicago or the Keynesian approach. It implies that purchases of consumer durables are influenced by both the money supply and the demand for credit by the other sectors of the economy. Thus the results of this study, although not in complete agreement with either of the two principal views concerning the channels through which monetary policy operates, may serve to bring them closer together.

The discussion above suggests that purchases of consumer durables should be treated as investment, that is, as autonomous expenditures. This casts doubt on the interpretations of the correlations between induced and autonomous expenditures that have appeared in a number of recent articles [2] [9] [15]. Generally, the correlations are viewed as providing support for the stability of the investment multiplier. How-

ever, if most of the short-run variation in consumption can be attributed to fluctuations in the purchases of durables, these results may do little more than reflect similar cyclical movements in two different kinds of autonomous expenditures.²¹

Finally, it seems worthwhile to say a few words about the lag in the effect of monetary policy. Tucker's analysis [38], [39] implies that the lags observed in both the consumer durables equations and the business investment equation (see, for example, [16]) do not indicate a corresponding lag in the effect of monetary policy. This conclusion is founded, however, on the assumption that either the availability of credit or quoted interest rates and other terms of lending contracts adjust fairly rapidly to changes in the money supply—in other words, that the suppliers of credit pass on every change in their costs when it occurs. To the extent that this is not done, the effects of monetary policy on the real sector will be postponed. Our results suggest that the suppliers of consumer credit do not behave in the manner assumed in Tucker's model, i.e., that they do not alter their prices every time there is a change in open market rates. It seems likely that other creditors (for example, the suppliers of trade credit) behave in a similar way, but this is an empirical question upon which there is very little evidence at the present time.

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²¹ Of the three studies noted above, the Friedman and Meiselman paper [15] is the only one that mentions the possibility of treating the purchases of consumer durables as autonomous expenditures.

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APPENDIX

Definitions of Symbols

- e_{21}^A : annual percentage rate of depreciation on existing stock.
- P_A : relative price of automobiles measured alternatively as: (1) the implicit price deflator for personal consumption expenditures on automobiles and parts divided by the implicit deflator for all consumption expenditures, 1958=100; and (2) the new car component of the Bureau of Labor Statistics' Consumer Price Index deflated by the index value for all items, 1957-59=100.
- P_{DEA} : relative price of other durables measured alternatively as: (1) implicit price deflator for personal consumption expenditures on durable goods other than automobiles and parts divided by the implicit deflator for all consumption expenditures, 1958=100; and (2) the BLS index of the prices of house furnishings deflated by the value of all items, 1957-59=100.
- C_A : personal consumption expenditures on automobiles and parts, seasonally adjusted at annual rates in billions of 1958 or 1957-59 dollars.

- C_{DEA} : personal consumption expenditures on durable goods other than automobiles and parts, seasonally adjusted at annual rates in billions of 1958 or 1957-59 dollars.
- Y : disposable personal income, seasonally adjusted at annual rates deflated by either the all items component of the CPI or the implicit deflator for all consumption expenditures, seasonally adjusted at annual rates in billions of 1958 or 1957-59 dollars.
- r_{Aaa} : Moody's *Aaa* rate on long-term corporate bonds, per cent.
- r_{FCP} : interest rate on directly placed three-six month finance company paper, per cent.
- r_{SV} : a weighted average of the rates paid on savings and loan shares, mutual savings bank deposits and time and savings deposits at member banks of the Federal Reserve System, per cent.
- M_B : monetary base, bank reserves plus currency in the hands of the public at the beginning of the period, seasonally adjusted in billions of 1958 dollars.
- M_{T1} : aggregate stock of demand deposits plus currency at the beginning of the period, seasonally adjusted in billions of 1958 dollars.
- M_{T2} : M_{T1} plus time deposits at the beginning of the period, seasonally adjusted in billions of 1958 dollars.
- M_{C1} : consumer stock of demand deposits plus currency at the beginning of the period, seasonally adjusted in billions of 1958 dollars.
- M_{C3} : M_{C1} plus consumer holdings of time and savings accounts at commercial banks, mutual savings banks and savings and loan associations at the beginning of the period, seasonally adjusted in billions of 1958 dollars.
- \dot{M}_{T1} : rate of change of M_{T1} during the period.
- \dot{M}_{T2} : rate of change of M_{T2} during the period.
- E_{RC} : expenditures on residential construction, seasonally adjusted at annual rates in billions of 1958 dollars.
- da : dummy variable for Liebenberg, Hirsch and Popkin [26] auto equation (-1 during strike quarter; $+1$ following strike quarter; $+1$ in 1955 to reflect abrupt credit and taste changes; 0 otherwise).

THE GROWTH OF OUTPUT AND INPUTS IN SOVIET TRANSPORT AND COMMUNICATIONS

By NORMAN M. KAPLAN*

The separate performance of major sectors of the economy has engaged the attention of many students of Soviet economic growth. In the case of transport, the aggregative aspects of performance have been neglected because neither the official measures nor the various non-Soviet estimates of output growth have contributed an index.¹ One purpose of this paper is to summarize the results of an attempt to construct and use output indexes for transport and communications. Part I presents the indexes and their growth rate implications and compares the output increases with increases in capital and employment; U.S. data for similar sectors are assembled as a framework for appraising U.S.S.R. performance. Because detailed descriptions and comparisons are available in [4] and [5], only the major results are briefly offered here.

Among the findings in Part I is a marked retardation in output growth within the postwar period. Part II seeks to explore that retardation by distinguishing between changes in the rate of increase of factor inputs and changes in the rate of increase of combined factor productivity.

I. *The Growth of Output and Inputs*

Table 1 presents output indexes for transport and its major components and for transport and communications combined. Transport includes the freight and passenger operations of the railroad, maritime, internal waterway, pipeline, motor and air systems. Excluded are the taxi, trolley-bus, and subway systems, as well as freight and passenger services rendered by administrative or producing units for their own respective uses. Communications include the postal system, the telephone and telegraph system, and wireless broadcasting.

The individual physical output components of the transport index are ton-kilometers of freight traffic and man-kilometers of passenger traffic for each of the carriers. The weights used to combine the output series into indexes of transport output are ruble operating costs per

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¹ For example, an index of national product, estimated in 1964 from sector output indexes, uses ton-kilometers of railroad freight as a surrogate for all transport output [12, p. 94]. The 1965 and 1966 successors to [12]—[13, p. 20] and [14, p. 128]—incorporate the transport and communications indexes to be described here.

TABLE 1—INDEXES OF TRANSPORT AND COMMUNICATIONS OUTPUT, 1928-1963
(1955=100)

| | Transport Output | | | | Transport and Communica- tions Output |
|------|-----------------------|-------------------------|---|-------------------------|---|
| | All Freight (1) | All Passenger (2) | Railroad Freight and Passenger (3) | All Transport (4) | |
| 1928 | 9.3 | 14.9 | 11.2 | 10.6 | 10.7 |
| 1929 | 11.1 | 19.2 | 14.0 | 12.9 | 13.0 |
| 1930 | 13.4 | 31.2 | 18.7 | 17.4 | 17.5 |
| 1931 | 15.4 | 36.7 | 21.7 | 20.1 | 20.4 |
| 1932 | 16.8 | 49.0 | 26.3 | 24.0 | 24.3 |
| 1933 | 17.2 | 43.0 | 25.1 | 23.1 | 23.8 |
| 1934 | 20.5 | 41.4 | 27.5 | 25.2 | 26.1 |
| 1935 | 25.5 | 39.9 | 31.2 | 28.7 | 29.7 |
| 1936 | 31.1 | 44.9 | 37.9 | 34.1 | 34.9 |
| 1937 | 33.5 | 52.8 | 42.4 | 37.8 | 38.5 |
| 1938 | 34.6 | 49.8 | 42.9 | 37.9 | 39.2 |
| 1939 | 36.1 | 55.1 | 45.9 | 40.4 | 41.7 |
| 1940 | 37.8 | 58.1 | 48.4 | 42.3 | 43.7 |
| 1945 | 29.4 | 38.8 | 35.4 | 31.4 | 32.1 |
| 1946 | 31.2 | 54.3 | 41.9 | 37.4 | 38.2 |
| 1947 | 33.4 | 58.2 | 42.8 | 38.9 | 40.0 |
| 1948 | 41.7 | 51.7 | 48.6 | 43.9 | 45.0 |
| 1949 | 48.9 | 51.7 | 54.7 | 49.5 | 50.7 |
| 1950 | 56.3 | 56.3 | 62.0 | 56.3 | 57.5 |
| 1951 | 62.9 | 63.7 | 69.8 | 63.1 | 64.4 |
| 1952 | 69.5 | 70.2 | 76.3 | 69.9 | 70.9 |
| 1953 | 76.0 | 79.0 | 82.5 | 76.7 | 77.8 |
| 1954 | 85.0 | 87.9 | 88.9 | 85.6 | 86.4 |
| 1955 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 |
| 1956 | 113.5 | 104.7 | 108.9 | 111.6 | 111.1 |
| 1957 | 129.3 | 119.6 | 121.4 | 127.1 | 125.8 |
| 1958 | 142.7 | 133.7 | 129.4 | 140.7 | 138.8 |
| 1959 | 159.0 | 150.2 | 140.6 | 157.1 | 154.5 |
| 1960 | 173.6 | 169.1 | 147.7 | 172.6 | 169.6 |
| 1961 | 185.7 | 190.2 | 153.6 | 186.7 | 183.3 |
| 1962 | 197.5 | 217.1 | 162.0 | 201.8 | 198.1 |
| 1963 | 212.9 | 241.9 | 170.7 | 219.4 | 215.2 |

Sources: [4, Tables A-4 and A-7] as revised and extended in [5, Table 4]. Column (5) is the Variant III index in [5, Table 4] and the communications component is the combined employment and ruble output series. For an extension of these indexes to 1964 and 1965, see fn. 2.

ton or passenger-kilometer for the relevant carrier in 1955. I omit here the several reservations that need to be attached to the results and the somewhat different results obtainable with different weights because they are stated in [4] and [5]. In submitting the indexes as a first attempt to measure the change in Soviet transport output I would be more diffident than I am were it not for the observation of similar solu-

tions to the problems of measuring U.S. transport output [6, Appendix G] [10, Appendix I].

Because Soviet capital data combine transport and communications, sector comparisons of output and input growth require an index of combined transport and communications output. Therefore, despite inherent difficulties for a sector like communications and the paucity of published data on Soviet communications, I have assembled an index of communications output by using an official ruble output series for several years and an employment series for interpolations and extensions to other years. The communications and transport indexes are combined with 1955 gross value weights to obtain the index in Table 1, Col. (5). Fortunately, the combined transport and communications index is relatively insensitive to alternative measures of communications output and alternative weights.²

For selected periods, Table 2 presents the average annual rates of output increase implied by the indexes in Table 1. The periods represented are the consequences of the following considerations. Students of the Soviet economy have noted a marked retardation in output growth after 1958 in comparison with earlier periods, a retardation observed not only for agriculture but also for industry and the economy as a whole, e.g. [12, pp. 91-98]; to determine whether a similar retardation holds for transport and communications, pre- and post-1958 periods are distinguished. To exclude years of substantial war damage and possibly cheap recovery, the postwar data begin with the early 1950s and the precise years chosen depend on the years for which capital data are available. After very rapid growth in 1928-32, the indexes in Table 1 show less rapid growth in 1932-37 and little growth in 1937-40 despite the territorial acquisitions for which no adjustment is made; to provide alternative prewar perspectives for viewing the postwar rates of growth, 1932-40 is separated from 1928-40.

In order to provide a benchmark for assaying the Soviet growth rates, Table 2 also presents growth rates for at least roughly comparable sectors in the United States from 1869 to 1950. The major conclusions from Table 2 are:

A. For Soviet transport and communications and its indicated components, the rates of output increase in 1958-63 are substantially below those in 1951-58 or 1953-58. The deceleration noted for other segments of the economy and for the economy as a whole holds also for transport and communications. It should be noted, too, that the conclusion does not depend on the precise time demarcations in Table 2: the marked

² See [4, Tables A-3 and A-7] and [5, Table 4]. It should also be noted that output data now available make it possible to extend the indexes to 1964 and 1965. The results are: for transport, 242.2 in 1964 and 266.0 in 1965; for transport and communications, 237.2 and 260.7. The output data are from [21, pp. 431, 433, 485, 500], [23, pp. 97, 98, 102], and 1962-64 relationships between motor freight output of "general use" and total motor freight output.

TABLE 2—AVERAGE ANNUAL RATES OF OUTPUT INCREASE, U.S.S.R. AND U.S. TRANSPORT AND COMMUNICATIONS

(per cent per year)

| | Transport and Communications | Transport | Railroad Transport |
|---------------|------------------------------|-----------|--------------------|
| U.S.S.R. | | | |
| 1928-40 | 12.4 | 12.2 | 13.0 |
| 1932-40 | 7.6 | 7.3 | 7.9 |
| 1951-58 | 11.6 | 12.1 | 9.2 |
| 1953-58 | 12.3 | 12.9 | 9.4 |
| 1958-63 | 9.2 | 9.3 | 5.7 |
| 1951-63 | 10.6 | 10.9 | 7.7 |
| United States | | | |
| 1869-79 | 7.0 | 7.0 | — |
| 1879-89 | 9.7 | 8.5 | 8.5 ^a |
| 1890-1909 | 6.4 | 6.1 | 5.7 |
| 1909-29 | 3.7 | 3.3 | 2.6 |
| 1929-50 | 3.3 | 3.3 | 1.2 |

^a 1880-90

Sources: For the U.S.S.R., the underlying indexes are in Table 1. For the United States, the 1869-79 and 1879-89 data are from [6] obtained by methods described in [4, Table 5, Note c] and the other data are from [10, pp. 458-59, 464-65, 472-73, 486-87]. The 1869-79 and 1879-89 data differ from the other data in that the former include street and electric railways. To obtain post-1889 transport and communications output, I have subtracted from the total for all regulated industries the output of electric light and power, manufactured and natural gas, and street and electric railways. Similarly, for post-1889 transport output the output of street and electric railways has been subtracted. The inclusion of street and electric railways for the years since 1889 yields essentially the same rates of increase except for 1929-50 with noticeably lower rates of increase.

deceleration survives with 1962, 1964, or 1965 as the terminal year of the later period, with 1954 as the initial year of the earlier period, and with 1959 as the dividing year between the two periods. On the other hand, there is no evidence of any appreciable decline in growth rates *within* the 1951-58 period.

B. Whether the post-1951 rates of increase exceed or are exceeded by the prewar rates of increase depends upon the prewar period considered. The 1951-58 and 1951-63 rates of increase are below the 1928-40 rates and, except for 1951-63 railroad transport, above the 1932-40 rates. Thus, the decline in growth rates between 1951-58 and 1958-63 cannot be dismissed as a simple continuation of past trends.³

³ One difficulty with these comparisons is that the growth rates are the conventionally defined average annual rates of increase, the rate which for a given period yields the terminal-year value from the initial year value. I am led by Boris Pesek's excellent essay on growth measures [9] to an appreciation of this difficulty, to a general preference for a measure that depends on all observations, and to specific candidates. Applying his methods VI and VII to the data at hand, I observe that the alternative measures make little or no difference except for 1928-40. For 1928-40, his method VII yields 10.5, 10.3, and 11.0 in place of the respective entries in Table 2. Thus, the conclusion in the text is strengthened by the use of this alternative measure. It should also be observed that none of the conclusions in the preceding or subsequent paragraph is affected by the use of the alternative measures from Pesek.

C. The growth rates for Soviet transport and communications and its major components generally exceed those for the United States and by very substantial amounts. The exception is the decade of the 1880s in the United States when the growth rates exceed those for *some* subperiods in the U.S.S.R.⁴ With that exception, even the 1958-63 growth rates for U.S.S.R. transport and communications substantially exceed those in the U.S.

To compare the Soviet output increases with capital and employment increases, Table 3 presents the basic series for selected years from 1928 to 1963 and the relative increases in each for several subperiods. The capital values are the official series for fixed capital at July 1, 1955 prices gross of depreciation. The results are clear. Over the 1928-63 period as a whole, output has increased by much more than the increase in either employment or capital and, therefore, by much more than the increase in combined factor inputs regardless of the input weights. For each of the indicated subperiods considered separately the same inequality holds: an output increase in excess of each of the input increases.

Since the persistence of that inequality might be construed to question the reliability of the output or capital series, it is relevant to cite similar comparisons for the United States. In [4, Table 7] I have derived and assembled roughly comparable data for U.S. transport and communications from [5] and [10]: output at 1929 prices, fixed capital net of depreciation at 1929 prices, and persons engaged and man-hours employed, for selected years from 1869 to 1953. The results are essentially the same as the U.S.S.R. results: over the period as a whole, output has increased by much more than the increase in either capital or employment and the same inequality holds for each of the separate subperiods.⁵ For the United States but not for the U.S.S.R., it is possible to make the same comparisons for separate components of transport, communications, and public utilities and in virtually all cases the comparisons show output increases in excess of the increases in either capital or labor.⁶

⁴ Application of Pesek's method VII (fn. 3) yields Soviet growth rates of 10.4, 10.7, and 8.7 for columns (1)-(3), respectively, of Table 2 in 1928-40 and 1948-63 combined. For 1932-40 and 1948-63, the rates are essentially the same.

⁵ The results, however, differ in one respect. In the U.S.S.R., capital-labor ratios increase throughout. In the U. S., capital-labor ratios increase for some subperiods and decrease for others and capital per man-hour is the same in 1948 as in 1869.

⁶ See [6, Appendixes G and H] [10, pp. 235-36, 256-67, 320-21, 374-75, 405, 406, 440, and Appendix I]. The components for which comparisons are possible are: transport, railroad transport, local street railways, local bus lines, communications and public utilities, electric power, manufactured and natural gas, the telephone system, and the telegraph system. The indicated result holds for all cases except natural gas, local bus lines since 1919, and the post-1929 telephone system.

TABLE 3: OUTPUT, CAPITAL AND EMPLOYMENT, U.S.S.R. TRANSPORT AND COMMUNICATION 1928-1963

| | Output (1955=100) (1) | January 1 Capital (billion 1955 rubles) (2) | Average Annual Employment | |
|---|-----------------------------|---|--------------------------------|--------------------------------|
| | | | Man-Years (millions) (3) | Man-Hours (1955=100) (4) |
| Basic Series | | | | |
| 1928 | 10.7 | 45 | 1.365 | 21.4 |
| 1940 | 43.7 | 156 | 3.903 | 68.2 |
| 1951 | 64.4 | 215 | 4.899 | 87.4 |
| 1953 | 77.8 | 237 | 5.352 | 95.0 |
| 1954 | 86.4 | 256 | 5.536 | 98.1 |
| 1958 | 138.8 | 330 | 6.332 | 105.8 |
| 1959 | 154.5 | 357 | 6.663 | 108.7 |
| 1962 | 198.1 | 450 | 7.509 | 111.0 |
| 1963 | 215.2 | 495 | 7.718 | 114.5 |
| Link Relatives By Subperiods (Ini- tial Year=100) | | | | |
| 1928-40 | 408 | 347 | 286 | 319 |
| 1940-51 | 147 | 138 | 126 | 128 |
| 1951-58 | 216 | 154 | 129 | 121 |
| 1958-63 | 155 | 150 | 122 | 108 |

Sources:

Output: Table 1.

Capital: For average 1928, 1941, 1951, and 1963, from the 1964 value and indexes in [20, pp. 55-56]; for 1928 and 1940, from the average semi-annual rate of increase between average 1928 and 1941. For 1953, from the 1960 value in [17, p. 67] and indexes in [19, pp. 52-53]. For 1954, from the 1963 value in the table and indexes in [22, p. 31]. For 1962, from [18, p. 69]. The 1958 figure is estimated from investment data on the assumption of the same ratio between 1953-57 and 1953-58 capital increments and investment.

Man-Years: From [18, p. 567], [20, p. 475], and [16, pp. 658-59] except for 1951 and 1954 which are estimates in [11, p. 60].

Man-Hours: From man-years and an index of hours worked per year. The latter is the product of days worked per year and hours worked per day. The data on days worked per year are from [8, Table Q-3] for 1928, 1940, 1950, 1958 and 1959; from [13, p. 80] for 1952, 1955, 1962 and 1963; and interpolated for 1951, 1953 and 1954. The data on hours are from [8, Table Q-3] for 1928 and from [13, p. 80] for all other years except 1951, 1953, and 1954 which are interpolated.

The appearance of the same results in both U.S. and U.S.S.R. transport and communications suggests the existence of some characteristic indigenous to the sector. The nature of the data makes it impossible to specify the substance of the characteristic, but the least demanding explanation may well be the relevant one. Suppose each individual system in transport and communications is characterized by substantial indivisibilities with, once the system has been established, declining capital and operating costs per unit of output. For time series aggregating such systems' data, output increases will exceed capital and labor

input increases so long as the indivisible capital expenditures for new systems are more than offset by the declining costs of existing systems.⁷ If this explanation is relevant, the conventional characterization of the transport and communications system as "social overhead" capital has economic substance to it.

The excess of output over input increases also raises other issues. Given the excess in the increase of national product over the increase of combined factor inputs for the economy as a whole, observed for both the United States and U.S.S.R., the problem is to explain the residual output increase. The question raised here is how much of the observed residual for the economy is accounted for by the pronounced residual in transport and communications. The substantial and persistent decreases in capital-output ratios for both U.S. and U.S.S.R. transport and communications are in strong contrast to the behavior of capital-output ratios for each economy as a whole. In the U.S.S.R., the capital-output ratio for the economy appears to have increased since 1928 or 1951 [8, Tables T-1 and P-1]. In the United States, it has been stable or rising over long periods of time, excluding the link between 1929 and the early postwar period.⁸ If these observations are correct, there must be in both the United States and U.S.S.R. either (a) some sectors for which the capital-output ratio has been increasing markedly or (b) substantial shifts in the relative importance of sectors. The implication would seem to be that studies involving capital-output ratios or the residual between output and input increases should not stop with the economy as a whole but should, whenever possible, incorporate the separate behavior of specific sectors and shifts in their relative importance.

II. *The Growth of Output per Unit of Inputs*

For many purposes the incremental capital-output ratio is a more relevant magnitude than the average capital-output ratio. In particular,

⁷ If this is the explanation, one should expect to find early periods of the development of transport and communications systems when the establishment of new systems predominates and when capital-output ratios increase with time. In the U. S. case this finding does tend to appear for major components of transport and communications except railroads. See [6, pp. 546, 548, 580, 581, 585-86, 592, 594] and [10, pp. 482, 486]. For railroads, however, the data on capital-output ratios begin in 1880 when substantial amounts of track were already in operation [10, pp. 268-69, 472-73].

⁸ Here I rely on: (1) Kendrick's Commerce Concept of GNP compared with fixed capital both net and gross of depreciation from Kuznets and Goldsmith; and (2) for the 1946-59 period separately, the Department of Commerce GNP data compared with similar capital data from Goldsmith. The GNP data are from [6, Tables A-III and A-IV] [24, Table I-2] [15, Table 5]. The Kuznets capital data in [7, pp. 64-65] include inventories; I have reproduced the fixed capital component by methods and with the results indicated in [3, Table III.10 and notes, pp. 145-47]. The Goldsmith capital data are from [1, Table W-3] [2, Tables A-35 to A-37 and A-46 to A-48].

given the evidence of a deceleration in the Soviet economy shared by transport and communications, it seems especially interesting to know the changes if any in incremental capital-output ratios. Calculating from Table 3 the ratio of the capital increment to the output increment for 1928-40, 1940-51, 1951-53, 1953-58, and 1958-63, I observe a substantial decline in the ratio from 1928-40 through 1953-58 and a substantial increase from 1953-58 to 1958-63.⁹ If these data can be interpreted even roughly as requirements indicators, they point to a marked recent increase in the investment burden per additional ruble of transport and communications output. What is not clear is why. The incremental capital-output ratio at best is an omnibus variable which summarizes the effects of such influences as the rates of increase of capital and labor, changes in their marginal productivities, economies or diseconomies of scale, technical change, etc.

Perhaps one can move a step further in the study of relationships between output and input increases by distinguishing between the influence of the rate of increase of inputs and the influence of all other factors. More specifically, let us begin with the previous observation of a decline in the average annual rate of output increase between 1951-58 and 1958-63 and seek to account for this decline in terms of (a) changes in the rate of increase of combined factor inputs and (b) changes in the rate of increase of factor productivity. To do so, consider the construction of an index of factor productivity in geometric form. Let: X_t , K_t , L_t = output, capital, and labor for year t ; a and $1-a$ = relative weights for capital and labor, respectively. The indexes for 1958 and 1963 with 1951 = 1.00 are:

$$\frac{\frac{X_{58}}{X_{51}}}{\left(\frac{K_{58}}{K_{51}}\right)^a \left(\frac{L_{58}}{L_{51}}\right)^{1-a}} \quad \text{and} \quad \frac{\frac{X_{63}}{X_{51}}}{\left(\frac{K_{63}}{K_{51}}\right)^a \left(\frac{L_{63}}{L_{51}}\right)^{1-a}}$$

where the subscripts denote the year.

The problem as posed involves comparisons of average annual rates of increase between the two time periods. Simplify the notation by letting X_1 , K_1 , L_1 represent the indexes of the variables over period 1 (here, 1951-58) and X_2 , K_2 , L_2 the indexes over period 2 (here, 1958-63).

⁹ In [4, Table 8] I present: (1) a similar calculation for somewhat different periods; and (2) for both transport and communications and railroad transport, a calculation of the changes in the ratio of investment to the output increment with investment lagged one year. The results are the same: an initial decline in the ratios followed by a recent substantial increase. From [4, Table 7] one can also observe declining or, sometimes, stable incremental capital-output ratios for the United States from 1869-79 through 1929-50.

Call the indexes of factor productivity, constructed as above, A_1 , and A_2 .¹⁰

Take logarithms of the indexes and, to handle comparisons between periods of unequal duration, divide by N_1 or N_2 —the number of years in the respective periods. In each case the result—denoted by \bar{A}_1 , $\bar{X}_1, \bar{K}_1, \bar{L}_1$, etc.—is a number whose antilogarithm is one plus the average annual rate of increase of the variable. Thus we have:

$$(1) \quad \bar{A}_1 = \bar{X}_1 - a\bar{K}_1 - (1-a)\bar{L}_1$$

$$(2) \quad \bar{A}_2 = \bar{X}_2 - a\bar{K}_2 - (1-a)\bar{L}_2$$

Known are: $\bar{X}_1, \bar{X}_2, \bar{K}_1, \bar{K}_2, \bar{L}_1, \bar{L}_2$. If a were known, one could account for the decline in the average annual rate of output increase between periods 1 and 2 in terms of changes in the rates of increase of factor inputs and factor productivity. That is to say, with known a one could account for $\bar{X}_1 > \bar{X}_2$ in terms of:

$$a\bar{K}_1 + (1-a)\bar{L}_1 \geq a\bar{K}_2 + (1-a)\bar{L}_2;$$

and

$$\bar{A}_1 \geq \bar{A}_2$$

The difficulty is to obtain the relative weights for capital and labor (a and $1-a$) in U.S.S.R. transport and communications. Perhaps the most obvious procedure is to use national-income-originating data by distributive shares. Apart from conceptual limitations in this use, such data are not readily available for the U.S.S.R. In addition, in the U.S.S.R. the "owners" of capital do not receive or impute a rate of return to capital as a distributive share. This institutional feature has led other students to introduce nonSoviet data on rates of return or distributive shares, either directly or with guessed adjustments.

To avoid such procedures, I have turned the problem around. Rather than seeking solutions for given a , I ask: for what values of a will specified conditions hold? The first entry in Panel I of Table 4 records the values of a for which:

$$(3) \quad \bar{A}_1 > \bar{A}_2.$$

$$A_1 = \frac{\frac{X_{58}}{X_{51}}}{\left(\frac{K_{58}}{K_{51}}\right)^a \left(\frac{L_{58}}{L_{51}}\right)^{1-a}} = \frac{X_1}{K_1^a L_1^{1-a}}$$

$$A_2 = \frac{\frac{X_{63}}{X_{51}}}{\left(\frac{K_{63}}{K_{51}}\right)^a \left(\frac{L_{63}}{L_{51}}\right)^{1-a}} \div \frac{\frac{X_{58}}{X_{51}}}{\left(\frac{K_{58}}{K_{51}}\right)^a \left(\frac{L_{58}}{L_{51}}\right)^{1-a}} = \frac{\frac{X_{63}}{X_{58}}}{\left(\frac{K_{63}}{K_{58}}\right)^a \left(\frac{L_{63}}{L_{58}}\right)^{1-a}} = \frac{X_2}{K_2^a L_2^{1-a}}$$

TABLE 4—CRITICAL VALUES OF THE CAPITAL SHARE (a) IN U.S.S.R. TRANSPORT AND COMMUNICATIONS

| I. Values of a for which factor productivity increases <i>more rapidly</i> in Period 1 than Period 2: | | | | | |
|---|-----------|-----------|-----------|-----------|-------------------|
| Period 1: | 1951-58 | Period 1: | 1951-53 | Period 1: | 1928-40 |
| Period 2: | 1958-63 | Period 2: | 1953-58 | Period 2: | 1951-58 |
| | > -0.34 | | $> +1.10$ | | $> +2.35$ |
| | | | | | Period 1: 1928-40 |
| | | | | | Period 2: 1951-63 |
| | | | | | $> +1.46$ |
| II. Values of a for which factor inputs increase <i>more rapidly</i> in Period 1 than in Period 2: | | | | | |
| Period 1: | 1951-58 | Period 1: | 1951-53 | Period 1: | 1928-40 |
| Period 2: | 1958-63 | Period 2: | 1953-58 | Period 2: | 1951-58 |
| | $< +0.37$ | | $< +0.54$ | | $< +2.64$ |
| | | | | | Period 1: 1928-40 |
| | | | | | Period 2: 1951-63 |
| | | | | | $< +1.88$ |
| III. Values of a for which factor productivity increases <i>within</i> indicated period: | | | | | |
| 1951-58 | 1958-63 | 1951-53 | 1953-58 | 1928-40 | 1951-63 |
| $< +2.43$ | $< +1.10$ | $< +7.57$ | $< +2.11$ | $< +2.68$ | $< +1.66$ |

Sources and Methods: The critical values of a are those which satisfy the inequalities (3), (4) and (5), pp. 1162 and 1164. The underlying data are in Table 3, Cols. (1), (2), and (4).

That is to say, the values of a are those for which the average annual rate of increase of factor productivity in 1951-58 *exceeds* that in 1958-63. The first entry in Panel II records the values of a for which the rate of increase of combined factor inputs in 1951-58 *exceeds* that in 1958-63: i.e., the values of a for which

$$(4) \quad a\bar{K}_1 + (1 - a)\bar{L}_1 > a\bar{K}_2 + (1 - a)\bar{L}_2.$$

The first two entries in Panel III records the values of a for which factor productivity increases in *each* of the periods 1951-58 and 1958-63: i.e., the values of a for which

$$(5) \quad \bar{A}_t > 0.$$

Other entries in Panels I, II, and III answer the same questions for different periods.

Consider, first, the results for 1951-58 and 1958-63. My desk calculator, unable to distinguish a set of possible or plausible weights from a numerical solution to an equation or inequality, simply grinds out the answer in Panel I that for $a > -0.34$, $\bar{A}_1 > \bar{A}_2$. This is an ignorant but, I submit, important result. It says that for *all possible* capital shares ($0 \leq a \leq 1$), or independently of the weights, the rate of increase of factor productivity in 1951-58 exceeds that in 1958-63. In Panel II, the answer similarly provided is ambiguous: whether the rate of increase of factor inputs in 1951-58 exceeds or is exceeded by that in 1958-63 depends upon whether a is less than or greater than $+0.37$. The answer in Panel III is again unambiguous¹¹ for all possible weights, factor productivity increases in both 1951-58 and 1958-63.

Thus, something has been added in explanation of the decline in average annual rates of output increase between 1951-58 and 1958-63. It is not clear whether the rates of increase of factor inputs have similarly declined: to know this one needs to know the appropriate weights for capital and labor. No matter what this answer is, however, and no matter what the appropriate weights are, the rates of increase of factor productivity have declined between 1951-58 and 1958-63 and so account for at least part of the decline in the rates of output increase.

Moreover, in several respects the results are stronger than indicated by the direction of inequality between 1951-58 and 1958-63 rates of increase of factor productivity.

A. From equations (1) and (2), one can calculate for any given pair of periods the quantitative difference between the rates of increase of factor productivity as a function of a . Numerically, the amount by which the 1951-58 rate of increase exceeds the 1958-63 rate turns out

¹¹ But here it is easy to see: it follows from output increases that are relatively greater than the separate increases in capital and labor.

to be an increasing function of a . Illustrative calculations for 1951-58 and 1958-63 are as follows:

AVERAGE ANNUAL RATES OF INCREASE OF FACTOR PRODUCTIVITY
(per cent per year)

| | $a=0$ | $a=.2$ | $a=.4$ | $a=.6$ | $a=.8$ | $a=1.0$ |
|---------|-------|--------|--------|--------|--------|---------|
| 1951-58 | 8.6 | 7.9 | 7.1 | 6.4 | 5.7 | 5.0 |
| 1958-63 | 7.5 | 6.1 | 4.7 | 3.3 | 2.0 | 0.7 |

Thus, we know not only the direction of inequality but that the quantitative difference lies between 1.1 and 4.3 percentage points. Since the difference between the rates of output increase is 2.4 percentage points (Table 2), the decline in the rate of productivity increase accounts for a substantial amount of the decline in the rate of output increase—for most or all of it so long as $a \geq .05$.

B. Because the average annual rates of increase depend only on initial- and terminal-year values, one might be concerned that the results are strongly influenced by: (a) the selection of 1951 as the initial year of the first period, given 1958 as the dividing year between the two periods; (b) the selection of 1958 as a dividing year between the two periods, especially when the capital value for that year had to be interpolated; or (c) the selection of 1963 as the terminal year of the second period, especially when the output index might have been unduly influenced by the absolute decline in 1963 agricultural output. Accordingly, I have calculated the critical values of a as in Panel I of Table 4 for each of the following alternative pairs of periods: (a) with 1958-62 and 1958-63 as period 2, 1951-58 or 1953-58 or 1954-58 as period 1; (b) with 1959-62 and 1959-63 as period 2, 1951-59 or 1953-59 or 1954-59 as period 1. In all cases the results are the same: all possible capital shares yield an average annual rate of productivity increase in period 1 exceeding that in period 2.

C. Is it possible that the decline in the rates of productivity increase is simply a continuation of past trends and, therefore, of no particular significance? The entries in Panel I point to the uniqueness of the decline. With 1951-53 as period 1 and 1953-58 as period 2, the values of a for which $\bar{A}_1 > \bar{A}_2$ are $a > +1.10$; with 1928-40 as period 1 and 1951-58 or 1951-63 as period 2, the critical values of the capital share are $a > +2.35$ or $a > +1.46$. For these comparisons, *all possible* capital shares yield average annual rates of increase of factor productivity that are *greater* in period 2 than in period 1. (To complete the picture for the earlier periods, note from Panels II and III that the rate of increase of factor inputs clearly declines between the prewar and postwar periods, that the direction of change between 1951-53 and 1953-58 is ambiguous,

and that factor productivity increases within each of the periods represented.)

D. Because of the conjectural nature of several aspects of the index of transport and communications output, it is certainly legitimate to ask whether the observed decline in the rate of increase of factor productivity is anything but a numerical consequence of particular and sometimes arbitrary "solutions" to data problems. Although I am unable to comment on this directly, I have undertaken similar computations for industry, agriculture, the nonagricultural economy and the economy as a whole. In each of these cases: (a) the rate of output increase declines between 1951-58 and 1958-63; and (b) the search for critical values of the capital share leads to the conclusion, as here, that for all possible values of the capital share the rate of increase of factor productivity declines between 1951-58 and 1958-63. These results will be submitted in a subsequent paper.¹²

¹² In that paper I proceed in a somewhat different fashion. I assume a Cobb-Douglas linear homogeneous production function with neutral technical change. With these assumptions, the index of technical change is formally identical with the index of factor productivity as constructed here and, indeed, all the equations and inequalities used here are identical with those arising from the production function assumptions. The major difference between the two approaches is the economic meaning of a and $1-a$. With the production function assumptions, a and $1-a$ are constant distributive shares or constant proportional marginal productivities. In the present construction of an index of factor productivity, a and $1-a$ are fixed weights for capital and labor weights whose appropriate definition may be distributive shares or proportional marginal productivities but which refer only to the weight-year.

Here, I have treated the problem as a straightforward matter of index-number construction for two reasons: (a) as an economy of exposition; and (b) as an analgesic for any who might be restive with the production function assumptions or their application to transport and communications. In so doing I have lied by omission but not really very much as the following remarks are intended to show.

1. I have used the geometric form of an index of factor productivity precisely because it is consistent with a Cobb-Douglas linear homogeneous production function with neutral technical change.

2. On the other hand, the geometric form represents a perfectly legitimate index number in itself. In principle, one should check index numbers for sensitivity to alternative weights but this is not necessary in my use for I obtain results independent of the weights. Some may observe that I never specified the year to which the weights refer and that the algebra in (fn. 10), seems to imply 1951 as the weight-year when 1955 weights are used for the output index. Note, however, that: (a) an extension of the algebra in that footnote would preserve the results for 1955 weights throughout, and (b) when index numbers are compared over more than one period, it is inevitable that terms appear with weights that refer to neither year in at least one of the periods. Note, also, that the problems which arise from an admixture of arithmetic and geometric methods of aggregation (in the output and productivity indexes, respectively) arise in both the index number and production function approaches.

3. Although the geometric form of the index number is legitimate, it is less frequently used than the arithmetic form. However, the arithmetic form of a productivity index also has implicit production function assumptions and, perhaps, less credible assumptions than Cobb-Douglas: namely, a production function in which the marginal product of either factor does not depend on the quantity of the other, a production function with infinite elasticity of substitution.

4. Whatever the relative merits of the geometric and arithmetic indexes, my results do not

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depend on the choice I have made. Define indexes of factor productivity, A'_1 and A'_2 , in arithmetic form as follows:

$$A'_1 = \frac{X_1}{aK_1 + (1-a)L_1}; \quad A'_2 = \frac{K_2}{aK_2 + (1-a)L_2}$$

where X_1 , X_2 , K_1 , K_2 , L_1 , L_2 , and a have the same meanings as above where periods 1 and 2 are 1951-58 and 1958-63, respectively. Inserting the values for X , K , and L , taking logarithms and dividing by N_1 and N_2 , one can determine by numerical methods that $\bar{A}'_1 > \bar{A}'_2$ for a ≥ -0.40 , approximately. Recall the comparable figure in Table 4: $a > -0.34$.

ALTERNATIVE INTEREST RATES AND THE DEMAND FOR MONEY: THE EMPIRICAL EVIDENCE

By TONG HUN LEE*

Despite voluminous literature on the influence of various interest rates on the demand for money, opinions vary widely as to which interest rate or subset of alternative interest rates is a relevant determinant of the demand for money. Eisner [6] and Latané [19] [20] favor an interest rate on long-term bonds, since Keynesian economics links a long rate to investment and income through the demand for and supply of money. Brunner and Meltzer [3] and Hamburger [15] argue that the demand for money should be treated in a theory of broader portfolio selection and suggest that the demand for money depends on the yield on equities as well as on bonds. Bronfenbrenner and Mayer [2], Teigen [24], Heller [16], and Laidler [18], on the other hand, take a position that the demand for money is a function of a short-term interest rate, as the short rate indicates the opportunity cost of holding money in place of close substitutes. Gurley and Shaw [11] [12] [14] [13] take a step farther to argue that liquid assets such as savings deposits at nonbank financial institutions are closer substitutes for money and, therefore, that the demand for money depends on interest rates on nonbank intermediary liabilities.

Although the demand for money is theoretically a function of a whole spectrum of interest rates, the principal issue is an empirical one: Which interest rate approximates "the" rate of interest and exerts the most significant influence on the demand for money? An answer to this question has important implications for monetary theory and policy. Recent empirical works by Heller [16], Hamburger [15], and Laidler [18] in this area are limited to an investigation of a short rate versus a long rate or a long rate versus the yield on equities. They ignore interest rates that are relevant for other hypotheses. Moreover, these studies do not use the differentials between their interest rates and the yield on money such as demand deposits.¹

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¹ Hamburger and Laidler recognize that the relevant interest-rate variables should be mea-

The present paper removes these deficiencies and shows that contrary to other claims, the yield on nonbank intermediary liabilities is the most significant interest rate variable in affecting the demand for money. The conclusion is thus consistent with my earlier results published elsewhere [21] that savings deposits at nonbank financial institutions are close substitutes for money. The present study also reveals a plausibility of the Baumol-Tobin-Whalen hypothesis [1] [26] [29] of economies of scale in holding cash for transaction and precautionary purposes. It further shows an effect of close money substitutes on measuring the rate of adjustment between the desired and the actual levels of money holdings. In addition, empirical results in this paper have some bearing on the question of the proper definition of money that is relevant for monetary theory and policy.

I. *Approach and Data*

The model employed for the analysis is a variety of Friedman's permanent income formulation [8] of the demand for money. This approach is chosen partly because recent papers [17] [4] report permanent income as a better explanatory variable than nonhuman wealth and/or current income and partly because the wealth data for the recent past is not available. The demand function is specified as

$$(1) \quad M' = f(Y_p, r')$$

where Y_p is per capita real permanent income, M' denotes per capita money stock in real terms for the traditional concept of money, and r' represents a vector of interest rate differentials between yields on other assets and the yield on money. The yield on traditionally defined money is derived as the weighted average of rates of return on demand deposits (the negative of service charges) and currency (zero). The price deflator employed for real variables is current price rather than permanent price.² This static model can be cast into a dynamic stock adjustment model of Chow [4] and Teigen [24] varieties by introducing a lagged dependent variable, M'_{-1} , as

$$(2) \quad M' = g(Y_p, r', M'_{-1})$$

Demand functions (1) and (2) for the traditional concept of money can be adjusted for Friedman's concept of money. For this purpose,

sured by the interest-rate differentials. They, however, dismiss the use of this measure for the reason that the interest-rate differentials cannot be either computed with quarterly data or extended over a long period with annual data.

² Use of permanent price as a deflator in a preliminary investigation did not alter the following conclusions to any significant extent.

money defined to include time deposits, M'' , is substituted for M' for the current and lagged variables, whereas interest rate differentials between yields on other assets and the yield on money so defined, r'' , is substituted for r' . The yield on money defined to include time deposits is measured by the weighted average of rates of return on time deposits, demand deposits, and currency. Detailed definitions and symbols used in the analysis are as follows:

| Symbols | Definitions ³ |
|--|---|
| M' : | Per capita currency and demand deposits held by the public in real terms |
| M'' : | M' plus per capita commercial bank time deposits held by the public in real terms |
| Y_p : | Friedman's permanent per capita net national product in real terms |
| r'_t : | The yield on commercial bank time deposits minus that on M' |
| r'_{s1} : | The yield on savings and loan association shares minus that on M' |
| r'_s : | The yield on four-to-six month commercial paper minus that on M' |
| r'_1 : | The yield on twenty-year corporate bonds minus that on M' |
| r'_e : | Moody's dividend yield on common stocks minus the yield on M' |
| r''_{s1} ; r''_s ; r''_1 ; r''_e : | The respective yields minus the yield on M'' |

The short rate, the long rate, and the yield on equities above are chosen primarily because they are the variables that Laidler [18] and Hamburger [15] used in their tests to show the relative importance of interest rates in affecting the demand for money. The choices of these variables among other relevant measures are not of crucial importance in yielding the following results.⁴

The period covered in this study is from 1951 to 1965, a period of relatively homogeneous institutional settings, following the 1951 Federal

³ Money stock series are the simple averages of the recently revised monthly series obtained from the 1966 September issue of the *Federal Reserve Bulletin*. The yields on demand deposits and time deposits that are required in computing the interest rate differentials are obtained from issues of the *Annual Report of the Federal Deposit Insurance Corporation*. The yield on savings and loan association shares comes from the 1966 *Savings and Loan Fact Book*. Permanent per capita net national product in current dollars is computed by the same method as that used by Friedman and Schwartz [10] and is deflated by implicit price index for gross national products.

⁴ In a preliminary investigation, interest rates on short-term and long-term government securities were used rather than those on four-to-six month commercial paper and twenty-year corporate bonds. The results from this experiment did not differ from those reported in this paper.

Reserve Accord with the Treasury and the 1950 revision of the Federal Savings and Loan Insurance Corporation insurance regulations.⁵ It also captures the characteristics of money-demand function associated with the revival of monetary policy.⁶ It can be assumed that during this period the supply schedule of money has been shifted by a Federal Reserve policy less reliant on movements of interest rates and the needs of trade. Although what is known as the identification problem in econometrics is not completely resolved in this study, use of this particular period should help to identify the effects of interest rates and permanent income on the demand for money.⁷

II. *Empirical Results on Money Substitutes*

Log-linear forms of equations (1) and (2) for the traditional concept of money are fitted to the data by the least squares method, using single interest rate differentials of alternative rates. The results in Table 1 show that regression equation (1.2) or (1.7) with the yield on savings and loan shares performs the best in terms of \bar{R}^2 among respective regressions of static or dynamic formulation. Hence, regressions are recomputed in Table 2 with two interest rate differentials using both the yield on savings and loan shares and the yield on other assets. These results indicate that in all of the regression equations, the coefficients of the yield on savings and loan shares remain highly significant. The yield on time deposits appears to have a modest effect on the demand for money, but the elasticity of time deposit rate is smaller than that of the yield on savings and loan shares in regression equations (2.1) and (2.5). Regressions were also recomputed with three interest rate differentials by utilizing the yield on savings and loan shares, the yield on

⁵ In 1950 the insurance provisions of the Federal Savings and Loan Insurance Corporation were revised and made more liberal than before in the event of default of an insured savings and loan association. As I have shown elsewhere[21], the revised insurance provisions have since made savings and loan shares more comparable in quality to demand and time deposits. Therefore, if a role of near-money in affecting the demand for money is to be compared with that of other assets, one should focus attention on this period.

⁶ The data for various interest rates used in the analysis are available for years after 1933, but Friedman's permanent income series based on recently revised national statistics can only be computed for the years after 1938. The reason for the latter is that Friedman's scheme of weighting the present and past values of net national products involves 11 years and the revised net national product series goes back only to 1929. Omitting the World War II years and the bond price support years out of 1939-65 period, the available observations that are left out of this analysis are the two-year observations of 1939-40. An experiment including these two observations, however, did not alter the conclusions drawn in the following analysis.

⁷ That the identification problem is not a crucial factor affecting the following results is demonstrated by the fact that my earlier cross-sectional estimates from the household survey data [21] conform to the following results concerning the substitutability of nonbank intermediary liabilities for money. As is well known, the demand function of individual households estimated from micro-cross-section data is not likely to pose a serious identification problem.

TABLE 1—LOG-LINEAR REGRESSION EQUATIONS FOR THE TRADITIONAL CONCEPT OF MONEY USING INTEREST-RATE DIFFERENTIALS: ANNUAL DATA, 1951-1965

| | | |
|--|-------------------------|------------------|
| (1.1) $M' = 2.36 + .589 Y_p - .285 r'_t$ (5.08)** (14.9)** | $\bar{R}^2 = .981^{**}$ | $DW = 1.77^*$ |
| (1.2) $M' = 3.36 + .535 Y_p - .633 r'_{t1}$ (6.18)** (19.5)** | $\bar{R}^2 = .989^{**}$ | $DW = 1.56^*$ |
| (1.3) $M' = 12.0 + .658 Y_p - .114 r'_s$ (2.53)* (1.84) | $\bar{R}^2 = .708^{**}$ | $DW = 1.06$ |
| (1.4) $M' = 6.94 + .039 Y_p - .397 r'_1$ (.172) (5.33)** | $\bar{R}^2 = .889^{**}$ | $DW = 1.74^*$ |
| (1.5) $M' = 7.36 - .144 Y_p + .324 r'_o$ (.388) (2.59)* | $\bar{R}^2 = .794^{**}$ | $DW = 1.17$ |
| (1.6) $M' = 1.28 + .543 Y_p - .235 r'_t + .208 M'_1$ (4.55)** (5.40)** (1.26) | $\bar{R}^2 = .982^{**}$ | $DW = 2.08^*$ |
| (1.7) $M' = 3.70 + .547 Y_p - .665 r'_{t1} - .058 M'_{-1}$ (5.68)** (6.93)** (.351) | $\bar{R}^2 = .988^{**}$ | $DW = 1.68^{**}$ |
| (1.8) $M' = -.956 + .173 Y_p - .040 r'_s + .947 M'_{-1}$ (1.06) (1.38) (7.14)** | $\bar{R}^2 = .943^{**}$ | $DW = 1.99^*$ |
| (1.9) $M' = .267 + .183 Y_p - .125 r'_1 + .771 M'_{-1}$ (1.10) (1.33) (3.51)** | $\bar{R}^2 = .943^{**}$ | $DW = 1.67^{**}$ |
| (1.10) $M' = -.131 + .208 Y_p + .063 r'_s + .939 M'_{-1}$ (1.05) (.810) (5.92)** | $\bar{R}^2 = .938^{**}$ | $DW = 2.08^*$ |

Note: Numbers in parentheses are t statistics. The symbol * or ** on the t statistics and \bar{R} indicates that the relevant statistic is significantly different from zero at the 5 per cent or 1 per cent level, respectively. The symbol * or ** on the Durbin-Watson statistics indicates that the null hypothesis of residual independence in the relevant equation cannot be rejected at the 5 per cent or 1 per cent level of significance. The tests of the Durbin-Watson statistics are based on the Theil-Nagar testing procedure [25].

time deposits, and still another yield on the rest of other assets. Although not reported in the paper, the results are the same as before: savings and loan shares are the closest substitutes for money, whereas time deposits at commercial banks are modest substitutes for money. Stepwise regressions were computed in this manner adding successively one more interest rate differential, etc., but the results indicated the same conclusion as before. Finally the regression equations with all the interest rate differentials enumerated above were computed with or

TABLE 2—LOG-LINEAR REGRESSION EQUATIONS FOR THE TRADITIONAL CONCEPT OF MONEY USING TWO INTEREST-RATE DIFFERENTIALS: ANNUAL DATA, 1951-1965

| | | | |
|-------|--|-------------------------|------------------|
| (2.1) | $M' = 2.78 + .583 Y_p - .408 r'_{s1} - .106 r'_t$ (7.56)** (4.02)** (2.31)* | $\bar{R}^2 = .992^{**}$ | $DW = 2.35^*$ |
| (2.2) | $M' = 3.32 + .539 Y_p - .617 r'_{s1} - .143 r'_s$ (6.25)** (17.3)** (1.06) | $R^2 = .989^{**}$ | $DW = 2.00^*$ |
| (2.3) | $M' = 3.31 + .543 Y_p - .674 r'_{s1} + .034 r'_1$ (6.08)** (10.0)** (.696) | $\bar{R}^2 = .988^{**}$ | $DW = 1.42$ |
| (2.4) | $M' = 2.95 + .577 Y_p - .607 r'_{s1} + .040 r'_s$ (6.31)** (15.8)** (1.23) | $\bar{R}^2 = .989^{**}$ | $DW = 1.71$ |
| (2.5) | $M' = 3.19 + .599 Y_p - .445 r'_{s1} - .107 r'_t - .071 M'_{-1}$ (7.00)** (3.48)** (2.25)* (.506) | $\bar{R}^2 = .991^{**}$ | $DW = 2.48^*$ |
| (2.6) | $M' = 3.53 + .547 Y_p - .636 r'_{s1} - .014 r'_s - .034 M'_{-1}$ (5.65)** (6.33)** (.968) (.207) | $\bar{R}^2 = .988^{**}$ | $DW = 2.07^*$ |
| (2.7) | $M' = 3.61 + .554 Y_p - .701 r'_{s1} + .034 r'_1 - .052 M'_{-1}$ (5.56)** (6.18)** (.650) (.307) | $\bar{R}^2 = .987^{**}$ | $DW = 1.53$ |
| (2.8) | $M' = 3.44 + .599 Y_p - .655 r'_{s1} + .043 r'_s - .091 M'_{-1}$ (5.84)** (6.98)** (1.26) (.559) | $\bar{R}^2 = .988^{**}$ | $DW = 1.79^{**}$ |

Note: See note to Table 1 for explanation of * and **. Numbers in parentheses are *t* statistics.

without lagged dependent variable as follows:

$$\begin{aligned}
 (3) \quad M' &= 2.43 + .625 Y_p - .491 r'_{s1} - .082 r'_t - .012 r'_s \\
 &\quad (7.48)** \quad (3.72)** \quad (1.53) \quad (.794) \\
 &\quad + .056 r'_1 + .034 r'_s \\
 &\quad (1.15) \quad (1.18) \qquad \qquad \qquad \bar{R}^2 = .991^{**} \\
 &\qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad DW = 2.60^* \\
 (4) \quad M' &= 2.81 + .642 Y_p - .516 r'_{s1} - .086 r'_t - .010 r'_s \\
 &\quad (6.72)** \quad (3.45)** \quad (1.50) \quad (.616) \\
 &\quad + .052 r'_1 + .036 r'_s - .070 M'_{-1} \\
 &\quad (.993) \quad (1.18) \quad (.463) \qquad \qquad \qquad \bar{R}^2 = .990^{**} \\
 &\qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad DW = 2.62^*
 \end{aligned}$$

where all variables are in natural logarithms and figures in parentheses

are t statistics. The coefficients of the yield on savings and loan shares remain highly significant while those on time deposits now melt away after introducing all other yield variables.⁸ It is quite evident through these successive tests that savings and loan shares are the closest substitutes for money among time deposits, short-term paper, long-term bonds, and equities.⁹

The estimated demand functions for Friedman's concept of money with single and two interest rate differentials are reported in Tables 3 and 4 respectively. They again show similar results that savings and loan shares are the closest money substitutes among alternative types of assets. The upshot of this evidence can also be seen by the following regressions:

$$(5) \quad M'' = 2.65 + .670 Y_p - .688 r_{s1}'' - .037 r_s'' + .037 r_1'' - .050 r_e''$$

(2.97)* (6.93)** (1.86) (.535) (1.28)

$\bar{R}^2 = .933^{**}$
 $DW = 1.28$

$$(6) \quad M'' = 1.71 + .531 Y_p - .553 r_{s1}'' - .044 r_s'' + .049 r_1''$$

(2.09) (3.59)** (2.13) (.715)

$$- .050 r_e'' + .265 M_{-1}''$$

(1.29) (1.14) $\bar{R}^2 = .934^{**}$
 $DW = 1.18$

One must then conclude that, no matter which definition of money

⁸ Although the coefficients of the yield on time deposits become weak in above regression equations, the yield on time deposits may still be an important variable in affecting the demand for money. In regression equations (2.1) and (2.5) the yield on time deposits as well as on savings and loan shares exerts significant influence on the demand for money while in regression equations (2.2)–(2.4) and (2.6)–(2.8) the yields on other assets do not. In regressions (3) and (4), the inclusion of the yields on other assets as regressors blurs the effect of time-deposit rates on the demand for money, but the coefficients of the yields on other assets again turn out to be insignificant. The weak effect of time-deposit rates in these equations may, therefore, be partly due to the increased noise of statistical multicollinearity when the apparently insignificant variables are added as regressors in the annual time-series analysis. Another partial cause may be the use of time-series data containing the period of the 1950s in which time deposit rates were kept substantially lower than other interest rates through maximum rate control on time deposits under Regulation Q. My cross-sectional estimates covering this period [21] indicate insignificant effects of time-deposit rates on the demand for demand deposits. Thus, the present result may have been heavily weighted with annual observations of the 1950s, while changes in the yield on time deposits may, in fact, have significant effects on the demand for money in a more competitive rate structure of the 1960s.

⁹ Reasonably stable regression coefficients on the yield on savings and loan shares in the successive tests actually demonstrate that the results are not distorted by statistical multicollinearity. The use of lagged dependent variables usually removes a time trend from the regression analysis, but it does not affect the present conclusion.

is used, the demand for money is highly sensitive to changes in the yield on savings and loan shares, and savings and loan shares are the closest substitutes for money. The first of the two conclusions above can also be confirmed by my previous study [21] in which I used cross-sectional aggregate data by states as well as household survey data and employed time-series data with a limited number of interest rate variables. These findings, therefore, reject such other claims that: (1) long-

TABLE 3.—LOG-LINEAR REGRESSION EQUATIONS FOR FRIEDMAN'S CONCEPT OF MONEY USING SINGLE INTEREST-RATE DIFFERENTIALS: ANNUAL DATA, 1951-1965

| | | | |
|-------|---|-------------------------|------------------|
| (3.1) | $M'' = .544 + .928 Y_p - .644 r_{s1}''$ (12.3)** (9.79)** | $\bar{R}^2 = .920^{**}$ | $DW = .71$ |
| (3.2) | $M'' = 2.53 + .596 Y_p - .072 r_s''$ (3.25)** (1.42) | $\bar{R}^2 = .382^*$ | $DW = .51$ |
| (3.3) | $M'' = .946 + .836 Y_p - .321 r_1''$ (5.52)** (3.83)** | $\bar{R}^2 = .675^{**}$ | $DW = .89$ |
| (3.4) | $M'' = -1.87 + 1.14 Y_p + .107 r_s''$ (1.64) (.944) | $\bar{R}^2 = .329^*$ | $DW = .48$ |
| (3.5) | $M'' = .079 + .855 Y_p - .570 r_{s1}'' + .135 M_{-1}''$ (5.41)** (3.70)** (.531) | $\bar{R}^2 = .915^{**}$ | $DW = .69$ |
| (3.6) | $M'' = -2.66 + .401 Y_p - .053 r_s'' + .942 M_{-1}''$ (4.50)** (2.26)* (6.76)** | $\bar{R}^2 = .869^{**}$ | $DW = 1.80^{**}$ |
| (3.7) | $M'' = -2.01 + .485 Y_p - .116 r_1'' + .771 M_{-1}''$ (3.35)** (1.42) (3.62)** | $\bar{R}^2 = .838^{**}$ | $DW = 1.26$ |
| (3.8) | $M'' = -1.78 + .238 Y_p - .015 r_s'' + .993 M_{-1}''$ (.590) (.226) (5.59)** | $\bar{R}^2 = .810^{**}$ | $DW = 1.44^*$ |

Note: See note to Table 1 for explanation of * and **. Numbers in parentheses are *t* statistics.

term bonds or long-term bonds and physical assets are closer substitutes for money than are other assets as indicated by Meltzer [23], Brunner and Meltzer [3], and Hamburger [15]; (2) the demand for money can be explained in terms of the short-rate movements alone besides permanent income or wealth as in the studies by Laidler [18] and Heller [16]; (3) as Friedman and his associates [8] [9] [10] believe, time deposits are close substitutes for money while savings and loan shares are not; and finally (4) the demand for money is independent of or even complementary with respect to the demand for savings and loan shares as advocated in Feige's study [7].

TABLE 4—LOG-LINEAR REGRESSION EQUATIONS FOR FRIEDMAN'S CONCEPT OF MONEY
USING TWO INTEREST-RATE DIFFERENTIALS: ANNUAL DATA, 1951-1965

| | | | |
|-------|--|-------------------------|------------|
| (4.1) | $M'' = .374 + .950 Y_p - .617 r_{s1}'' - .031 r_1''$ (13.5)** (9.89)** (1.77) | $\bar{R}^2 = .932^{**}$ | $DW = .84$ |
| (4.2) | $M'' = .499 + .934 Y_p - .617 r_{s1}'' - .022 r_1''$ (11.7)** (5.83)** (.335) | $\bar{R}^2 = .913^{**}$ | $DW = .67$ |
| (4.3) | $M'' = 2.64 + .670 Y_p - .674 r_{s1}'' - .047 r_s''$ (2.76)* (9.56)** (1.12) | $\bar{R}^2 = .938^{**}$ | $DW = .85$ |
| (4.4) | $M'' = -.474 + .824 Y_p - .483 r_{s1}'' - .035 r_s'' + .239 M_{-1}''$ (5.82)** (3.35)** (1.97) (1.03) | $\bar{R}^2 = .932^{**}$ | $DW = .85$ |
| (4.5) | $M'' = .040 + .862 Y_p - .545 r_{s1}'' - .022 r_1'' + .133 M_{-1}''$ (5.19)** (302)* (3.14) (.502) | $\bar{R}^2 = .907^{**}$ | $DW = .65$ |
| (4.6) | $M'' = 2.17 + .601 Y_p - .602 r_{s1}'' - .047 r_s'' + .131 M_{-1}''$ (2.12) (3.86)** (1.07) (.518) | $\bar{R}^2 = .916^{**}$ | $DW = .77$ |

Note: See note to Table 1 for explanation of * and **. Numbers in parentheses are *t* statistics.

III. Implications for Other Related Hypotheses

The present study has important bearings on a number of controversies connected with monetary theory and policy. One of them is related to "the" proper definition of money. Traditionally, money is defined to include currency and demand deposits for the reason that these assets represent means of payment. On the other hand, Friedman and Schwartz [10, p. 650] propose to broaden the definition of money by arguing that the term, money, should "refer not solely to a medium of exchange but also . . . to a temporary abode of purchasing power. . . ." Thus, Friedman and his associates [8] [9] [10] adopt a broad concept of money to include time deposits with the assumption that time deposits are close or even perfect substitutes for currency and demand deposits while savings and loan shares are not. The findings of the preceding section do not support their assumption. My earlier results from cross-section data reported elsewhere [21] also indicated that savings and loan shares were closer substitutes than were time deposits.

Laidler [18] among others argues that Friedman's concept of money is superior to the traditional one, since the predictability of the money demand function for Friedman's concept is better than that for the traditional concept. Comparisons of \bar{R}^2 for the relevant regressions of the preceding section show that when Laidler's criterion is used, Friedman's concept of money works much worse than the traditional concept of

money.¹⁰ Laidler further argues that the estimated demand functions for Friedman's concept of money are much stabler over time than those for the traditional concept of money. Although this may be one of the criteria for choosing a particular definition of money, the stability question cannot be answered from regression analysis unless the money demand function is correctly specified. The demand functions investigated by Laidler do not incorporate the yield on savings and loan shares as an independent variable. Moreover, as Heller [16] shows, the tests of the stability of regression estimates from annual time series are misleading. In fact, using quarterly data, Heller concludes that Friedman's as well as the traditional concept of money yields equivalently stable money demand functions. Heller's conclusion cannot be investigated in this study owing to the lack of quarterly data on the yields on savings and loan shares and time deposits. But it is clear that Friedman and his associates as well as Laidler are not able to justify their reasoning for proposing the broad definition of money. Although the proper definition of money relevant for monetary theory and policy is an open question, if their criteria are used, the results in this paper provide evidence against Friedman's definition of money.

The estimated coefficients of lagged dependent variables in this study also have some implications for the question of how rapidly income responds to money-supply changes. Tucker [27] in a recent article shows under a dynamic Keynesian framework that if distributed lags in the demand for money in response to changes in interest rates exist, the money-demand lag tends to offset long distributed lags in investment in response to changes in interest rates. Thus, utilizing the money-demand lag and other parameters estimated by prior studies, Tucker attempts to show that the money-supply changes affect income and production rather rapidly. The money-demand lag used by Tucker and those estimated by others, however, do not reflect the speed of adjustment when there are close substitutes for money such as savings and loan shares. In all of the functions estimated for both the traditional and Friedman concepts of money, the coefficients of lagged dependent variables become statistically insignificant whenever the yield on savings and loan shares and/or the yield on time deposits are introduced in the demand functions. This result is not surprising. When money holders can substitute highly liquid assets for money without incurring much time, effort, and transaction costs, as is likely to be the case for savings deposits, the behavioral lags of actual money holdings in response to

¹⁰ Although not reported in the paper, the standard errors of estimates of the regression equations for Friedman's concept of money are larger than those of the corresponding regression equations for the traditional concept of money. It should also be noted that the former regressions do not show the independence of residual disturbances while the latter regressions do in the relevant cases.

changes in yields on savings accounts may be quite small or even zero.¹¹ If this is the case, a quick response of income and production to monetary policy has to be explained through other channels of financial behavior rather than the offsetting effect of money-demand lag on the lags involved in investment.¹²

Recent theories on the holdings of cash balances indicate that when money balances are held for transaction and precautionary purposes, there exist economies of scale in holding money balances. Baumol [1] and Tobin [26] show that optimal transaction demand for cash varies less than in proportion to changes in the volume of transactions or income. Whalen [29] on the other hand shows a similar relationship between optimal precautionary demand for cash and the volume of transactions. Money defined to include currency and demand deposits especially for the period of this analysis is likely to consist of cash balances held for transaction and precautionary purposes rather than for speculative purposes. Since cash balances held for speculative purposes are not tied to current needs of transactions, they can easily be siphoned into liquid, riskless, and interest-bearing assets while awaiting the opportunities for reinvestment in long-term securities. Such liquid assets yielding relatively high rates of return have indeed become more available since 1950. Savings and loan shares became more liquid and secure than before [see footnote 5] and their yields have been kept substantially higher than they were before [see 21]. Moreover, rates on time deposits became more competitive with other market rates in recent years, and an active use of negotiable certificates of deposit in the 1960s would have added to the temporary investment media for speculative demand for cash balances. Therefore, if permanent income is regarded as a measure of the volume of transactions, it is not surprising to find that the permanent income elasticity of the demand for money exclusive of time deposits is less than unity. The estimated permanent income elasticities for the traditional concept of money are significantly less than unity in the relevant regressions (1.2), (2.1)–(2.4), and (3), whereas those for Friedman's concept of money are not statistically

¹¹ Use of quarterly rather than annual data may exhibit a lagged relationship of the money demand function, even when rates on savings and loan shares and time deposits are introduced in the demand functions. Unfortunately, quarterly data on these rates are not yet available. Evidence from annual time series here, however, is sufficient to indicate that the quarterly estimates of time lags in the demand for money reported in other studies [24] [5] are likely to contain considerable upward biases. Hamburger [15] uses a framework which differs from the present one to estimate the time lags involved in household responses to changes in interest rates, but he uses neither the yields on savings and loan shares and time deposits nor the interest rates on short-term securities.

¹² Tucker actually carries out his arguments on the basis of linear rather than log-linear models. Experiments with the linear money-demand functions did not affect the present conclusion. They, in fact, did not affect the other findings of this paper to any great extent.

different from unity in the regressions (3.1), (4.1)–(4.3), and (5).¹³ One may, therefore, observe that there has been improved cash management at least since 1951, not only as a result of an increasing availability of liquid assets but also as a result of higher interest rates on such assets. The world of cash management postulated by Baumol, Tobin, and Whalen, when they emphasized the role of opportunity cost in holding cash balances, may after all depict the financial practices of a modern era.¹⁴

IV. *Conclusions*

Empirical results of this paper show that the relative importance of savings and loan shares with respect to other assets as money substitutes is the most outstanding. This is so whether money is defined to include or exclude time deposits. The evidence is consistent with the substitution hypothesis of Gurley and Shaw and also with my empirical results shown elsewhere that nonbank intermediary liabilities are close substitutes for money.

Since the demand for money is highly sensitive to changes in the yield on nonbank intermediary liabilities, monetary economists henceforth ought to pay more attention to the developments of nonbank financial intermediaries and to their role in affecting the demand for money and the velocity movement in the short run as well as in the long run. The importance of the existence of such institutions should

¹³ One may note that the permanent income elasticity for Friedman's concept of money for the period after 1950 is smaller than, though not significantly different from, unity, whereas the elasticity for the period including earlier years was significantly greater than unity in my previous estimate [21, p. 445]. The postwar rise in income velocity which Friedman *et al.* [8] [10] cannot explain may partly be accounted for in terms of an improved cash management after 1950. Friedman *et al.* explain the postwar rise in velocity with the ad hoc argument of the public's favorable economic expectations concerning postwar economic stability [10, pp. 637–75]. Although Friedman *et al.* may be correct in attributing the velocity rise during the 1946–50 period to postwar economic stability (see for example [21, p. 446]), such an explanation cannot solely account for the rise in velocity for such a long period after 1950. Instead, it can be argued that a more efficient cash management coupled with the increasing attractiveness of nonbank intermediary liabilities is responsible for a reversal after the war of, what Friedman *et al.* call, the secular velocity decline. The evidence that an increasing attractiveness of nonbank intermediary liabilities can account for the postwar rise in velocity is shown in my earlier paper [21, pp. 446–47].

¹⁴ A number of empirical studies in the past [8] [23] [22] [16] [28], however, report conflicting evidence as to the presence of economies of scale in holding money balances. Although space limitation precludes detailed comparisons with the present estimates, they differ mainly in three or more of the following respects: (i) the period of analysis does not conform to that of present analysis; (ii) interest rates on liquid assets are not incorporated in the statistical demand function; (iii) an opportunity cost measured by the differential between the yield on alternative assets and the yield on money is neglected; (iv) the definition of money includes time deposits; (v) permanent rather than current price is used in deflating money stock series; and finally (vi) wealth or current income rather than permanent income is used in measuring the relevant elasticity.

also be reflected in the current theories of the term structure of interest rates. The relevant issue in the term structure theory may be the relationship between the yield on nonbank intermediary liabilities and the long-term or the short-term interest rate rather than that between the short-term and the long-term rates.

The results in this study do not support the rationale used by Friedman *et al.* for the broad definition of money. The study also shows evidence that distributed lags of the demand for money in response to interest rate changes are negligible when the yields on close cash substitutes are incorporated into the money demand function. The evidence, therefore, does not support Tucker's explanation that a speedy reaction of economic activities to money-supply changes can take place through the offsetting forces of lags in investment and lags in money demand within a simultaneous economic system. This study, contrary to empirical results by others, further shows a plausibility of the Baumol-Tobin-Whalen hypothesis that there are economies of scale in holding cash for transaction and precautionary motives.

The several questions posed in this paper can generally be explained in terms of an increasing availability and attractiveness of close cash substitutes. There are other obvious ramifications in these questions that cannot be handled in a short paper. Nevertheless, this paper shows that liquidity should be an important consideration in studying the demand for money and a significant degree of the liquidity is generated by the presence of nonbank intermediary liabilities of recent years.

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CHANGES IN THE COSTS OF TREATMENT OF SELECTED ILLNESSES, 1951-65

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The idea of a medical care price index based on the average costs of treatment of specific illnesses rather than on the prices of selected items of medical care as is the Bureau of Labor Statistics medical care price index has been in the air for quite a while. Some years ago, the writer gave a paper describing how such an index could be constructed and discussing its advantages over the present index [2, pp. 128-42]. The research project some of whose results this paper analyzes was the direct outcome of this discussion and had a twofold purpose: (1) to explore if it was feasible to estimate average costs of treatment of specific illnesses, and (2) if it was feasible, to estimate the average costs of treatment of a number of illnesses in two different periods and compare their cost changes with the price changes indicated by the medical care price index. The present paper is limited to this second point and concentrates on the principal factors that explain at least to some extent why the cost figures we obtained show a considerably greater increase than the medical care price index shows. To sum up the findings regarding feasibility, the cost-per-episode-of-illness approach was found workable by and large, although some illnesses lend themselves better to it than others. However, it is probably a considerably more costly method than that used by the BLS. An index of this type should therefore not be considered a substitute for the present medical care price index but a possible additional index that would be prepared every few years for purposes of comparison and evaluation of the present index.

To give a brief description of the study, it was carried out under the auspices of the Palo Alto Medical Research Foundation, and the patients studied were treated by physicians of the Palo Alto Medical Clinic in Palo Alto, California, a group practice of about 100 physi-

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cians in 1964. Data on treatment and costs were collected from the medical histories and financial records of the Palo Alto Medical Clinic and of the Palo Alto-Stanford Hospital where patients of Palo Alto Medical Clinic physicians are treated when they need hospitalization; for 1964-65, some data were also obtained from the patients themselves who were contacted by questionnaire. With the help of a committee of Palo Alto Medical Clinic physicians, the following ten illnesses were originally selected for study in two periods, 1951-52 (the earliest year for which the Palo Alto-Stanford Hospital still had the patients' ledger cards) and 1964-65: otitis media in children, fracture of the forearm, acute cystitis, treated hypertension, pneumonia proved by x-ray, duodenal ulcer, coronary occlusion, maternity care, acute appendicitis, and cancer of the breast. The selection of these illnesses was guided by the following criteria: the illnesses should be common illnesses or representative of a group of common illnesses (e.g., pneumonia was chosen as an example of respiratory illnesses); the sample should include illnesses usually treated entirely on an out-patient basis (e.g., otitis media, acute cystitis), illnesses that always require hospitalization (e.g., appendicitis), illnesses treated on either basis depending on the individual case (e.g., pneumonia, duodenal ulcer), and illnesses which require both forms of treatment (e.g., maternity care, cancer of the breast); the sample should cover all age groups; and the sample should contain roughly the same number of male and female cases. Fairly early in the study, however, it became apparent that we could not get enough usable cases for 1951-52 for five of these illnesses, partly because it would have involved sampling more medical records than we could afford to sample (since the Palo Alto Medical Clinic has only a very incomplete diagnostic file, we had to get our 1951-52 cases by going through old medical records), but even more because the early medical records for these illnesses were rather poor. As a result, the study covered only five illnesses: acute appendicitis, maternity care, otitis media in children, fracture of the forearm (also in children only), and cancer of the breast.

I. Summary of Findings

In the fourteen years from 1951-52 to 1964-65, the costs of treatment of all five illnesses covered by the study (with one minor exception) increased more—some of them substantially more—than the BLS medical care price index. The medical care price index for the United States as a whole rose by 55 to 59 per cent during this period (55 per cent for 1964, 59 per cent for 1965). By contrast, we found the following increases:

TABLE 1—PERCENTAGE INCREASE IN AVERAGE COSTS OF TREATMENT, 1951-52 TO 1964-65

| Illness | Percentage Increase |
|---|---------------------|
| Acute appendicitis | 87 |
| Maternity care | 72 |
| Otitis media in children | 68 |
| Cancer of the breast | 106 |
| Fracture of the forearm ^a | |
| 1. Splint only | 75 |
| 2. Cast only | 55 |
| 3. Closed reduction, physician's office | 110 |
| 4. Closed reduction, hospital | 167 |
| or: | |
| 3. Closed reduction, local or no anesthetic | 87 |
| 4. Closed reduction, general anesthetic | 315 |

^a The cases are grouped by degree of severity as indicated by type of treatment. The second grouping of the cases in categories 3 and 4 is more meaningful because in 1951-52 category 3, "Closed reduction, physician's office," included some cases severe enough to require a general anesthetic which was, however, given in the physician's office, a practice no longer followed in 1964-65.

The differences between the BLS index and our indexes become slightly less if instead of the overall medical care price index we use the BLS index for medical care excluding health insurance, which is more appropriate for this comparison and which rose by 59 to 63 per cent, respectively. Furthermore, according to the BLS, the medical care price index for San Francisco rose slightly more than the national index. Even if we add, say, another five percentage points, the differences would still be pronounced.

II. *Factors Contributing to the Differences Between our Indexes and the BLS Index*

Analysis of our data shows that at least a partial explanation of the differences between the BLS index and our indexes lies in the fact that our cost figures take account of a number of factors affecting costs which the BLS index does not take account of, and most of which make for a greater difference in costs than that measured by the BLS index. (This is not to say that the BLS index *should* take account of all of these factors; this question will be dealt with later.) The importance of these factors varies between different illnesses. Wherever possible, we have tried in our analysis to give at least a rough idea of the order of magnitude of their effect on costs of treatment and hence on the indexes of the illnesses covered by the study.¹

¹ It might be expected that because in our figures for the illnesses requiring hospitalization (appendicitis, maternity care, breast cancer, and forearm fractures treated in the hospital), hospital costs of room and board have a greater weight than they do in the BLS index (11-35 per cent as against 4-9 per cent), this would account for some of the

Changes in Treatment

The most obvious of these factors is changes in treatment which our figures reflect but the BLS index not only does not reflect (except very partially at the occasion of periodic revisions) but deliberately excludes,² since its purpose, like that of the whole Consumer Price Index, is "to measure only the change in price for items of the same quality and quantity customarily bought by urban wage-earner and clerical worker families" [1, p. 1057]. Some of these changes in treatment raised costs (e.g., an increase in the number and variety of laboratory tests or x-rays, or an increase in the use of private or semi-private rooms in place of wards); others, while they did not actually lower costs (at least in our study we did not come across any that did), kept costs from rising as much as they would have risen as a result of price rises only (e.g., a decrease in the average length of hospitalization, or a decline in the percentage of home visits). We found that in the case of appendicitis, breast cancer, and forearm fractures requiring a general anesthetic, the *net* effect of changes in treatment was to raise costs more than they would have risen if only prices had changed. In other words, part of the differences between the BLS index and our indexes for these conditions is due to changes in treatment.³

differences between our indexes for these conditions and the BLS index since the hospital room rate has shown the sharpest rise of all medical care items (139 per cent according to the BLS, about 117 per cent according to our findings). Actually, it does so to a very limited extent only. Changes in treatment and prices raising the relative importance of other medical services, combined with a decrease in the average length of hospitalization in the case of maternity care and breast cancer (i.e., a decrease in the weight given to the hospital room rate as against a constant weight in the BLS index) explain why this factor is relatively unimportant. The increase in costs *exclusive of costs of hospital room and board in both periods* would give us the following increases in costs of treatment during the period covered by our study:

| | |
|--|------------------------|
| Appendicitis | 74% (instead of 87%) |
| Maternity care | 70% (instead of 72%) |
| Breast cancer | 121% (instead of 106%) |
| Forearm fractures requiring hospitalization | 168% (instead of 167%) |
| Forearm fractures requiring a general anesthetic | 297% (instead of 315%) |

² The periodic revisions of the BLS index to take account of changes in family spending do not give full weight to the effect of changes in treatment since additional items are linked in so as not to affect the level of the index at that time. We are using the term "treatment" in a very wide sense, covering not only medical items such as physician visits, laboratory tests, x-rays and length of hospitalization but also type of hospital accommodation (ward, semi-private room or private room) and type of physician visit (office call, home call).

³ In the case of otitis media, the net effect of changes in treatment was to have slowed down the rise in costs resulting from price increases, almost entirely because of the drastic reduction in the percentage of home visits from 35 per cent to 3 per cent of all physician visits; if treatment had remained as in 1951-52, average costs of treatment would have

If treatment in 1964-65 had been the same as in 1951-52, average costs of treatment of appendicitis would have risen by 81 per cent instead of 87 per cent⁴ and those of breast cancer by 78 per cent instead of 106 per cent. In the case of forearm fractures requiring a general anesthetic, the effect of changes in treatment is much more dramatic, accounting for about one-half of the 315 per cent increase in costs of treatment. For in the earlier period, physicians still quite frequently administered a general anesthetic in their office when setting a fracture whereas now this is always done in the hospital.

Medical Care Items Not Priced Separately in the BLS Index

Another factor contributing to the differences between the BLS index and our indexes is the inclusion in our cost data of a number of medical services at their actual prices which the BLS until the 1963 revision of the index did not price separately. The most important of these services are laboratory tests and x-rays in and out of the hospital, use of the operating room and delivery room in the hospital, and anesthesiologists' services. To take account of these items for which it did not collect prices, the BLS assumed that their prices changed by the average amount of all medical care items (exclusive of health insurance) for which it did collect prices. For the period 1951-52 to 1964-65, this is 59 to 63 per cent.⁵ According to our findings, however, the prices of these ancillary services rose considerably more. We found that the prices of laboratory tests rose by 50 to 100 per cent, with an average of 60 to 70 per cent; the prices of x-rays by 55 to 100 per cent, with an average of 60 to 70 per cent; and the operating room and delivery room rates by 108 and 122 per cent, respectively.⁶ Table 2

risen by 105 per cent instead of 68 per cent. In the case of maternity care, the net effect of changes in treatment was neutral, the cost of additional services (mainly laboratory tests) being just about offset by the savings due to the reduction of the average length of hospitalization by almost one day. Treatment of forearm fractures not requiring a general anesthetic was the same in both periods; see, however, pp. 1189-90 below.

⁴ Another factor raising costs above what they would have been as a result of price rises only is the increase in the incidence of perforated cases from about 5 per cent of all cases in 1951-52 to almost 15 per cent in 1964-65; in the latter period, average costs of treatment of these cases were about 64 per cent higher than those of simple acute cases. If both treatment and incidence of severe cases had been the same in 1964-65 as in 1951-52, costs of treatment of appendicitis would have risen only 70 per cent.

⁵ Anesthesiologists' fees were assumed to change like the average fees for physician services for which the BLS collected data. For the period covered by our study, this would mean an increase of about 54 per cent. Our data show a rise of 80 to 90 per cent.

⁶ Operating room charges as they appeared on hospital ledger cards rose by only about 64 per cent, and delivery room charges by 33 per cent. However, in 1951-52 these charges included anesthesia supplies for which there was a separate charge in 1964-65. The above figures of 108 and 122 per cent refer to the increase in operating and delivery room charges including anesthesia supplies. We found that this practice of breaking up charges into separate items has become considerably more common in recent years and has to be watched for when making price comparisons between different years.

TABLE 2—RELATIVE IMPORTANCE IN AVERAGE COSTS OF TREATMENT OF ITEMS NOT PRICED SEPARATELY IN THE BLS MEDICAL CARE PRICE INDEX, BY ILLNESS, 1951-52 AND 1964-65

| Illness | Percentage of Average Costs | |
|---|-----------------------------|---------|
| | 1951-52 | 1964-65 |
| Appendicitis | | |
| Anesthetist fee | 7.2 | 7.5 |
| Laboratory tests | 4.0 | 7.5 |
| Operating room (and post-operative room in 1964-65) | 12.2 | 15.0 |
| Intravenous solution | .5 | 1.8 |
| Total | 23.9 | 31.8 |
| Maternity care | | |
| Anesthetist fee | 7.0 | 5.3 |
| Laboratory tests | 1.9 | 5.6 |
| Delivery room | 10.3 | 13.1 |
| Total | 19.2 | 24.0 |
| Cancer of the breast | | |
| Anesthetist fee | 6.4 | 5.4 |
| Laboratory tests | 4.7 | 4.8 |
| Operating room (and post-operative room in 1964/65) | 8.6 | 10.7 |
| x-rays, diagnostic | 1.0 | 2.3 |
| Radiation therapy | 3.7 | 14.2 |
| Medical/surgical supplies | 4.1 | 1.9 |
| Total | 28.5 | 39.3 |
| Fracture of the forearm, splint only | | |
| x-rays | 36.1 | 43.5 |
| Fracture of the forearm, cast only | | |
| x-rays | 32.3 | 36.8 |
| Fracture of the forearm, closed reduction, physician's office | | |
| x-rays | 36.1 | 26.6 |
| Fracture of the forearm, closed reduction, hospital | | |
| x-rays | 21.4 | 16.5 |
| Operating room | 7.2 | 8.0 |
| Anesthetist fee | 12.0 | 10.0 |
| Total | 40.6 | 34.5 |

gives an idea of the relative importance of these items in our cost data for the different illnesses in the two periods. As the table shows, these items accounted for 19 to 40 per cent of total costs of treatment, depending on the illness. It goes without saying that their relative impor-

tance in total medical care costs would be considerably less; we shall deal with this briefly later on.

*Differences Between Customary Fee and Average Fee:
the Closing Gap*

The difference between the BLS method of estimating changes in physicians' fees and our way of arriving at them is yet another reason why our figures show a greater cost increase than the BLS index. When the BLS compares physicians' fees for the same service in two periods, it compares the fees physicians report as their customary fees. By contrast, in our study we compared actual average fees at different times. We found considerable evidence not only that in the past there was much more variation around this customary fee than there is today, but also that the actual average fee charged often was substantially below the customary fee, whereas today the two either coincide or are very close. To put this differently, there is evidence that in the past physicians adjusted their fees more to patients' ability to pay, with discounts having been more general (and possibly more substantial) than higher charges. As an example, the table below shows pediatricians' customary and average fees in the two periods which we found in our study of otitis media, and the increase in customary as against average fees.

TABLE 3—CUSTOMARY AND AVERAGE FEES FOR OFFICE AND HOME VISITS
BY PEDIATRICIANS, OTITIS MEDIA, 1951-52 AND 1964-65

| | 1951-52 | 1964-65 | Percentage Increase |
|---|---------|----------------------|---------------------|
| Customary fee for office visit | \$ 4.00 | \$ 7.50 | 87.5 |
| Average fee for office visit | \$ 2.74 | \$ 6.70 | 144.5 |
| Customary fee for home visit | \$ 6.00 | \$12.00 ^a | 100.0 |
| Average fee for home visit | \$ 5.33 | \$12.33 | 131.3 |
| Average fee for office visit as a percentage of customary fee | 68.5 | 89.3 | — |
| Average fee for home visit as a percentage of customary fee | 88.8 | 102.8 | — |

^a This was the customary fee in the daytime. After hours and on weekends it was \$15.00. This explains why the average fee as a percentage of the customary fee for home calls was more than 100 per cent in 1964-1965.

As the table shows, the customary fee for an office visit rose by 88 per cent while the average fee increased by 145 per cent; similarly, the customary fee for a home visit doubled while the average fee rose by 131 per cent. Other examples, although less striking, are surgeons' fees for an appendectomy and their fees for a mastectomy. While the custo-

mary fee for an appendectomy was \$200 in 1951-52 and \$250 in 1964-65, an increase of 25 per cent, the actual average fee in 1951-52 was \$182 (or 91 per cent of the customary fee) as against \$251 (about the same as the customary fee) in 1964-65, an increase of 38 per cent. The corresponding figures for the mastectomy fee are an increase of 43 per cent from \$350 to \$500 in the customary fee, and 58 per cent from \$314 (about 90 per cent of the customary fee) to \$495 in the average fee.

While we did not find this difference between customary fee and average fee, and a diminishing gap between the two, in all the illnesses we covered (for example, in the case of maternity care the two fees roughly coincided in both periods), we nevertheless believe it to be quite an important factor. For it should be pointed out that when calculating average fees we did not count charges that physicians were unable to collect as zero charges: we counted actual charges, whether or not they were paid. Our study of patient ledger cards indicates that the percentage of uncollectable charges has diminished considerably over the period covered by our study. If we took this additional factor into account, therefore, the difference between customary and average fees would have been even larger in 1951-52 and hence the increase in fees and in average costs of treatment greater than our figures show.

Increased Specialization in Medicine

A further factor affecting relative costs in the two periods which is reflected in some of our indexes but not in the BLS index is the increased use of specialists. In our study, this played an important role in raising the costs of treatment of forearm fractures. We found that in the early 1950s, about 60 per cent of such cases were treated by orthopedic surgeons and 40 per cent by other physicians (mainly general practitioners and general surgeons); by 1964-65, orthopedic surgeons treated 89 per cent of all cases, and the remaining 11 per cent treated by other physicians were only the simplest cases requiring a splint only. We also found that in 1951-52—and in 1964-65 in the case of the one form of fracture still treated by physicians other than orthopedic surgeons—fees of orthopedic surgeons for treating a given type of fracture were considerably higher than those of other physicians. For example, in 1951-52 the fee for a closed reduction in the physician's office was about 75 per cent higher if it was done by an orthopedic surgeon rather than by a general practitioner or general surgeon. Similarly, in 1964-65, the fee for treating a fracture requiring a splint only was about double if the attending physician was an orthopedic surgeon rather than a nonspecialist.⁷ To give a rough idea of the effect of this factor on relative costs in the two periods, we have estimated that the

⁷ Actually, we should say a "less specialized specialist" since these days almost every physician is a specialist.

cost of treatment of a forearm fracture requiring a general reduction in the physician's office would have risen by about 56 per cent instead of 110 per cent had it not been for this change-over to specialists. This agrees pretty well with the 55 per cent increase in costs which we found for cases requiring a cast only, all of whom happened to be treated by orthopedic surgeons in both periods.

Change in City Status of Palo Alto

Finally, there is one factor which may have affected our indexes to some extent, causing them to show a greater increase than the BLS index, and that is the change in the status of Palo Alto (where, as mentioned earlier, our study was carried out) from relatively small-town to city status during the period covered by our study. The prices of many goods, including many medical services, tend to be higher in large than in small cities. As a city grows in size, therefore, we would expect these prices to rise quite apart from any general price increases or changes in quality. This may have happened in Palo Alto which in the early 1950s still had some small-town characteristics but by the mid-1960s had not only doubled in size but become part of the metropolitan San Francisco area.

We have little data with which to substantiate this theory, much less to evaluate its importance. The BLS has some data for 1965 that show that physicians' fees for a number of services are higher in large than in small cities. We did a little investigation of pediatricians' fees in Palo Alto and San Francisco in 1951-52 and 1964-65 which showed that in the earlier period the customary fee for a routine office visit apparently was \$4.00 in Palo Alto and \$5.00 in San Francisco while today it seems to be the same in both cities. In other words, the increase was 50 per cent in San Francisco and 87 per cent in Palo Alto. In view of the informality of our investigation, however, we would hesitate to regard these findings as conclusive, especially since we found some evidence that the fees for some other medical services were the same in both cities in both periods; for example, the customary fee for a routine office visit by an internist seems to have been \$5.00 in both Palo Alto and San Francisco in 1951-52, and \$7.50 in both cities in 1964-65, an increase of 50 per cent. Moreover, the same BLS data just mentioned show that many drugs are cheaper in large than in small cities, undoubtedly because of the greater prevalence of cut-rate drug-stores in the former. In view of our lack of adequate data, all we can say is that this factor may possibly have caused some of our indexes to overstate the increase in costs. We doubt, however, that this factor was very important since in both periods Palo Alto was a relatively high-income community where prices probably were pretty much like those in San Francisco.

III. *Evaluation of the BLS Medical Care Price Index in the Light of Our Findings*

What, if anything, can we say about the medical care price index in the light of our findings? It may be presumptuous to attempt even a tentative evaluation on the basis of such a limited study as ours, but we believe a few general statements can be made.

Leaving aside for the time being the problem of quality change (i.e., changes in treatment and the increase in specialization), let us see

TABLE 4—PERCENTAGE INCREASE IN THE PRICES OF SELECTED ITEMS OF MEDICAL CARE,
BLS INDEX AND OUR INDEXES

| | BLS Index 1951-1965 | Our Indexes 1951-52-1964-65 |
|---|--------------------------|--------------------------------|
| Physician fee for office visit ^a | 53.6 | 144.5 (87.5) ^b |
| Physician fee for home visit ^a | 60.3 | 131.3 (100.0) ^b |
| Surgeon's fee for appendectomy | 22.5 ^c | 38.1 |
| Obstetrical case | 63.6 | 56.9 |
| Hospital room rate | 139.2 | 117.1 |
| Drugs and prescriptions | | |
| Antibiotics | 10.1 (30.8) ^d | 31.7 ^e |
| Other drugs | | 31.0 ^e |

^a Fee of family doctor in BLS index, of pediatrician in ours.

^b The figure in parentheses refers to the increase in the customary fee, the other figure to that of the average fee.

^c Increase 1951 to 1963; pricing of this service was discontinued in 1963.

^d The figure in parentheses refers to the increase in the drug index for San Francisco (unpublished BLS data).

^e Based on prices of drugs used in the treatment of otitis media. "Other drugs" are mainly antihistamines, ear and nose drops, and cough medicines.

what we can say about the BLS index, accepting its basic principle of measuring only changes in price, holding the quality and quantity of goods bought constant. Table 4 presents the percentage increase in the prices of a number of medical services as shown by the BLS index and by our indexes.

To take these up item by item, it can be seen that the two sets of indexes are not too far apart on two items: the fee for obstetrical care and the hospital room rate. The same is true for drugs and prescriptions if we compare our index with the (unpublished) BLS drug index for San Francisco which rose by 30.8 per cent rather than with that for the United States as a whole. The difference in the increase of the fee for an appendectomy would probably be somewhat smaller if the BLS figure were for 1965 instead of 1963 (at which time the BLS stopped pricing this service); moreover, what is more important, as was pointed out earlier, the 38.1 per cent increase shown by our data refers to the increase in the actual average fee while we found that the cus-

tomy fee had risen by only 25 per cent, i.e., almost the same amount as shown by the BLS index. It is quite possible, therefore, that as a result of comparing customary instead of average fees, the BLS index for this service understates the increase.

The differences in the indexes for physicians' office and home visits are more difficult to evaluate. Even if we compare the BLS indexes for these services with the indexes we obtained for customary rather than average fees, the differences in the increases shown are substantial (87.5 per cent for our index as against 53.6 per cent for the BLS index in the case of an office visit). As mentioned earlier, we have some reason to believe that pediatricians' fees for office visits, having been lower than those of other physicians in 1951-52 and having caught up with them by now, rose more than those of other physicians. However, assuming that customary fees rose by only 50 per cent (i.e., the probable increase in internists' fees in Palo Alto), our figures would still show a greater increase than the BLS figures if we compare average rather than customary fees; for example, if we assume that the average physicians' fee was 10 per cent lower than the customary fee in 1951-52 (a conservative estimate, in our opinion) but the same as the customary fee in 1964-65, our index would show an increase of 67 per cent as against the BLS figure of 53.6 per cent. Taking everything into account, therefore, we tend to believe that the BLS index for physician visits is on the low side.

To the above possible downward biases of the medical care price index must be added that caused by the BLS method (before the 1963 revision) of estimating changes in the prices of ancillary services; as stated earlier, the BLS index assumed that they rose by 59 to 63 per cent respectively whereas our data indicate that they rose, on the average, by 60 to over 100 per cent. While expenditures on these services are not a major item in family expenditures on medical care, they are not negligible. According to the 1950 survey of consumer expenditures (the latest for which published data are available) [3, p. 10], they accounted for 6 to 14 per cent of average family expenditures on medical care in San Francisco.⁸ An underestimate of their price increase, therefore, would be important enough to be reflected in the medical care price index.

⁸ Data are published by city, and no national averages are given. According to this survey, expenditures in San Francisco on the ancillary medical services included in our data (laboratory tests, x-rays, and operating room) accounted for about 6 per cent of average family medical expenditures, while expenditures for *all* ancillary services accounted for about 14 per cent. The most important additional item included in the latter figure is nursing care which accounted for about 4 per cent of family medical expenditures. So far as we have been able to find out, fees of private nurses, like the prices of ancillary services for which our study has data, have also risen more than 59 to 63 per cent, the increase imputed to them in the BLS index. It seems reasonable, therefore, to conclude that the BLS index underestimated the increase in the average price of ancillary services.

To sum up, we are inclined to think that, accepting what the BLS index sets out to measure, it probably underestimates the increase in the costs of medical care over the fourteen-year period from 1951 to 1965, although we cannot say by how much. In the future, however, it should be a reasonably accurate measure of changes in medical care costs, again accepting its aim and assuming that the health insurance component of the index is accurate. For since the 1963 revision, the major ancillary medical services are priced directly, and the difference between customary and average fees is likely to be of very little importance since it has practically disappeared already.⁹

We can now turn to the problem of changes in treatment and the increase in specialization. As pointed out earlier, both of these factors are not only not taken account of by the BLS index but deliberately excluded on the grounds that they represent changes in quality. What we want to present very briefly in concluding is the case for taking account of them in a medical care price index.¹⁰

Before doing so, a few words should be said about the probable effects of these two factors on medical care costs in general. There is little doubt that increased specialization has raised costs in the past, and the examples we cited from our study are by no means unique. Even physicians would probably agree, justifying the higher fees by the additional training undergone by specialists. The importance of this increase in specialization may be illustrated by the fact that in 1951 the physicians at the Palo Alto Medical Clinic represented 12 fields; by 1964, they represented 21 fields, a change not entirely explained by the growth of the Palo Alto Clinic. For Palo Alto as a whole, the corresponding figures were 18 and 37, respectively. There is every evidence that this trend toward greater specialization will continue, with ever more subspecialization in many fields. Just as today most general surgeons no longer perform orthopedic, gynecological, or thoracic surgery, in the future orthopedic surgeons, for example, will probably split into subgroups, each group specializing on a special part of the body.

⁹ We should like to add, however, that in the future the average fee actually charged may well be higher than what physicians report as the customary fee. Fee schedules are becoming more and more detailed and are distinguishing between services rendered during regular office hours and on weekends or evenings, the fees for the latter being higher. (In our study, we found that this distinction had not yet been made in the early 1950s, though it has been common for some time now.) The customary fee reported would be the regular daytime fee since the majority of visits would be at this fee. As shown in Table 3, as a result of after-hours and weekend visits, the average fee for a house call by a pediatrician was about 3 per cent higher than the customary fee.

¹⁰ Perhaps it should be noted that the writer has completely reversed herself on this point, having argued when originally discussing a medical care price index based on costs-per-episode-of-illness that one of its advantages would be that it would facilitate correcting for quality changes.

The effect on costs of changes in treatment is much less clear-cut and more difficult to evaluate. As mentioned earlier, we found in our study that on balance such changes had raised costs in the case of appendicitis, breast cancer, and forearm fractures requiring a general anesthetic, while in the case of otitis media their net effect was to have slowed down the rise in costs, and in the case of maternity care their net effect was neutral. It would take a much more comprehensive study than ours to estimate the net effect of *all* changes in treatment over the past 14 years. Many physicians have maintained that the medical care price index has overstated the increase in costs by not taking account of changes in treatment, citing the decline in the average length of hospitalization as their main evidence. If forced to make a guess, we would say that the net effect of changes in treatment, at least over the past 14 years, has been to raise costs, and hence that the medical care price index, if it had taken account of such changes, would have shown a greater rise than it did. However, we might get quite different results if we compared costs of treatment in the early or mid-1930s with costs in the mid-1940s. For during this period two dramatic changes in treatment occurred which may well have actually lowered costs: the use of antibiotics and the practice of getting surgical patients on their feet (and out of the hospital) as soon as possible.

To answer the question whether a medical care price index should or should not take account of quality changes, we must ask ourselves what we want such an index to measure or, to put it differently, what use we want to make of it. One of the main uses of the CPI, including the medical care price index, is in labor-management contracts to adjust wages to enable workers to maintain their standard of living. On the whole, it seems reasonable therefore to hold the quantity and quality of items in such an index constant, at least in the short run. When it comes to a medical care price index, however, this is not nearly so reasonable. In the case of most though not all goods, the consumer has a choice between different qualities of the same item. In the case of medical care, his choice is infinitely more limited. He can choose his physician, although today, with the increasing standardization of fees, the price range from which he can choose is narrow; furthermore, even if he decides to consult a general practitioner, he may well get referred to a specialist. He can go to a ward instead of a more expensive room if he has to go to the hospital. He can buy his drugs at a cut-rate drug-store instead of the more convenient corner drugstore. But that just about covers all the choice he has. By and large, we can say that at any given time he can only buy the current quality of medical care. To come back for a moment to the case of the forearm fracture requiring a general anesthetic mentioned earlier, there is no doubt that the pa-

tient is better off if he is given such an anesthetic in the operating room of a hospital equipped for all contingencies than in the physician's office where the risk is much greater. When the question is how much more he has to earn to have his fracture treated than he needed ten or fifteen years ago, however, it does not make much sense to answer this by comparing costs 15 years ago with the estimated costs of the identical treatment today when it is no longer available. The fact that the present treatment is better will increase his welfare; it still leaves him with the problem of how to pay for it.

The above argument can, of course, be extended to other necessities included in the CPI where there has been a forced substitution of a more expensive for a less expensive variety of a product. It does not apply when we consider the other main purpose of the CPI, i.e., its use as a general economic indicator. For this purpose, the present type of index seems preferable. To serve both purposes, therefore, if forced substitution of a higher-quality for a lower-quality commodity is at all common, we might well want two types of CPI, one reflecting price changes only, the other price and quality changes; or alternatively, we might rely on other existing measures of economic activity and let the CPI reflect both price and quality changes. This is a question, however, which goes beyond the scope of the present paper.

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DEBT INSTRUMENTS IN BOTH MACROECONOMIC THEORY AND CAPITAL THEORY

By EARL A. THOMPSON*

Economic theorists have always treated the debt instrument as a *deus ex machina*, never fully integrating either governmental or private debt creation into general equilibrium theory. In particular, the literature contains, at best, only incomplete theoretical statements of both the effects of and rationale for debt financing. This paper attempts to complete these statements for worlds not complicated by transaction or information costs.

To do this, the paper raises and offers answers to three questions. First, as regards the effects of debt financing by the government, what tax-subsidy policy is equivalent to a policy of replacing current taxes with government debt? Our answer will serve to specify the exact impact of debt financing on aggregate demand and will point out some fallacies in various modern attempts to introduce government bonds into macroeconomic theory.

The second and third questions concern the rationale for debt financing. The second question is: Does there exist a class of full, general equilibria for which some governmental debt financing policy would improve the allocation of resources in the standard Paretian sense? Our answer is affirmative. Our class of Pareto nonoptimal general equilibria contains as a special subclass that class of golden-age equilibria in which the rate of interest is below the rate of growth. This special subclass has been called "inefficient" in several recent papers on capital theory (the first of which was by Samuelson [18] and the most complete by Phelps [17]). The authors of these papers uniformly purport to demonstrate that perfect competition can readily generate such inefficient equilibria. Our third and final question directly challenges the "demonstrations" of these authors. Its answer, which states that genuinely perfect competition cannot generate such nonoptimal equilibria, will introduce a rationale for the *private* creation of debt instruments in perfectly competitive markets which has been heretofore ignored in standard economic theory.

The second and third inquiries, aided by the results of the first inquiry, will serve to generalize, respectively: (a) Fisherine necessary

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conditions for Pareto optimality and (b) Fisherine theories of interest rate determination under conditions of perfect competition. These generalizations will amend portions of the results of most of the recent literature on the allocation over time in infinite horizon, general equilibrium settings.

I. *The Effects of Debt Financing by the Government*

A. *The Tax-Subsidy Equivalent*

Our own analysis can be built upon the general analysis of debt financing of Buchanan [3], [4, ch. 28], and Bowen, Davis, and Kopf [2].¹ These authors decompose governmental debt financing, i.e., the replacement of current taxes with new issues of government bonds, into the following two components: (1) a tax component—a replacement of *current* taxes with future taxes of equal present value, and (2) a bond sale component—a current sale by the government of an amount of bonds equal in value to the above reduction in current taxes. The authors utilize their analysis to expose a major fallacy in the textbook doctrine that debt financing cannot shift the real tax burden of a current government expenditure onto future generations. For this first inquiry, we utilize their decomposition, specialized by two assumptions stated below, to derive a simple tax-subsidy equivalent of debt financing. This equivalent exposes what appear to be fallacies in modern attempts to integrate bonds into macroeconomic theory and permits a specification of the exact mechanism whereby debt financing affects current aggregate spending.

Our assumptions are: (1) all taxes, present and future, are lump-sum in nature and perfectly foreseen, and (2) the market in present bonds is perfectly competitive.

Our first assumption serves to restrict the meaning of the tax component of debt financing. The government's replacement of current *lump-sum* taxes with future *lump-sum* taxes would obviously induce no substitutions between real goods so that the only possible changes in the demands for or supplies of real goods resulting from the tax component are due entirely to changes in individual wealths. Since the *aggregate* future tax increase is equal in present value to the *aggregate* current tax reduction, these possible changes in individual wealths can amount to at most a redistribution of wealth among the domestic taxpayers.

While the tax component of debt financing does not induce any sub-

¹ Accordingly, money, a medium of exchange, is excluded from our definition of debt. A debt asset is any financial asset which is valued solely for its interest return. Our assumption of zero transaction and information costs insures that the money rates of interest, or "marginal efficiencies," on all nonmonetary assets are equal.

stitutions between real goods under our assumption of lump-sum taxation, it does induce an increase in the demand for bonds at the initial set of prices (over and above any change in demand which might result from the possible redistribution of wealth). This increase in demand for bonds is exactly equal in value to the tax reduction—given our second assumption that the market for present bonds is perfectly competitive. An explanation of this equality is as follows: Since, before accounting for the possible redistribution of wealth between the various taxpayers, the reduction in each individual's current lump-sum tax payment and equal expansion in his liability for future lump-sum tax payments simply amount to a net loan to the individual, and since the individual had already chosen his optimal borrowing-lending pattern in the perfectly competitive market for present bonds, each individual will put his extra disposable current income from the tax reduction into bonds in order to restore his previously optimal amount of borrowing. This increase in the *demand* for present bonds—taken in conjunction with the equal increase in the *supply* of present bonds which makes up the entire second component—implies that debt financing does not disequilibrate a competitive market for present bonds at the initial set of prices and distribution of wealth. Thus, taking both components together, debt financing has absolutely no effect upon the allocation of resources in the absence of a redistribution of wealth—i.e., in the absence of a difference between the present cost of the extra taxes which any citizen expects to be paying because of the service charges on an increased national debt and the decrease in his *current* tax bill made possible by the debt financing. For with no one's wealth affected at the same prices and rates of interest, excess demands for all goods would be unaltered by the policy. Moreover, when debt financing does redistribute wealth, the economy is affected only because of the redistribution.

Hence, with perfect markets for present bonds and neutral, perfectly foreseen taxes, debt financing is equivalent to either no change or a policy of lump-sum transfer between domestic taxpayers. The gaining citizens, if any, are those taxpayers whose net tax liabilities are reduced due to the government's substitution of future for current taxes; the corresponding, losing citizens are those taxpayers whose net tax liabilities are increased.

B. Application to the Literature on Government Bonds in Macroeconomic Theory

One might think, following an American tradition begun by Hansen [9, pp. 135-222] and carried on into practically every modern textbook discussion of fiscal policy, that an increase in current aggregate

consumption expenditures results from the increase in current aggregate disposable income from the current tax reduction involved in debt financing. But this thought would be basically fallacious, for it completely disregards the equal decrease in aggregate wealth due to the increased future tax liabilities. One might similarly think, following Patinkin [14, ch. 12], [16, ch. 12] or Tobin [20], for example, that debt financing, by increasing the supply of government bonds, would by definition increase the aggregate real wealth of the community. This hypothesis would also completely ignore the liability of future tax payments,² a liability which is exactly equal in value to the new financial asset to each citizen when there are no distribution effects.³ Finally, one might think, following Modigliani [13] or Tobin [20], for example, that the increased supply of government bonds would force a substitution of bonds for real assets and consequently increase the rate of interest on bonds. But this would also be fallacious, for, in the absence of distribution effects, the reduction in current taxes and equal increase in future tax liability increase the value of the demand for bonds at the original interest rate by an amount equal to the increase in the value of the supply of bonds. This is because, at this interest rate, with wealth unchanged, planned streams of expenditures and receipts on the purchases and sales of real assets remain unchanged so that the additional funds required in the future in order to

² As the following inquiries in this paper show, there does exist a special class of imperfectly competitive economies for which a special debt refinancing program will allow some increase in debt to amount to an increase in aggregate wealth. Nevertheless, the types of studies criticized above both assume perfect competition and fail to specify anything like the requisite kind of refinancing operation.

³ Patinkin [14, Ch. 12], referring to the paper of Christ [6], has recently amended his position to allow for some discounting of future tax liabilities. Nevertheless, he maintains that, because of a "distribution effect," at least part of an increase in the supply of government bonds can still represent an increase in aggregate wealth because of an incomplete accounting for future tax liabilities. Following the unfortunate precedent set by Metzler [12], Haberler [8], and Johnson [10], Patinkin specifies neither the kind of redistribution involved nor the method whereby a redistribution gives rise to an incomplete accounting for the future tax liabilities.

It is not difficult, following our own analysis, to produce this missing information. First, since the above authors are discussing open market bond sales rather than debt financing, the current tax decrease in the analysis above must be replaced with an equal increase in the real value of those cash balances which are not used to purchase the bonds. Thus (with real bonds and no money illusion), an individual is a net gainer at the original prices if and only if the increase in the real value of his cash balances exceeds the increase in real value of his future tax liability. For what it is worth, my guess is that sales of *long-term* securities would typically redistribute toward those with the higher marginal propensities to consume out of wealth (viz., those in the older half of the population who will not live long enough to pay a full share of the future taxes), thereby *increasing* the interest rate; but sales of *short-term* securities would redistribute toward those with relatively low marginal propensities to consume out of wealth (viz., the younger adults, who have relatively low taxable incomes), thereby *decreasing* the interest rate.

finance the increased tax liability must be obtained by spending the extra disposable income from the tax reduction on bonds (utilizing the future receipts from these bonds to pay the future tax liability).

C. *Debt Financing and Aggregate Demand*

An immediate implication of the analysis is that, with perfect bond markets and neutral taxes, the popular policy of reducing taxes and expanding the national debt in a recession is completely ineffective in the absence of distribution effects. However, as we now illustrate, such a policy is typically expansionary because it *does* redistribute wealth toward those citizens with high marginal propensities to consume out of wealth: When the older generation succeeds in receiving a current real tax reduction in excess of the present cost of its future real tax payments on the increased debt, the older generation clearly gains at the expense of those who suffer present costs of increased future tax liabilities which exceed their current tax reductions. This kind of redistribution through debt financing is typical in most economies because the old generally share in an income tax reduction but do not live to share in the servicing of new debt. Such a redistribution will increase current aggregate consumption, for an older generation has a marginal propensity to consume out of *wealth* which is close to unity while the younger (and unborn) generations, whose permanent incomes are mere fractions of their wealths, have marginal propensities to consume out of *wealth* which are mere fractions of unity. The resultant decrease in aggregate savings increases interest rates and lowers future wage rates, but the reallocation of resources and price changes occur only because of the wealth redistribution between internal taxpayers.

D. *Introduction to the Next Section*

A government policy that is purely redistributive (via lump sums) is typically regarded by economists to be irrelevant to the Paretian efficiency of a general equilibrium. That is, in general equilibrium, any purely redistributive policy is supposed to make a person better off only by making some other person worse off. Since, as we have seen, debt financing is purely redistributive, it would be conventional to conclude that debt financing is irrelevant to the Paretian efficiency of a general equilibrium—to conclude that if debt financing makes someone better off, it must concurrently make someone else worse off. However, as the following inquiry shows, there is a wide class of general equilibria for which a certain national debt policy will lump-sum redistribute between generations in such a way as to always benefit at least one person without harming any other person.

II. *Debt Financing, Intergenerational Transfers, and the Pareto Nonoptimality of a Class of General Equilibria*

A. *Assumptions and Definitions*

Before specifying the Pareto nonoptimal class of general equilibria and a debt financing policy which leads to optimality, some simplifying assumptions and definitions of relevant concepts are in order.

1. While our results will hold as long as successive generations coexist for some positive amount of time, the discussion below will be restricted to a life-cycle environment in which two and only two successive generations coexist at all times (Cf. Samuelson [18], Diamond [7], and Cass and Yaari [5]). With a constant age of reproduction for all generations, this restriction implies that the length of time that any individual spends in each of the two generations is the same. This length of time is taken to be the length of one "period." New national debt has a debt maturity of one period and all consumptions and fiscal operations are concentrated at the beginning of each period.

2. The rate of interest for a given period is defined as the marginal rate of return over cost, the marginal rate of time preference of the younger generation, and, if an international capital market exists, the international market rate of interest. Hence, all lifetime consumption patterns are assumed to satisfy the usual Fisherian optimality conditions, conditions which will be seen to comprise only *part of* the set of necessary conditions for a Pareto optimal allocation over time.

3. Consumable wealth is defined as that part of wealth (evaluated in terms of current consumer goods) which is marginally transformable at the current rate of interest into current consumer goods. Consumable wealth of the younger generations would be equivalent to the aggregate wealth of the younger generations if, for example, (1) a perfect international market exists for the conversion of claims into current consumer goods, or (2) no resources owned by the younger generations are specific to future production processes. Obviously, consumable wealth can never exceed wealth proper.

We can now proceed to the economic analysis.

B. *The Class of Pareto Nonoptimal General Equilibria*

Consider the class of general equilibria for which the *present* value of the *aggregate* consumable wealth of each younger generation alive an infinite amount of time from now is *infinite*. Because infinity minus a constant is still infinity, a transfer of wealth from the infinitely wealthy generations to the current generation—if such a transfer is technically feasible—would not reduce the present value of the wealth of any generation. As we are about to see, this "transfer" is indeed

technically feasible, at least in a special case, through the use of an appropriate national debt policy.

The special case which we consider here is one in which each period's current interest rate falls short of the percentage increase in the aggregate consumable wealth of next period's younger generation over that of the current period's younger generation by at least some positive constant. This broad class of general equilibria is not further restricted to any particular economic environment; there may exist, for example, any patterns of population growth and technical change, government expenditures and taxation, nonconstant returns to scale, or market imperfections. A simple present value calculation shows that, in this class of general equilibria, the present value of the aggregate consumable wealth of any younger generation alive an infinite time from now is infinite.

What we wish to show is the following: When the percentage increase in the aggregate consumable wealth of the next period's younger generation over that of the current younger generation forever exceeds the real rate of interest in each future period by at least some positive constant, then there exists a debt financing policy which satisfies a Pareto condition—i.e., which makes someone better off without making anyone else worse off.

C. *A Note on a Generalized Theorem*

Before proceeding to our nonmathematical demonstration of this, it should be noted that it is mathematically demonstrable [19]⁴ that, under plausible regularity conditions,⁵ *any* time there exists an infinite present value of the aggregate wealth of all generations born an infinite time from the present, there exists a debt financing policy (e.g., the one described below) which satisfies a Pareto condition. The condition that the interest rate always falls short of the percentage increase in the wealths of the successive younger generations by at least some positive constant is only a special case of the general infinite wealth condition. Nevertheless, this special case—a special case of which has been mathematically established as Pareto nonoptimal by Koopmans and Phelps [17]—will be seen below to be amenable to nonmathematical

⁴ This is available upon request from the Institute of Government and Public Affairs, UCLA, Los Angeles, California, 90024.

⁵ These conditions are: (1) that the intertemporal utility and transformation functions for each generation are continuously differentiable, (2) that there exists a positive constant such that for all generations, the elasticity of substitution of at least one of these functions exceeds this constant, and (3) that all growth rates and interest rates in the pre-policy allocation over time are bounded by some positive constant from above and some constant greater than -1 from below. The latter two conditions also apply to the analysis of the present paper.

examination. My paper [19] shows that if there is also strict convexity of the standard Fisherine intertemporal utility and production-possibilities sets, then the above infinite wealth condition is *necessary* as well as sufficient for the existence of an intergenerational redistribution program satisfying a Pareto condition. (It is also seen that if it is always the case that one of the two Fisherine intertemporal functions is not strictly convex but, instead, representable by a straight line, then the necessary and sufficient infinite wealth condition stated above is replaced by the weaker condition that the present value of the wealth of all younger generations exceeds some positive constant.)

D. The Curative Debt Financing Policy

Our debt financing policy is first to raise the level of national debt, giving tax relief (or direct subsidies) only to the older generation, and then to increase the debt and old-age benefit level in each succeeding period at a percentage rate equal to the real rate of interest in that period. The consequent increase in consumption of the current older generation does not reduce the present value of the consumption of the current younger generation, who will be compensated for their initial loss of consumable assets with both principal and interest by tax relief (or direct subsidies) in their later years financed by the succeeding, as yet unborn, younger generation, who will in turn be compensated like their forefathers through a tax reduction in their old age achieved by the flotation of a still larger amount of national debt. With the ratio of the national debt to the consumable wealth of taxpayers ever decreasing because of the relatively rapid rate of growth of consumable wealth, the process can continue on forever. The current older generation gains and no other generation will lose.

With reduced amounts of consumable assets available to them while they are young, every younger generation will either (1) consume less while they are young, thereby raising their marginal rate of time preference, (2) carry over a smaller stock of consumable assets into their old age, thereby raising the marginal rate of return on cost, (3) import consumable assets, thereby bidding up the world market rate of interest, or (4) choose some weighted combination of the above three actions. Whichever the case, each period's rate of interest rises as a result of our policy. Clearly, the larger the increase in the initial level of the debt, the larger the rise in each period's interest rate.

Once the initial level of the national debt has jumped to a level which forces the resulting rates of interest forever above the corresponding rates of increase of the consumable wealths of successive younger generations, our conventional theorems of the Pareto optimality of a competitive equilibrium become applicable; the present value

of the wealth of generations alive an infinite time into the future is zero. At these rates, we can no longer specify a policy which makes one generation better off without making another generation worse off; any policy such as that suggested above is physically impossible. For a further rise in the initial level of national debt would have to be followed by percentage increases in this debt which are equal to the real rates of interest in order to hold all of the succeeding generations on their original indifference levels; but since this implies that the national debt forever grows relative to the consumable wealth of the young, sooner or later the time would come when the consumable wealth of the young would be less than the national debt and therefore insufficient to cover this generation's real tax bill. In other words, there would be some future older generation that could not hope to increase the national debt and have its real taxes reduced (or subsidies increased) because it would be physically impossible to collect the appropriate taxes from the younger generation. It would therefore be physically impossible to compensate *this* older generation for the consumable assets which it had to surrender in tax payments in order to keep the immediately preceding generation from being made worse off.

Thus, when rates of interest forever exceed corresponding percentage increases in the aggregate consumable wealths of successive younger generations, changes in the national debt serve only to redistribute the finite current wealth between the various taxpayers. But when the opposite relation holds—when the percentage increase in the aggregate consumable wealths of successive younger generations forever exceeds the corresponding rate of interest by at least some positive constant—appropriate increases in the national debt can, in the standard Paretian sense, improve upon the allocation of resources. This is done by redistributing wealth away from generations which will exist an infinite time from now.

E. Application to the Literature on Steady-State Equilibria

Applying our general result to the modern growth model discussion, a steady-state equilibrium, or "golden-age," with an interest rate below the steady-state growth rate of the economy is not only "inefficient" in the "golden-rule" sense, a very special sense which requires a social welfare function that completely ignores the utilities of all nongolden-age generations. It is "inefficient" in the standard Paretian sense, a sense which can be based upon any Bergson Social Welfare Function, a function which counts the utilities of all generations.

Several other authors have also noted that a steady-state with a rate of interest below the rate of growth of aggregate income can be improved in something resembling a Paretian sense (e.g., [1] [5] [18]).

Applied to the steady-state case, our model above is unique in that it shows that the economic problem is necessarily one of the intergenerational distribution of wealth. At the same time, our model both (1) specifies a familiar economic policy which will simultaneously satisfy a Pareto condition and remove the inefficiency by raising the interest rate⁶ and (2) shows why standard analyses of efficiency and equilibrium apply once the interest rate exceeds the growth rate. Most important, an understanding of the distributional nature of the problem is essential in the following inquiry.

III. *The Pareto Optimality of a Genuinely Perfectly Competitive Equilibrium*⁷

A. *The Implications of a Perfect Market in Bond Futures*

The above was not restricted to any particular institutional setting. Nevertheless, in an institutional framework of genuinely perfect competition, it is impossible for the rate of interest to forever fall short of the corresponding percentage increase in the consumable wealths of successive younger generations by at least some positive constant.

The reason is that if interest rates *were* forever this low, *private* firms would proceed to bid up interest rates by issuing new debts to finance current new expenditures in the same way that our optimizing government above issued new debts to finance its current subsidy to the aged. That is, under these circumstances the corporate firm, an institution with an infinite legal life, would rationally adopt the following policy: (1) presently buy real assets by issuing new corporate debt; then, (2) in the next period finance *both* principal and interest on this debt by issuing a correspondingly larger amount of debt; and (3) repeatedly expand the corporate debt in this way, i.e., by selling bonds in return for the consumable assets necessary to service and retire the previous issue. The current stockholders in the corporation could thereby freely acquire current real assets. Since the percentage increase in the consumable wealth of the successive, debt-purchasing younger generations would never fall below the interest rate, each group of younger generations would readily absorb the firm's debt flotation. Hence, with no injured parties, there would be no legal objection to the firm's explicit financial policy. This opportunity to acquire

⁶ Our national debt policy, in contrast to those examined by Diamond [7], satisfies a Pareto condition and therefore *demonstrates* the Pareto nonoptimality of the class of general equilibria described above, a class which contains that specified by Diamond. Diamond does not demonstrate the Pareto nonoptimality of his "inefficient" equilibria, and his suggested policies merely redistribute wealth between *finitely* wealthy generations so as to raise the interest rate. This kind of policy is analogous to redistributing all wealth away from the consumers of a monopolistically sold product in order to remove a familiar monopoly-misallocation.

⁷ Jack Hirshleifer contributed helpful comments on an earlier draft of this inquiry.

current assets at zero cost leads to increases in the amount of *private* borrowing until interest rates are brought to levels sufficiently high that any such financial policy simultaneously fails to satisfy a Pareto condition and would lead to bankruptcy, which is not allowed in a perfectly competitive system.⁸

An orderly method of allotting property rights to the limited number of feasible interest-and-principal refinancing programs, or contracts promising the delivery of future bonds, could be one organized on a first-apply, first-receive basis. For example, an application for a property right could be considered complete once the refinancing plan is made known in bond indentures or the corporate charter.⁹

B. Applications to the Literature on Capital Theory

A straightforward, powerful application of this property of perfectly competitive institutions is that there never can be a negative rate of interest in a perfectly competitive stationary state. For example, this property completely alters the results of the stationary model of Samuelson [18], a purportedly perfectly competitive model which generates social inefficiencies because of negative rates of interest. More generally, rates of interest below golden-age growth rates, i.e., Samuelson-Phelps-type inefficiencies, are impossible in any *genuinely* perfectly competitive equilibrium. Again, when such inefficiencies are present, there are positive profits to private borrowing, with repayment accomplished through a plan of continual refinancing of both principal and interest. Such inefficiencies are simply inconsistent with the existence of perfectly competitive markets in future bonds.

Most other modern growth models, while perhaps of less direct policy relevance than those discussed above, also admit steady-state solutions in which the "competitive" interest rate is below the growth rate.

⁸ In the case that the bankruptcy implies that creditors of the corporation at the time of bankruptcy are not paid off, *no generation* would buy the bonds of the company. This is because creditors in the period immediately preceding any period of bankruptcy depend for *their* repayment on bond sales to creditors who will not buy at any positive price because the company is going bankrupt. In this way, the zero value of the bonds of the company is transmitted from the future to the present. I owe this point to my colleague, Joseph Burns.

⁹ Armen Alchian has suggested an alternative to a market in bond futures. He asserts that the rational family will accomplish the same intergenerational transfer as the futures market. This does appear to be true, at least for a case in which some "family" units always contain members from both generations and the ratios of the consumable wealths of the younger generations of some such family to the corresponding aggregates of consumable wealth of the younger generations are always above some positive constant. Alchian offers as an example the custom, prevalent in many Oriental families, in which the elderly members of the family are supported largely by younger members. Examples of the mechanics of such intra-family, intergenerational transfer systems can be found in Meade [11].

These models should be formally altered by the inclusion of a competitive market in future bonds. This would amputate an entire class of steady-state equilibria from these growth models.

C. *An Empirical Example*

An example of a market in bond futures is found in the establishment and operation of a modern-day "pension fund." In the United States, several insurance companies, industrial corporations, and labor unions set up these funds through "retirement plans" for their workers (or customers in the case of insurance companies, customers purchasing term life insurance). Members of these plans are, in effect, buying bond futures from their companies. A firm offering a retirement plan is typically a "growth company" in that its wage bill (or the total income of its customers in the case of an insurance company) is expected to grow steadily at rates at least approximating market rates of interest. (Such growth policies would not be sustainable if the growth rates of the aggregate consumable wealth of the younger generations were always below the interest rates, for then any such company would sooner or later reach the size of the entire economy and its previous growth policy would be subsequently impossible.) A contributor to such a retirement plan can expect payment of both principal and interest, the source of such payments being a pension fund formed out of earmarked future income, the income generated by the contributions of succeeding generations of contributors. The companies, of course, use the initial contributions for the enrichment of the stockholders, whose aggregate consumption drives interest rates up to the point that any additional pension fund would eventually go bankrupt. In most cases, a license to establish and operate a pension fund is required by a state, an institution responsible for bankrupts.

D. *Policy Implications of an Imperfect Market in Bond Futures*

Introducing market imperfections characteristic of the real world, it may well be that the private transaction costs in effecting transfers from the currently infinitely wealthy generations sometimes exceed the administrative costs of having an efficiently operating government do the same job. In such cases, the efficient government would have the role given to it in the second inquiry. That is, if it were estimated in an actual economy that the present value of the transferable aggregate wealth of the generations born an infinite time from now were infinite, *and* if it were estimated that the transfer benefits in effecting an appropriate policy exceeded the administrative costs of the government policy, then the efficient government would effect a transfer policy such as the debt financing policy specified in the preceding inquiry.

IV. *Conclusions*

Debt financing by the government in any economy with lump-sum taxation and a perfectly competitive market for present bonds is equivalent to a policy of either no change or lump-sum wealth redistribution between the country's internal taxpayers. The consistent application of this equivalency in our macroeconomic models would yield theoretically correct analyses of the expansionary impact of debt financing and open-market operations and would prevent our falling into various, peculiar intellectual traps. In particular, we could not, following Patinkin [14, Ch. 12], [16, Ch. 12], and Tobin [20], for example, believe that debt financing creates only an asset, government debt, which is an additive component of the total wealth of a perfectly competitive economy. We could not, following Modigliani [13] and Tobin [20], for example, believe that governmental debt financing creates a "substitute for real assets," thereby forcing up the rate of interest on these latter assets. Finally, we could not, following Hansen [9, pp. 135-222] and most American textbooks, believe that governmental debt financing increases consumption expenditures simply because the policy increases *current* disposable income.

Despite the fact that any debt financing policy is at most just redistributive, there exists a debt financing policy which makes someone better off without making anyone else worse off in a broad class of general equilibria. Such a policy is to first raise the level of national debt, giving tax relief (or direct subsidies) only to the older generation, and then increase the level of national debt and tax-relief-to-the-aged in every succeeding period at a percentage rate equal to the real rate of interest in that period. The class of general equilibria for which this type of debt financing policy will benefit someone without harming anyone else is the class for which there exists an infinite *present* value of the aggregate consumable wealth of the generations born an infinite time from the present.¹⁰ A general subclass of this class of Pareto non-optimal general equilibria is that characterized by future rates of interest which forever fall short of the corresponding percentage increases in the aggregate consumable wealths of the successive younger generations by at least some positive constant. A special subclass of this subclass of Pareto nonoptimal general equilibria is the class of economies demonstrated to be "inefficient" by Koopmans and Phelps [17]. Another special subclass is the class of "inefficient" golden-age

¹⁰ This general proposition, which holds under the strict convexity of intertemporal production and preference sets, is demonstrated in [19]. Under weak and not strict convexity of at least one set for future periods, the *infinite* wealth condition above is seen to be replaced by an analogous, *positive* wealth condition.

growth paths of Samuelson [18], Diamond [7], Cass and Yaari [5], and Aaron [1], paths characterized by golden-age interest rates which are below golden-age growth rates. All of these "inefficient" economies are Pareto nonoptimal because of faulty distributions of wealth between presently living generations and generations born an infinite time from the present.

Our general class of Pareto nonoptimal general equilibria cannot be perfectly competitive because, in such equilibria, positive marginal profit exists to any corporation that buys additional real assets by financing its purchase through a private debt issue followed by continual interest-and-principal refinancing through future debt issues. Hence, for example, the allegedly perfectly competitive models noted above (i.e., [1] [5] [7] [17] [18]), models which generate explicitly inefficient golden ages, are in fact inconsistent with the existence of a perfectly competitive market for private debt instruments. Most other modern growth models are similarly defective in that they also are allegedly perfectly competitive but generate the class of non-perfectly-competitive, steady-state solutions in which the rate of growth of the economy exceeds the rate of interest. All of these models suffer from the omission of a market in bond futures.

The general methodological implications of our second and third inquiries for the study of perfectly competitive allocations over time can be summarized as follows: We should (1) add to the standard Fisherian necessary conditions for Pareto optimality the requirement that under strict convexity there is a finite present value of the aggregate wealth of some generation born an infinite time from the present, and (2) add to the standard set of perfectly competitive institutions a perfectly competitive market for future debt instruments.

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A PRIVATE OWNERSHIP BIAS IN TRANSIT CHOICE

By ROGER SHERMAN*

The continuing defection of public transit riders in big cities suggests that eventual dominance by private automobiles may be inevitable [7, pp. 267-69]. A Chicago survey has indicated that only a very large subsidy would persuade a significant number of auto travelers to switch to public modes [8]. There remains a question, however, whether the private auto is gaining ascendancy by genuine consumer choice or because private costs differ sufficiently from social costs to produce a misallocation of resources in favor of autos. A. A. Walters attempted the difficult analysis of auto travel's social cost using data from several studies and concluded: "In order to make private costs approach social cost we suggest that a *minimum* fuel tax of 33 cents a gallon must be raised" [9, p. 67].¹ C. O. Meiburg raised another allocation issue by pointing out tendencies toward overinvestment in highways through our political decision process, which builds highways to relieve congestion but also distributes them free so that congestion is inevitable [5]. Such studies suggest that present institutional arrangements may not enable consumers to choose the allocation of transit resources they prefer.

On the reasonable assumption that costs decrease for each transit mode, we demonstrate in Section I a bias in consumer choice that favors private autos over public transit. The bias arises as a result of an auto traveler's commitment to auto ownership, which forces a choice between auto and other modes and also makes the average price he pays per mile vary with his usage while for public modes it does not. Also, constant prices for decreasing cost public transit modes can prevent optimal allocation among them, thereby making them a less effective substitute for auto travel. Section II describes one way of financing public transit that will avoid this particular allocation bias.

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¹ Walters argues further that railroad fares are greater than the marginal cost of the service they purchase (except for peak periods on commuter services). This argument, combined with evidence that private auto costs are less than their marginal social cost, makes the allocation bias complete. M. B. Johnson calls for more precise pricing, after reaching similar conclusions regarding private and social costs [3]. William Vickrey has long advocated more precise pricing and has proposed several practical means for achieving it [10].

I. *Private Choice: Automobile, or Public Transit?*

Broadly, there are two alternative travel means, the private automobile and public means, including bus, railroad, taxi, airplane, or rental vehicles. We should expect a consumer to choose either automobile or public transit, not both, because auto ownership carries with it a substantial commitment to the automobile as a means of travel. If used-car markets were perfect this might not be so, for a consumer could then buy a car for one trip and sell it afterward. But transaction costs in automobile markets are high. Not only does the purchase of an automobile require time and effort, its sudden sale at an inopportune time will result in financial sacrifice. A tendency of persons to travel either by auto or public transit is evident in data from a Chicago study of commuters [6, pp. 138-41]. Of those who owned cars (85 per cent owned cars), four out of five used the car in traveling to work, three out of four traveling all the way into the city by car. Of those who did not own cars, 85 per cent traveled to work via public transit.

Over a short period, the marginal cost of auto travel will be very low for an auto owner while for a nonowner, transaction costs make short-period marginal cost very high. Thus, the short-run choice between public and private transit will depend on whether the chooser already owns an automobile. Analysis of choice must extend over a time period long enough to permit an effective choice, enough time for a consumer to purchase or sell an automobile and satisfactorily amortize his transaction costs. The average period of automobile ownership in the United States is a little more than three years [11], and we shall assume that the auto-traveling consumer bases his choice on an estimate of his auto travel costs over that period. To avail himself of such long-run costs, however, he must commit himself to auto travel for the period. Failing such commitment, he will have to confine his travel primarily to the public mode.

Some costs of auto travel vary with time (e.g., insurance, licensing, garaging, some depreciation) and others vary with miles traveled, so traveling more miles over a given time period will lead to a lower average cost per mile. Notice that the cost per auto mile, or consumer price per mile, will therefore be different for different consumers, depending on how many miles they travel. The effect of miles traveled on price per mile will, in turn, affect the consumer's decision regarding the number of miles he travels. By thus affecting planned travel miles, marginal cost exerts an influence on the eventual average price per auto mile, making the average cost lower for consumers who travel more. Average cost per mile also decreases with more miles traveled via public transit. In the public transit case, however, there can exist

one average cost per mile, when fixed costs are allocated over the total number of passenger miles traveled. As a consequence of such cost allocation a single, constant price per mile can be charged users of public transit and related to the cost per passenger mile.² Since we wish to examine allocation in the absence of subsidies, and because we consider a combination of public modes some of which are not subsidized, we shall assume that cost and price per mile are equal. In the public transit case, then, one consumer's price is not affected by his usage. To see the difficulties this arrangement causes for resource allocation among transportation modes, consider the consumer's utility-maximizing problem.

Well-known conditions for the consumer to maximize his utility require that his marginal utility from each good divided by its marginal price be equal for all goods and services consumed, this ratio of marginal utility to marginal price being the consumer's marginal utility of income. In planning his possible auto travel over a three-year period, the consumer must first reduce income available for all goods by making fixed payments associated with auto ownership. Consumption of all goods (ignoring inferior goods) is reduced as a result. At the same time, a lower marginal price is used to evaluate auto miles traveled, and this invites the consumer to travel more than he would if he paid a constant average price per mile. In planning possible public transit travel, in contrast, the consumer bases his usage on a constant average price which equals average cost but is greater than marginal cost. The private auto arrangement has an advantage for utility maximizing that can be illustrated easily. Regard as private transit an automobile, privately purchased. Consider as a public transit alternative an automobile that can be rented, the rental rate per mile for one person being regulated at an average price equal to average cost. If a person is given a private or public choice in these terms, he will always prefer the private alternative, for he can reach the same consumption goods mix that is available to him under the public arrangement and because of a lower effective marginal price can reach a preferred position as well (assuming only that the marginal rate of substitution between travel and other goods diminishes).

When this example is extended to a genuine public transit service, there will be only one average price for all public transit consumers rather than a different one for each consumer. That single average price is affected not by one's own usage of the service, but by the usage of all other consumers combined. Those whose marginal utility from

² The charge may only approximate a price equal to cost per passenger mile because of administrative problems in fare collection, but we regard the pricing of public transit as on a fee-per-mile basis.

travel diminishes rapidly are then likely to prefer public transit because the price per public transit mile is lower than they can achieve privately. Those who travel much, however, will face a lower cost per mile via auto travel. It is this effect that is troublesome in decreasing cost industries, where long-run average cost can be lowered by higher rates of usage. The commitment that will enable an individual to achieve the benefits of his own higher usage is a commitment to automobile ownership, which will simultaneously reduce the usage of public transit services. It does so because the auto owner enjoys a low marginal price per mile by auto once he commits himself to ownership, and is therefore less likely to seek public transportation at a price equal to its average cost. Thus, if benefits of higher usage are available only through the private auto transportation mode, that mode will be made more attractive, while passenger miles lost to it from public modes will raise the average cost of service via those modes.

The allocation problem is further aggravated by the presence of many separate public transit modes. From the utility-maximizing model we know that in choosing among public transit modes, a consumer's marginal utility from each mode divided by its marginal price must be equal for every mode that is used. If marginal costs of public transit modes are not proportional to average costs,³ consumers who face prices equal to average cost are sure to reach an equilibrium that is nonoptimal. Exchanges among the consumers, at marginal costs, could improve their welfare. Thus, the coordination of different public modes through single, fixed prices is imperfect. And failure to achieve an optimal combination of the different modes reduces the effectiveness of public transit as a substitute for auto travel.

II. *Avoiding Ownership Bias*

One way to achieve an efficient combination of public transit services, and at the same time offset the advantage of marginal cost influence in private auto choice, is to subsidize all public transit modes so that they may price at marginal cost. Unfortunately, this will not solve the problem completely, for it makes inefficiencies necessary elsewhere in order to finance the subsidy. More important, it implicitly raises the question: should an automobile be given to every family as well, so that only short-run marginal costs will influence the choice between public and private transit? The costly duplication that would result from such a scheme reveals the importance, shown clearly by R. H. Coase [2], of affording long-run as well as short-run choice; consumers must take into account all costs when expressing their prefer-

³ We are not suggesting that proportionality to marginal cost is a satisfactory price criterion. It is surely not satisfied by public transit modes.

ence, not just short-run marginal costs. But to cover all costs, the fees would have to be in two parts: one for short-run marginal costs, and another for fixed costs which depend on time rather than usage.⁴

Since coordination of public transit modes is needed to make public transit effective as an alternative to the private automobile, a collective institution such as a public transit club would offer genuine advantages. Membership fees could correspond to fixed costs of the modes chosen by the member. Non members could still have access to the public modes but at higher prices per mile than members, who would pay only marginal costs. The club could of course serve as a credit institution as well, for it would be a billing and collection agency. Fixed fees could be distributed among the modes by the club, dividing the fee among different firms within each mode in proportion to the direct services provided by the firms. To be sure, allocating investment risks and fixed costs of the modes among members in the form of fixed fees is not easy. The degree of commitment to an automobile is evident to any owner; he makes an investment in an auto and he assumes risks of ownership. Unlike the automobile case, public transit investment does not occur in separate parts that are identified with each user. Nevertheless, the fixed costs of a public transit mode, e.g., vehicles, roadbed, and structures, will depend on the number of passengers it must be capable of carrying, not just on passenger miles. Indeed, the number of rush-hour passengers has repeatedly been revealed to be a crucial determinant of costs for different public transit modes.⁵ And if members can influence their average price per public transit mile by making a fixed payment and then paying a marginal price equal to marginal cost, the bias in favor of automobiles will be eliminated.⁶

It would be possible to accommodate many variations in a membership institution. When several agencies offer service by the same mode, differences in variable fees are possible and would enforce competition

⁴According to W. A. Lewis [4] a "two-part tariff" was first proposed by Dr. John Hopkinson in 1892 in a recommendation for fixed and variable prices of electric power in England.

⁵See, e.g., [6] and [10].

⁶This proposal does not guarantee ideal resource allocation beyond the transportation sector. In particular, it may favor transportation generally, for it lets consumers evaluate their own effect on the price they pay per transportation unit, while prices for non-transportation goods and services are held constant (See [1]). If these other goods and services are produced by perfectly competitive industries, however, their marginal costs should equal their prices and so no misallocation need arise. Alternative goods supplied by public utilities such as telephone and electricity are often priced in a manner that reflects usage, and this proposal only offsets their former advantage.

It should be noted that two-part price arrangements, often called incentive prices, have been applied to transit in a number of places with varying degrees of success. Some communities forbid their use. While such prices would tend to reduce the bias examined here, they have not been broad enough in scope to significantly offset it.

within those modes. Classes of service could also be distinguished, in fixed or variable fees or in both. Where different services share fixed costs jointly it might be desirable to grant access to both services, at appropriate prices per mile, for only one fixed fee. Peak load pricing can be accommodated, too. For example, limited memberships which deny rush hour access might be offered at lower fees.⁷ Our purpose is only to register feasibility, and so we shall not elaborate further on these possibilities. Fortunately, large, established public transportation institutions already exist. Thus, the major problems would be those that accompany transition to an institutional form which would unify pricing, not problems associated with the initiation of new transit agencies or modes.

Organizing such an institution is not a simple matter, to be sure. Especially difficult is the question of its geographic scope. It seems most appropriate for individual cities, yet one of its advantages is coordination of all transit modes into an effective consumer service including intercity railroads and airlines, which would suggest national scope. The most practical form might be a federation of metropolitan Riders' Clubs, each serving major urban centers in the country. Whether the Riders' Clubs be public or private enterprises is an interesting issue we cannot open here. Our objective is only to sketch an alternative institution which might enhance the effectiveness of consumer choice and of the competitive process among those who provide transportation services.

III. *Summary*

A resource allocation bias can favor private autos when an ownership commitment encourages an either/or choice between private and public modes, assuming that all transit is characterized by decreasing costs, and that public transit must cover its costs and uses single, constant prices to do so. Possible misallocation among the public transit modes was also demonstrated. To compensate for these allocative biases, we proposed a collective institution to implement fixed and variable fees. The proposed institution would enable each consumer to plan his transit usage, taking account of approximate marginal social costs, and on that basis make a forward commitment either to private or public transit means. In this way investment in transit resources, as well as short-run usage, can be coordinated. The rise in alternative transit modes makes this approach more appropriate than the traditional regulation of separate monopolies. The argument is relevant primarily

⁷ A family could possess one membership with rush hour privileges and limited membership for all other family members. Occasional rush hour travel could still be made, of course, but at higher nonmember prices.

for urban areas, where substitution possibilities are greatest, where necessary organizing is most feasible, and where transit resource allocation problems are most urgent.

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COMMUNICATIONS

Price Elasticity of Demand as an Element of Market Structure

Is price elasticity of demand an important element of market structure. Jesse Markham says it is and cites the conduct of the producers of rayon and copper in support of his contention [6]. Richard Caves agrees that elasticity is an element of structure but apparently not a very important one. He cites no empirical evidence for his views but gives a simple theoretical argument which will be expanded upon momentarily. He also cites with approval a paper by Franco Modigliani which contains a convincing theoretical argument that elasticity of demand helps determine barriers to entry [3, pp. 30-31] [7]. In his book *Industrial Organization*, Joe Bain does not even list demand elasticity in a miscellaneous category of dimensions which "might offer added explanations" of market conduct but which are not of major significance [1, pp. 264-65.]. Robert Clodius and Willard Mueller go further, suggesting by implication that elasticity is definitely not to be considered an element of structure in industrial organization research [4].

Perhaps enough has been said to show that our initial question involves unsettled issues. In this brief note, we argue on theoretical grounds that elasticity of demand may well be an extremely important dimension of market structure where by structure we mean the set of environmental elements that influence market conduct and performance.¹

I

Consider two industries with the same market structure except that the first has a highly elastic demand curve and the second, a highly inelastic demand curve. Caves argues, and rightly so it would seem, that in the first case a firm would be more likely to resort to price cutting in order to increase its market share because total industry sales would increase even if other firms decided to follow suit. The same cannot be said in the case of inelastic demand where the firms in total might end up selling about the same output at a lower price. It might be noted in addition that the greater the elasticity of demand, the greater the probability that firms will not recognize their interdependency and the greater the probability that any chiseling on some form of collusive agreement will go undetected.

In many ways the argument is akin to that used to show the relevancy of concentration. When concentration is high, a change in the market policy of any one firm is said to have a pronounced impact on the performance of the market and on the profit accounts of all other firms. At issue is oligo-

¹ The elasticity of supply confronting a procurement industry has been considered a structural element by Helmberger and Hoos using arguments similar to those adduced here [5].

polistic interdependency—when is it recognized and under what conditions does it lead to collusive market conduct? Intuitively, the impact of a change in one firm's output of market price reflects not only that firm's market share but also the elasticity of market demand. Intuition does not get one very far, however, in ascertaining the sensitivity of market price to changes in a single firm's output depending on that firm's market share and the elasticity of market demand. At this juncture a bit of mathematics renders significant results.

II

Consider an industry, comprised of n firms, producing a homogeneous product. Let Q equal total industry output and X_i the output of the i^{th} firm. Of interest is the impact on market price of a change in the output of the i^{th} firm when the output of all other firms in the industry remain constant.² Assuming that the change in the i^{th} firm's output is given by cX_i , where c is some number, the change in industry output may be expressed as the total differential of Q :

$$(1) \quad dQ = cX_i$$

Using the formula for price elasticity of demand, (2), and substituting (1) into (2), a rearranging of terms yields (3):

$$(2) \quad -E = \frac{dQ}{dP} \cdot \frac{P}{Q}$$

$$(3) \quad \frac{dP}{P} = - \frac{cS_i}{E}$$

where S_i is the market share of the i^{th} firm, X_i/Q . To facilitate exposition, let $dP/P = K$. Then, treating c as a variable,

$$(4) \quad \frac{\partial K}{\partial c} = - \frac{S_i}{E}$$

Thus, given market share and elasticity, the impact of output change on price is constant for a unit change in output. The relationship depends on market share and elasticity but not on the level of output change. Consider briefly two limiting cases. If $S_i = 1$ then a monopoly exists and the effect of changes in output on price is inversely related to elasticity. On the other hand, as the market share of the firm decreases, i.e., as S_i approaches 0 in the limit, the other limiting case of perfect competition is approached. As expected in this case, equation (4) indicates that changes in the firm's out-

² Alternatively, we could assume that all other firms hold prices but not quantities constant. This assumption plays a crucial role in the models of monopolistic competition and kinky demands where elements of product differentiation are present. The very notion of the responsiveness of market output to changes in market price becomes ambiguous where product differentiation is present. For this reason we assume homogeneous outputs; the results seem less applicable the greater the degree of differentiation.

put will have no impact on market price. In this situation, elasticity is obviously irrelevant to an explanation of market conduct.

A similar relation holds for changes in the firm's share of the market, as shown by

$$(5) \quad \frac{\partial K}{\partial S_i} = - \frac{c}{E}$$

which indicates that given c and E , the impact on price of changes in market share is constant. The price effect is independent of the share of the market held by the firm but is inversely related to the market elasticity.

Finally, the impact of change in elasticity on price is given by

$$(6) \quad \frac{\partial K}{\partial E} = \frac{cS_i}{E^2}$$

Equation (6) is very significant. It shows that the price effect is very sensitive to changes in E .

The same conclusion follows from evaluation of (7) and (8) below:

$$(7) \quad \frac{\partial^2 K}{\partial c \partial E} = \frac{S_i}{E^2}$$

$$(8) \quad \frac{\partial^2 K}{\partial S_i \partial E} = \frac{c}{E^2}$$

These equations show that the impacts of changes in output and market share decrease at an increasing rate with increases in the magnitude of the elasticity. A numerical example will help. Let $c = .1$. Then, a firm with a 20 per cent share of the market where $E = -.3$ has about the same effect on market price as would a firm with a 60 per cent share of a market where $E = -1.2$!

III

A number of important implications for research may be drawn from the above analysis.

1. There is little reason to suppose that elasticity of demand is even roughly the same from one industry to the next. Indeed, the evidence suggests considerable variation [8] [2]. Where cross-section data are used in searching for associations between market performance (profit rate, e.g.) and market structure, exclusion of elasticity of demand as a structural dimension may lead to biased results. Though inclusion of an elasticity variable may be very difficult, if not impractical in many cases, we ought at least be aware of the possible consequences associated with its exclusion.

2. At first blush, one might suppose that, in cross-section analysis, concentration and elasticity of demand might well be collapsed into a single variable using equation (3) above. While experimentation along this line might be worthwhile, there is a compelling reason why the number of

(large) firms is important per se: it is likely to be easier to secure an agreement among three people than among six. The same holds for enforcement of an agreement reached earlier. Thus, at the extremes of market structure, either collusion or independent conduct is all but assured by the number and size distribution of firms. The same cannot be said with much confidence for the intermediate cases, and this is precisely where the search for associations between market structure and performance has proved so fruitless.

3. If demand elasticity is an important dimension of structure, one might first look for performance consequences in industries characterized by extreme elasticity or inelasticity. Case studies of such industries might provide interesting evidence on the empirical validity of the hypothesis at hand.

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A Note on Economic Rent

Although the concept of economic rent has been widely used by professional economists for many years, as yet no clear consensus concerning its meaning exists. At present two very different versions of the rent idea are usually encountered. The origin of one can be traced through Marshall and Mill back to Ricardo while the other had its beginnings in the works of Pareto. Unfortunately many writers do not recognize the dissimilarity between these concepts and use them interchangeably, while others see the difference but fail to derive

any worthwhile advantages from the use of two rent definitions rather than one.¹

The crux of the definition issue is this. Economic rent is defined by the followers of Ricardo as the excess amount earned by a factor over the sum necessary to induce it to do its work. The Paretian rent concept is the excess earnings over the amount necessary to keep the factor in its present occupation [6, par. 745-55]. Clearly these are very different definitions. In the first the test is whether the factor is supplied at all or not; the second is concerned with where it is supplied. In the latter case it is obvious that opportunity costs created by the competitive uses must be met as a condition of holding the factor in its present use.² Only the surplus over these opportunity costs would be rent in this sense.

The first of these definitions had its origin in Ricardo's concept of differential surplus. In essence, it is the same as Taussig's "producer's surplus" [8, p. 63] and Marshall's "scarcity rents" [4, pp. 422-30]. It is oriented to the motives and behavior of the supplier of resources and presumably may serve as the basis of moral judgments approving or condemning their rewards.

Pareto's definition is user oriented. Because of the use of the present occupation test, it differentiates payments generated by the industry or firm employing productive resources from those imposed in the market by the competition of other firms or industries. As a result multiple rent concepts appear. First is rent from the point of view of the economy, which coincides with the Ricardian type of definition. Next, however, is rent from the point of view of the industry, which is any payment made by the industry to a factor in excess of the opportunity costs created by other industries. These extra payments are made necessary by the industries' contribution to the total demand for the factor. Finally, rents from the standpoint of the firm are payments by the firm in excess of what other firms in the same industry would offer the agent for its services.

The existence of two significantly different but widely used approaches to the concept of economic rent is obviously unfortunate. It is appropriate, therefore, to examine critically the relative merits of the two types of definitions in order to determine whether significant simplification is possible and desirable.

The concept of economic rent is encountered most frequently in two important areas of economics, the theory of distribution and the theory of cost. Consequently the merits of the two types of rent concepts can appropriately be

¹ Even Boulding in his excellent recent revision of *Economic Analysis*, offers two definitions, one following Ricardo and the other patterned after Pareto without explicitly mentioning that these definitions are different and that they may lend themselves to different uses [1, p. 265 and p. 512]. An examination of the latest editions of most leading intermediate and advanced texts shows this situation to be quite general [7] [3].

² At times a distinction is made between the payment needed to hold a factor in its present occupation and the return it could earn in other uses [5]. These could, of course, be different because of nonpecuniary elements involved in different lines of work. This distinction is not employed here because it introduces another, and for our purposes extraneous, variable into the problem. Although we must admit the validity of this contention, it should be ruled out by the simplifying assumption that the net nonpecuniary advantages of the occupations in question are the same.

tested by the contributions they make to these theories and their applications.

The traditional approach which views rent as the excess payment received by a factor over the minimum required to induce it to do its work passes the distribution theory test with flying colors. This is not unexpected since the concept had its origin in early formulations of distribution theory and was an integral part thereof. In its early as well as its present form it was designed to explain the income received by factors of production. It was first used in connection with the return paid to natural resources where no elasticity of supply was believed to exist. Since the compensation of such a factor generated no increase in productive effort it was viewed as a surplus payment. Even when this line of reasoning was later extended to other types of agents, the term rent continued to be used to explain all or part of the income shares they received. Here the rent or surplus designation was applied to that part of the compensation of the factor in excess of the payment necessary to bring forth its services. This was true both of the excess over the minimum necessary to get the factor to work at all and the excess over the payment needed to induce each additional increment of performance by the factor. In modern terms, of course, this concept views "economic rent" as the part of the compensation of the factor graphically depicted by the area between the supply curve to all uses of the factor and the horizontal line representing the rate pay received by the factor.

In addition to being a clear-cut distribution concept the traditional view of rent has the great conceptual merit of being closely related to the basic causes of scarcity. Rent in this sense exists because the supplies of the factors are less than perfectly elastic. This in turn is occasioned by those fundamental forces which cause the services of factors to be available in limited quantities only. When applied to factors other than land this approach forces us to focus on the behavior of factor suppliers. As a result the study of scarcity must be related to and draw upon the findings of other behavioral sciences.

The traditional rent concept also enables us to divide, conceptually at least, factor compensation into two parts, payments which induce factors to work and surplus which only confers a greater reward for work which would have been done anyway. In classical terms it distinguishes payments made to overcome "real costs," labor pain, abstinence, etc., from unnecessary bounties. Although we no longer subscribe closely to a "real cost" doctrine, it is still worthwhile to have a conceptual separation of those payments which are a necessary condition of production and those which are not. This is particularly important in applied economics where moral judgments are passed and policies suggested.

Pareto's approach has little to contribute to distribution theory. At best the set of concepts which ascribe individual rents to each of the industries or firms using an agent might enable us quantitatively to identify where the components of the total factor rent came from. If available and valid this information might be interesting, but it would contribute nothing that a thorough demand analysis would not provide. However, this type of compilation would be conceptually incorrect. Paretian rent from the point of view of any individual user of a factor is determined by how his demand influences the price of a fac-

tor assuming the demands of all other users to be *present in the market*. It is clear that the sum of these influences is not necessarily the same as the sum of the consequences when all of the individual demands are added to or subtracted from the market at the same time. Hence the total individual user rents would in all probability differ greatly from the total rent received by the factor. This difference would be especially large where supply is highly inelastic. In addition we must remember that, when any user adds his demand to the factor market, the price rise he produces generates "rents" not only for himself but also necessitates additional payments by all other users of the agent.

We should also point out that the multiple-rent concepts are of no value whatsoever when the theory of distribution is used in applied economics. Certainly no moral significance or basis of policy formation can be found in firm or industry rents. It should be clear, therefore, that the Pareto approach is a misfit from the standpoint of distribution theory and must justify itself, if at all, on other grounds.

When the theory of cost is considered, Pareto's approach fares somewhat better. The traditional concept draws its lines of division between necessary and unnecessary payment at the social level. What is necessary or unnecessary for the firm or the industry, however, is far different. Opportunity costs are real and must be met even though from the social point of view most of these payments are rent. Nevertheless this does not justify separate rent concepts for industries or firms. All that is required by the theory of cost at either firm or industry level is that the conditions of supply of the factor to the firm or the industry be known. Here the concepts of supply curves to industries or firms are entirely sufficient. In situations where a firm or industry can control the market sufficiently to influence costs, the theories of monopsony or oligopsony are adequate. As a result rent concepts at the firm and industry levels that identify and name the income generated by firms or industries are totally unnecessary to the theory of cost. They are worse than useless, however, because they confuse elements of cost and distribution theory. The distribution concept of rent is needlessly brought into the theory of cost while the cost-oriented user classification complicates the theory of distribution without enhancing our knowledge of factor income determination.

It should be clear from what has been said that on theoretical grounds the traditional concept is vastly superior. There is also no suggestion that the multiple-rent concepts are of any practical importance for either the businessman or the social planner. That they are lacking in moral significance and provide no meaningful policy guides should be evident as well. Consequently, the foregoing analyses suggest that economic rent is best defined as the surplus over the payment needed to induce a factor to do its work, and best defined in this way alone.³

One final argument in favor of the Pareto concept merits separate attention. Economists have long debated the role of rents in price making, some contend-

³ The suggestion that rent be defined as the opportunity cost of land (or for that matter of any other factor) and that any payment in excess of this amount be designated "factor profits" [9, pp. 269-75] must also be rejected. Under conditions of pure competition such a distinction is unimportant. On the other hand, when individual firms play a significant role in the market it is at least misleading and often clearly incorrect.

ing that rents are price determining while others argue that they are price determined. The multiple rent concepts of Pareto's followers seemingly permit a neat compromise which allows one to say that rents are from one stand-point price determined yet from another price determining. The argument proceeds somewhat as follows. Although rents in the sense of payments in excess of the minimum amount needed to induce factors to work are a result of the structure of prices and therefore price determined from the standpoint of the economy as a whole, many components of these payments are price determining to particular industries or firms. This is because the competitive bidding of other industries has built the parts of those rents not generated by the specific industry itself into opportunity costs which the industry must meet and which must be covered in the price of the product. Hence those components of "rents from the standpoint of the economy" which are embodied in opportunity costs are price determining from the standpoint of the industry while rents generated by the industry itself are price determined. Similarly, payments which are rents from the standpoint of the industry are often included in the market price of agents purchased by the firm and are price determining at that level.

This somewhat persuasive line of reasoning, however, misses the basic point in the entire price-determined price-determining controversy. In essence the controversy arises from a misleading statement of the problem. In fact under properly constructed sets of assumptions it is easy to prove that rent is either price determined or price determining. For example, consider a factor X , which is used in the manufacture of product A . Let us assume that X is used in the production of other products and the manufacture of A requires other agents. If now the conditions of supply of all of the agents are treated as given, as well as the technical coefficients and the preference patterns for all products except A , a change in the preference pattern concerning A will alter the price of A . This in turn will lead to a change in the demand by the producers of A for X and therefore a change in the price of X , of which the part in excess of the minimum supply price is rent. Consequently the change in the price of A changes or "determines" the rent of X . On the other hand, if every basic determinant of the system is treated as given, except the supply of X , a change in the supply of X will alter its price including its rent and, in consequence, the price of A . Here rent might be said to be price determining.

In reality, of course, economists have long known that rent is neither price determining nor price determined since neither rent nor price is a basic determinant of the system. They are co-determined as the result of the interaction of more fundamental forces. We know from Cassell's simple general equilibrium model that these forces are the condition of supply of the agents, the technical coefficients, and the preference patterns of consumers [2, pp. 134-55]. Their simultaneous interaction determines the prices of all products and factors, as well as the quantities of all goods produced and the allocation of resources. Rent along with price is a result not a causal factor.⁴ The validi-

⁴ It is true that autonomously determined rents or prices might themselves be basic elements in a more complex model. This however, is not the point at issue in this classical controversy.

ty of this basic viewpoint is not significantly altered when more sophisticated assumptions are introduced to the system.

Consequently, even this last argument fails to justify the confusion now existing with the rent concept.

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Drawings on an Old Envelope: Short-Run and Long-Run Average Cost Curves

Jacob Viner's path-breaking analysis of cost curves [6] taught the profession to relate long-run and short-run average costs (LAC and SAC) by means of an envelope curve, showing a set of SAC curves perched on the LAC curve. Viner's presentation, however, implies a cost function such that short-run total costs decrease as output increases and short-run marginal costs (SMC) are negative. Viner's diagram is reproduced here as Figure 1. Many widely used texts have followed Viner's example, some "improving" on its tidiness by making the SAC curves more nearly identical.¹ Negative marginal costs are inconsistent with the usual assumptions of efficient output and continuity of functions, and we doubt that the authors intended to depict them as negative.

The requirement that SMC not be negative sets a limit to the curvature of the downward-sloping portion of SAC . The limiting case would be one of constant short-run total cost, producing a rectangular hyperbola for its SAC . Any SAC curve drawn with steeper slope than the rectangular hyperbola passing through minimum SAC will imply negative marginal costs. As an illustration, Figure 1 shows such a hyperbola superimposed on SAC .²

¹ See, *inter alia*, Bain [1, p. 119], Chamberlin [2, p. 233], Due and Clower [3, p. 168], Samuelson [4, p. 469], and Stigler [5, pp. 131, 139, and 161].

² The interested reader is invited to measure the vertical and horizontal coordinates of selected points on the SAC curve and establish, as we did, that the implied SMC functions (except SAC 1) are negative.

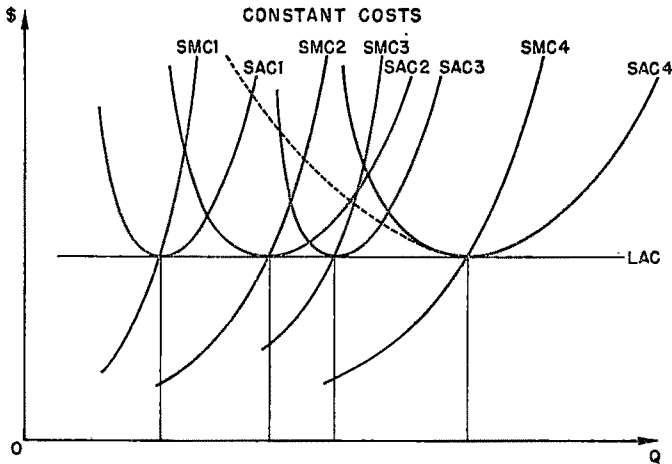


FIGURE 1

If the *LAC* curve is flat or nearly flat, the hyperbolic limits to successive *SAC* curves must become increasingly flat as one moves to successively larger scales of operation. For relatively simple pedagogical production functions, it can be shown that any *SAC*₂, representing a larger scale of output, tends to be wider and shallower than *SAC*₁, representing a smaller scale. (The width can be measured along a line some given distance above the minimum point.)

Consider a production function with equally spaced isoquants of identical curvature. Provided that the long-run expansion path is linear, the *SMC* curves will have the same shape and slope for any Q_m (where Q_m is the output at which *SAC*₁ is at a minimum). The shallowness and width of *SAC* is determined by the absolute change in total cost divided by the proportionate change in quantity. Since the absolute change in total cost associated with a given change in output will be invariant with Q_m , but the denominator will be negatively related to Q_m , identical *SMC*'s will necessarily yield flatter *SAC*'s as Q_m increases. For successive *SAC*'s not to become shallower and wider, the *SMC*'s must become steeper as Q_m increases; this will require that the isoquants become progressively more tightly curved as output level increases, so that the increased cost due to the curvature exceeds the effect of the increase in the denominator. When the long-run expansion path is nonlinear, limits can be shown to exist which will enforce the general tendency for *SAC*'s to become ultimately shallower and wider.

We can show algebraically the tendency for *SAC*'s to become shallower and wider as Q_m increases for the simple production function

$$Q = a^{1-x}b^x \quad (0 < x < 1)$$

Assume that a short-run production function can be identified by substituting some specific value b^* for one input. The quantity of the other input can

then be expressed as

$$(1) \quad a = \left(\frac{Q}{b^{*x}} \right)^{1/1-x}$$

The short-run total cost function (*STC*) is determined by multiplying the quantity of each input by its price, the two terms in the equation representing fixed and variable cost respectively. Dividing each term by Q yields the functions for *AFC* and *AVC*:

$$(2) \quad AFC = \frac{P_b \cdot b^*}{Q}$$

$$(2') \quad AVC = P_a \cdot \left(\frac{Q}{b^*} \right)^{x/1-x}$$

For any given value of b^* , there is a Q_m at which *SAC* is minimized. This can be determined by setting equal to each other the derivatives of *AFC* and *AVC* with respect to Q .

$$(3) \quad \frac{P_b \cdot b^*}{Q^2} = \left(\frac{P_a}{b^{*x/1-x}} \right) \left(\frac{x}{1-x} \right) Q^{(2x-1/1-x)}$$

Solve for Q and let the solution represent Q_m

$$(4) \quad Q_m = b^* \left[\left(\frac{P_b}{P_a} \right) \left(\frac{1-x}{x} \right) \right]^{1-x}$$

Let us assume that input prices are given and that we define units of input so that $P_a = P_b = 1$. *SAC* can then be expressed in terms of Q_m :

$$(5) \quad SAC = \left(\frac{Q_m}{Q} \right) \left(\frac{x}{1-x} \right)^{1-x} + \left(\frac{Q}{Q_m} \right)^{x/1-x} \left(\frac{1-x}{x} \right)^x$$

For any given value of x , *SAC* is a function of Q/Q_m . As a result, the width of *SAC* at any given height is proportional to Q_m . For instance, when $x=9/10$, *SAC* has a value of 6.2 when $Q/Q_m=0.2$ and 1.5, approximately. The width of each *SAC* at the height 6.2 is about 1.3 Q_m . Similar results follow for other values of x .

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The Foundations of the "War on Poverty" Reexamined

How rapidly will economic growth and full employment acting alone reduce the proportion of people living in poverty? In his communication "The Foundations of the 'War on Poverty'" [2] Lowell Galloway argued that the percentage of families living in poverty would decline to considerably lower levels by 1970 or 1980 than had previously been estimated by the Council of Economic Advisers [8].

Galloway regressed the logarithm of the percentage of families with incomes below \$3,000 in 1963 dollars on median family income and the unemployment rate. From his basic equation, Galloway estimated poverty rates in 1970 and 1980 under two assumptions regarding growth of median family income and unemployment rates. These results are shown in Table 1. His estimates of poverty rates in 1980 were a bit over half of those previously published in [8]. From these results Galloway concluded [2, p. 130] "In short, the case for substantial anti-poverty programs of a selective character is weakened by the findings of this paper. Rather, greater consideration should be given to the role which economic growth can play in eliminating poverty." Because Galloway's results have an obvious and important bearing on matters of considerable economic and political significance, they merit careful scrutiny, which this article attempts to provide. The following are the major conclusions.

1. Aggregate poverty statistics are inherently incapable of providing evidence for or against the backwash thesis—the thesis that poverty will not largely disappear with growth.

2. Galloway's results are sensitive to the functional form of his regression, and, in particular, the semilogarithmic transformation tends to overstate the impact of rising incomes on the poverty percentage.

3. Galloway's results are sensitive to the data he uses. Both Galloway and the CEA used poverty estimates based on a \$3,000 family income cutoff. If the semilogarithmic regression is run separately for the percentage of white and nonwhite families living in poverty, it appears that white family poverty is more sensitive to economic growth, but less sensitive to the unemployment rate than is nonwhite family poverty. On the other hand, the definition of poverty appears not to affect results significantly. Use of aggregate poverty statistics based on the definition of poverty developed by Mollie Orshansky for the Social Security Administration [6]

TABLE 1—REGRESSIONS OF POVERTY RATES ON MEDIAN FAMILY INCOME AND UNEMPLOYMENT RATES AND ESTIMATES OF POVERTY RATES IN 1970 AND 1980

| Model | Constant | Median Income (<i>M</i>) | Unemploy- ment Rate (<i>U</i>) | \bar{R}^2 | Predicted Poverty Rates (percentage of families) | | | |
|-----------------------------------|----------|-------------------------------|--|-------------------|---|-------------------|-------------------|-------------------|
| | | | | | 1970 ^a | 1970 ^b | 1980 ^a | 1980 ^b |
| 1. CEA estimates ^a | | | | | | | 10 | 13 |
| 2. Galloway estimates semi-log | 1.945100 | -.000121 (.000003) | .005086 (.001431) | .99+ ^a | 12.6 | 14.2 | 6.4 | 8.7 |
| 3. White poverty semi-log | 1.909827 | -.000116 (.000004) | .003723 (.002825) | .9847 | 10.8 | 11.6 | 5.4 | 6.7 |
| 4. Nonwhite poverty semi-log | 2.151410 | -.000090 (.000008) | .006249 (.005002) | .9138 | 30.6 | 32.8 | 18.8 | 21.4 |
| 5. Galloway estimates double logs | 6.306461 | -1.332623 (.035155) | .043557 (.020980) | .9919 | 14.3 | 15.1 | 9.7 | 10.9 |
| 6. White poverty double logs | 6.535934 | -1.410371 (.041096) | .037398 (.024526) | .9902 | 12.0 | 12.6 | 8.0 | 9.0 |
| 7. Nonwhite poverty double log | 5.805022 | -1.114743 (.109364) | .071077 (.056989) | .9082 | 32.9 | 34.8 | 23.8 | 26.5 |

^a Assumes that growth in median income occurs at the same rate as during the period 1947-56 and the unemployment is 4 per cent.^b Assumes that growth in median income occurs at the same rate as during the period 1957-63 and that unemployment is 6 per cent.^c As reported in Lowell Galloway, "The Foundations of the War on Poverty," *Am. Econ. Rev.*, March 1965 [2].^d It is not clear from the source whether or not this correlation coefficient is corrected for degrees of freedom.

[7]¹ yields substantially the same results as those based on the \$3,000 cutoff that Galloway uses, as far as the impact of economic growth on poverty in the aggregate is concerned. Unemployment rates are not related to poverty rates in a statistically significant way for the period 1959-64. Using the Orshansky poverty numbers, it is possible to carry out regressions of poverty rates on median income and unemployment for several population groups. This was done for seven groups, broken down by location of residence (farm, nonfarm), race (white, nonwhite), and sex of family head. These results provide further evidence that poverty is heterogeneous and should be disaggregated to be understood. Poverty among some groups seems highly sensitive to economic growth (e.g., white farm households); other groups are barely affected by it (e.g. nonfarm, nonwhite, female headed families). The case for special programs aimed at "disadvantaged groups" is strengthened by these results.

Since Orshansky also compiled statistics on the near poor,² the relationship between the number of poor and near poor (the "needy"), on the one hand, and median income and unemployment rates, on the other, was also examined. The chief result here is that the number of needy households is significantly related to median income, but not, in most cases, to unemployment. For most groups, the impact of higher median income is weaker on the number of needy households than on the number of poor households.

These results will be discussed in more detail below.

I. *The Need for Disaggregation*

The aggregate approach is inherently incapable of testing the backwash thesis. This thesis asserts that some disadvantaged groups benefit to a far smaller extent from growth than does the remainder of society. In the extreme case, assume that there are two groups: A, in which poverty rates decline linearly with growth in real income; and B, in which poverty rates are unaffected by growth in real income. Thus

$$(1) \quad {}^A P_t = a - bM_t,$$

$$(1a) \quad {}^B P_t = c,$$

where ${}^A P_t$ is the poverty percentage for group A at time t , M_t is median family income at time t , and a , b , and c are all parameters. The relation of the aggregate poverty rate, ${}^{A+B} P_t$, to median income would be

$$(2) \quad {}^{A+B} P_t = (a + c) - bM_t$$

Even if the fit were perfect, as Galloway's very nearly is, the results would be no evidence against (or for) the backwash thesis. Extrapolation implies

¹ The poverty threshold is defined on the basis of the Department of Agriculture's "economy" food budget for families of various sizes, adjusted downwards for farm families and for the aged. The resulting food budget is multiplied by factors determined on the basis of budget studies to get the poverty threshold. There are 108 thresholds depending on family size, age and sex of head, and location. See [6]

² Near poor families have incomes greater by from \$305 in 1963 dollars, for one-person family, to \$1,305, for a family of seven or more persons, than do poor families. See [6]

that the poverty rate goes to zero, but by assumption the poverty rate will not fall below c (assuming negative poverty rates for group A to be impossible). To examine the backwash thesis it is necessary to examine the relationship of poverty to family income and unemployment for the groups to which the backwash thesis might reasonably apply. In so doing, it should be kept in mind that even if economic growth seemed to erode poverty rapidly for every subgroup, the backwash thesis might still be valid, since within each subgroup there could be sub subgroups analogous to A and B just described.

II. Specification

Galloway's basic equation was of the form $P_t = ab^M c^U e_t$, where P_t is the percentage of families with incomes below \$3,000 in 1963 dollars in year t , M_t is median family income in year t , U_t is the national, average unemployment rate in year t , a , b , and c are parameters, and e_t is an error term. The estimated equation is shown as equation (2) in Table 1. By plugging in assumptions regarding growth of median income and unemployment rates, one may extrapolate the trend in the poverty rate. Galloway projects the poverty rate to 1970 and to 1980, as did the CEA using another model before him. The CEA's results, as reported by Galloway, are shown as equation (1) in Table 1.

The question arises of why the semilogarithmic transformation was chosen rather than some other form. Richard Muth has shown, in an unpublished paper [3], that the semilogarithmic form produces estimates which overstate the rate at which increases in income will reduce the percentage of families with incomes below any constant cutoff if incomes are distributed approximately log-normally. In that case the poverty percentage P is shown by

$$(3) \quad P = \int_{-\infty}^C \frac{1}{\sigma\sqrt{2\pi}} \exp - \frac{1}{2\sigma^2} (X - \mu)^2 dX$$

where C is the log of the poverty cutoff, X is the log of family income, and μ and σ are the first and second moments of the log normal distribution. Equation (3) implies

$$(4) \quad P = f\left(\frac{C - \mu}{\sigma}\right) = f\left(\frac{C - \log M}{\sigma}\right), \quad \text{or}$$

$$(4a) \quad \log P = g\left(\frac{C - \log M}{\sigma}\right),$$

where M is median family income, since $\mu = \log M$. Totally differentiating (4) and (4a) yields

$$(5) \quad dP = -\frac{f'}{\sigma} \left[\frac{dM}{M} + \left(\frac{C - \log M}{\sigma} \right) d\sigma \right]$$

$$(5a) \quad d \log P = -\frac{g'}{\sigma} \left[\frac{dM}{M} + \left(\frac{C - \log M}{\sigma} \right) d\sigma \right]$$

$$(5b) \quad \frac{\partial P}{\partial M} = \frac{f'}{\sigma M}$$

$$(5c) \quad \frac{\partial \log P}{\partial M} = -\frac{g'}{\sigma M}$$

$$(5d) \quad \frac{\partial P}{\partial \log M} = -\frac{f'}{\sigma}$$

$$(5e) \quad \frac{\partial \log P}{\partial \log M} = -\frac{g'}{\sigma}$$

Muth evaluated the partial differentials in (5a) and (5b) using a poverty cutoff of \$2,650 in 1959 dollars with adjustments to the cutoff for farm families. Since increases in income (M) will tend to reduce the value of the partial derivatives in (5b) and (5c), unless offset by decreases in σ (i.e., greater income equality), which seems unlikely, regression of P or $\log P$ on $\log M$ seems preferable to regression on M . In addition, Muth evaluated the partial derivatives (5b) through (5e) and found only (5e) roughly constant over time, indicating that double log regression would be more satisfactory than regressions of the poverty rate or its logarithm on median income.

The results of a double-logarithmic regression, where $P = aM_i^{-b}U_i^ce_i$ and the poverty rates are the percentages of families with income below \$3,000 (1964 dollars), are shown as equation (5) in Table 1. The estimated poverty rates are higher than those which Galloway obtained for 1970 and much higher for 1980. In fact, under the more favorable assumptions regarding growth and unemployment, the double-log results differ little from the original CEA estimate which Galloway criticized.

III. Categories of the Poor

The percentage of families with incomes below \$3,000 (1964 dollars) broken down by white and nonwhite families was obtained from Census. Both the semi-log and double-log models were fitted for these two groups. The parameter estimates are shown as equations (3) and (4), and (6) and (7) in Table 1, and the poverty rates are extrapolated to 1970 and 1980. In both models, the nonwhite poverty rate is less sensitive to median income and roughly twice as sensitive to unemployment rates as is the white poverty rate. The goodness of fit for nonwhite poverty rates is markedly lower than for white poverty rates. The greater sensitivity of nonwhite poverty to unemployment no doubt is caused by the fact that more nonwhites than whites are near poverty even when full employment conditions prevail, and, hence, are pushed under the line when economic activity slackens. No similarly obvious explanation for the lower value of the coefficient of median income in equations (4) and (7) in Table 1 is apparent. But,

whatever the reason, the difference is statistically significant, and the forecasts which emerge from those equations give a far less sanguine picture of the disappearance of poverty than do Galloway's estimates. After 15 years of full employment and high growth, between 18.0 per cent and 23.8 per cent of nonwhite families would be in poverty in 1980, about three times the white percentage.

The above findings suggest the desirability of still other breakdowns of the poverty population to ascertain whether economic growth affects in differing degrees various other subgroups among the poor. Accordingly the Orshansky poverty numbers were used for further regressions of the percentage of poor households on median income and unemployment. Because the poverty thresholds developed by Orshansky [7] [8] vary, depending on family size, location, and age, the more comprehensive household statistics can be used.

Unfortunately, poverty indices based on these definitions have been computed only for the years 1959-64. On the other hand, these data are available for subgroups, and statistics on the near poor have also been compiled. The semilogarithmic form of the model was fitted for poverty data on households (families plus unrelated individuals) in the following groups.³

A. All households

This regression was run to determine whether the Orshansky poverty numbers led to substantially different parameter estimates than did the longer series based on the \$3,000 poverty cutoff. The coefficients of median income are remarkably similar in the two instances. When the new definition and the shorter time span are used, the coefficient of the unemployment rate is not significant and the sign is negative. However, since in the long run changes in the unemployment rate have little effect on poverty rates relative to income growth, this problem is minor. The insignificance of the unemployment coefficient may perhaps be explained by the relatively narrow limits within which the unemployment rate fluctuated in the period 1959-64.

The impact of changes in median income on the number of needy (N = poverty plus near poverty) households is smaller than the impact on the number of households in poverty. This finding is explained by the fact that when income rises the ranks of the near poor neither grew nor shrank. As some near poor graduated into higher brackets, some previously poverty stricken households moved into the near poor category.⁴

These results suggest that neither the use of a different definition of poverty, nor the use of an index of poor households rather than poor families, will explain any large differences in the coefficients of median income for particular groups from those for the whole population.

³ The semilogarithmic form is used to evaluate the indictment of the backwash thesis on the most favorable terms.

⁴ If $P_{t-1} - P_t = N_{t-1} - N_t$ but $N_t > P_t$, then $\log P_{t-1} - \log P_t > \log N_{t-1} - \log N_t$. Hence, the resulting parameter estimates.

B. Nonfarm, white, male headed households

This group contained 67.0 per cent of all households and 35.4 per cent of all poor households in 1964—10.5 per cent and 17.6 per cent of all households in this group were classified as poor and needy respectively in 1964, the lowest for any group examined here. The results suggest that poverty in this group is more responsive to rises in median family income than is poverty for the nation as a whole. If the higher assumption regarding economic growth is employed here, the percentage of poor and needy will fall to 7.3 per cent and 13.7 per cent respectively in 1970; the numbers for 1980 will fall to 3.2 per cent and 7.8 per cent respectively. The coefficient for unemployment is disregarded because its sign is contrary to expectation and is insignificant. If these rates were applicable to all other groups, the case for special poverty programs would indeed be weakened.

C. Nonfarm, white, female headed families

This group contained 18.4 per cent of all households and 34.8 per cent of all poor households in 1964. The poverty and needy rates were 40.3 per cent and 47.6 per cent respectively in 1964. Changes in the percentage of households living in poverty are significantly correlated with changes in median family income, but the effect of a rise in median income on the poverty rate is smaller for this group than for male headed families discussed above. This smaller effect is quite consistent with a priori expectations based on the lower participation rates of women. If high economic growth persists, then the poverty rate in this group will fall to 34.6 per cent in 1970 and 24.7 per cent in 1980.

D. Nonfarm, nonwhite, male headed households

This group included 7.0 per cent of all households and 11.1 per cent of all poor households in 1964. The percentage of households living in poverty is significantly correlated with median income. However, the relation is weaker than for white households (group B above), both in the sense that the coefficient is smaller and in the sense that the fit is looser. In this group 31.6 per cent and 42.8 per cent of households were classified as poor or needy in 1964. Under the assumption of high growth and low unemployment the poverty and needy percentages will fall to 22.8 per cent and 36.1 per cent in 1970 and to 11.3 per cent and 22.5 per cent in 1980.

E. Nonfarm, nonwhite, female headed households

This group included 3.1 per cent of all households and 10.2 per cent of all poor households in 1964; 64.6 per cent were poor and 72.8 per cent were needy. The coefficient of median income is rather small and the fit is rather poor in each case, suggesting that economic growth has little impact on the poverty rate in this group. According to these equations, by 1970 the percentage of households classified as poor or needy would have fallen to 56.2 per cent and 67.3 per cent respectively; by 1980, to 43.8 per cent and 60.1 per cent respectively. However, little weight can be accorded to these esti-

TABLE 2—POVERTY REGRESSIONS FOR POPULATION CATEGORIES BROKEN DOWN BY RESIDENCE, RACE, AND SEX—SOCIAL SECURITY POVERTY DEFINITIONS^a

| Population Category | Dependent Variable—Log Poverty Rate | | | | Dependent Variable—Log Needy Rate | | | |
|---|-------------------------------------|-----------------------|------------------------|----------------|-----------------------------------|------------------------|------------------------|----------------|
| | Constant | Median Income | Unemployment Rate | R ² | Constant | Median Income | Unemployment Rate | R ² |
| All households | 1.989469 | -.000105 (.000008) | | .9638 | 1.924490 | -.000074 (.000008) | | .9364 |
| | 2.041078 | -.000109 (.000009) | -.000537 (.000517) | .9645 | 1.993651 | -.000079 (.000007) | -.007200 (.003889) | .9604 |
| Nonfarm, white, male headed households | 1.948490 | -.000141 (.000012) | | .9549 | 1.873968 | -.000096 (.000010) | | .9365 |
| | 1.976271 | -.000057 (.000009) | | .8534 | 1.977047 | -.000102 (.000007) | -.010730 (.003894) | .9760 |
| Nonfarm, white, female headed households | 2.044646 | -.000061 (.000009) | -.007118 (.005393) | .8763 | -.684837 | + .000272 (.000166) | + .116016 (.096841) | .0164 |
| | 2.366405 | -.000130 (.000024) | | .8261 | 2.171983 | -.000080 (.000027) | | .5256 |
| Nonfarm, nonwhite, male headed households | 2.203854 | -.000119 (.000024) | + .016922 (.014102) | .8433 | | | | |

^a In all following regressions there are two degrees of freedom when there are two independent variables and three degrees of freedom when there is one independent variable. The necessity of relying on so few degrees of freedom is regrettable, but not unprecedented. See, for example, (1).

TABLE 2—(Continued)

| Population Category | Dependent Variable—Log Poverty Rate | | | | Dependent Variable—Log Needy Rate | | | |
|---|-------------------------------------|-----------------------|-----------------------|----------------|-----------------------------------|-----------------------|-----------------------|----------------|
| | Constant | Median Income | Unemployment Rate | R ² | Constant | Median Income | Unemployment Rate | R ² |
| Nonfarm, nonwhite, female headed households | 2.007052 | -.000042 (.000023) | +.016275 (.013172) | .4427 | 1.912880 | -.000019 (.000013) | +.015251 (.007695) | .5174 |
| Farm, white, male headed households | 3.126233 | -.000226 (.000097) | -.058270 (.056971) | .2864 | 2.926363 | -.000177 (.000088) | -.053303 (.051312) | .1639 |
| Farm, white, female headed households | 4.004713 | -.000328 (.000084) | -.063733 (.049276) | .6712 | 3.125573 | -.000230 (.000057) | | .7077 |
| | | | | | 3.493655 | -.000254 (.000059) | -.038317 (.034695) | .7230 |
| Farm, nonwhite male headed households | 2.746581 | -.000143 (.000069) | | .2816 | 2.642792 | -.000118 (.000037) | | .5750 |
| | 3.257018 | -.000177 (.000067) | -.053137 (.039285) | .4050 | 3.011393 | -.000143 (.000027) | -.038372 (.015635) | .8116 |

mates, because none of the coefficients is significant. The proportion of poor falls more rapidly than the proportion of needy.

F. Farm, white, male headed households

This group included 4.8 per cent of all households and 6.0 per cent of all poor households in 1964; the poverty rate was 25.0 per cent and the needy rate was 34.4 per cent, both rates up sharply from 1963. If 1964 is excluded, the goodness of fit would be substantially improved and the coefficient of median income would be higher. A generally loose fit between farm poverty

TABLE 3—PER CENT OF FAMILIES, BY RACE, WITH LESS THAN \$3,000 ANNUAL INCOME, MEDIAN INCOME, MEDIAN FAMILY INCOME, AND UNEMPLOYMENT RATE, UNITED STATES 1947-1964

| Year | Per cent of Families With Less Than \$3000 Annual Income (1964 dollars) | | | Median Family Income (1964 dollars) | Unemployment Rate |
|------|---|-------|----------|-------------------------------------|-------------------|
| | All | White | Nonwhite | | |
| 1947 | 31.7 | 27.5 | 65.2 | 4,214 | 3.9 |
| 1950 | 31.2 | 27.6 | 61.6 | 4,293 | 5.3 |
| 1953 | 25.8 | 22.4 | 52.1 | 4,928 | 2.9 |
| 1954 | 27.4 | 24.0 | 52.9 | 4,819 | 5.6 |
| 1955 | 24.4 | 21.3 | 50.9 | 5,143 | 4.4 |
| 1956 | 22.2 | 19.1 | 49.7 | 5,478 | 4.2 |
| 1957 | 22.2 | 19.2 | 49.2 | 5,466 | 4.3 |
| 1958 | 22.5 | 19.3 | 50.8 | 5,457 | 6.8 |
| 1959 | 21.4 | 18.0 | 48.7 | 5,773 | 5.5 |
| 1960 | 20.9 | 17.9 | 44.3 | 5,904 | 5.6 |
| 1961 | 20.9 | 17.8 | 45.8 | 5,970 | 6.7 |
| 1962 | 19.7 | 16.6 | 43.3 | 6,135 | 5.6 |
| 1963 | 18.5 | 15.5 | 42.5 | 6,358 | 5.7 |
| 1964 | 17.6 | 15.3 | 37.3 | 6,569 | 5.2 |

Source: Census Bureau statistics.

and median income is to be expected because of the dependence of farm income on transfer payments which are only loosely correlated with changes in median income. If a separate series of median family income in agriculture were used, the relationship for 1964 would be less out of the ordinary, because median family income in agriculture remained virtually unchanged between 1963 and 1964, while median family income in general rose by 3.3 per cent. On the other hand, the coefficient is biased upward somewhat by the use in 1963 of a stricter definition of poverty than was used in other years. In any case, three of the most important influences on farm income are excluded, namely crop yields, the rate of out-migration, and the level of farm support outlays.

G. Farm, white, female headed households

This group included 0.5 per cent of all families and 0.8 per cent of all poor households in 1964; 35.5 per cent were poor and 44.7 per cent were needy; these rates were up only slightly from 1963. The coefficients of median income are significant and larger in absolute size than were those for male headed households (group F above). If high growth persists, then in 1970 the proportion of these families which are poor and needy will be 15.2 per cent and 22.9 per cent respectively and in 1980, 2.8 per cent and 5.9

TABLE 4—PER CENT OF VARIOUS HOUSEHOLD GROUPS POOR AND (NEAR POOR), UNITED STATES 1959-1964

| Year | All Households | Nonfarm | | | | Farm | | |
|------|----------------|---------------|---------------|----------------|---------------|---------------|----------------|----------------|
| | | White | | Nonwhite | | White | | Non-white |
| | | Male Head | Female Head | Male Head | Female Head | Male Head | Female Head | Male Head |
| 1959 | 24.0 (7.7) | 13.4 (7.7) | 45.2 (6.2) | 39.7 (10.9) | 69.4 (6.6) | 32.0 (9.2) | 63.8 (6.9) | 86.3 (7.0) |
| 1960 | 23.7 (7.0) | 13.5 (6.9) | 44.1 (5.4) | 39.2 (9.9) | 73.2 (5.3) | 30.9 (9.4) | 47.6 (7.5) | 81.1 (8.4) |
| 1961 | 22.6 (7.1) | 12.8 (6.8) | 42.8 (7.1) | 39.9 (9.5) | 72.8 (6.5) | 26.0 (8.3) | 43.1 (11.1) | 72.8 (7.6) |
| 1962 | 21.8 (7.4) | 12.3 (7.3) | 41.9 (6.2) | 38.8 (12.5) | 69.6 (6.7) | 23.3 (7.7) | 37.5 (13.5) | 78.8 (7.7) |
| 1963 | 21.0 (7.5) | 11.4 (6.9) | 41.8 (7.2) | 34.6 (12.4) | 69.1 (7.8) | 19.7 (8.3) | 34.9 (8.2) | 59.1 (13.9) |
| 1964 | 19.8 (7.6) | 10.5 (7.1) | 40.3 (7.3) | 31.6 (11.2) | 64.6 (8.2) | 25.0 (9.4) | 35.5 (9.2) | 69.6 (6.3) |

Sources: [4] [5]

per cent. The estimates for 1980 are probably excessively low since they imply lower poverty rates than for nonfarm, white, male headed families (group B above).

H. Farm, nonwhite, male headed households⁵

This category included 0.5 per cent of all households and 1.7 per cent of all poor households in 1964; 69.6 per cent were poor and 75.9 per cent were needy. The superior fit of the regression involving the needy rate is due to the use of a special definition of poverty for agriculture in 1963 which caused many families to be classified as near poor in that year who would have been classified as poor in other years. If high economic growth and employment

⁵ There are too few farm, nonwhite, female headed households to permit separate estimation.

persist, then the needy rate for this group will fall to 57.5 per cent in 1970 and to 24.7 per cent in 1980. In general, the evidence is not inconsistent with a hypothesis that agricultural poverty and near poverty will fall rapidly as economic growth proceeds, and no doubt also, as the out-migration, which depends on favorable nonfarm economic opportunities, continues. However, the evidence is also not inconsistent with the hypothesis that the farm poor remain poor even after they move off the farm, and should not be construed as evidence that poverty programs aimed at today's farm poor may not be necessary to prevent the mere transfer of poverty from the farm to the city.

IV. Summary

The rather optimistic view of the impact of economic growth on poverty advanced by Galloway is unwarranted. (1) Aggregate data cannot be used to test the backwash thesis. (2) His results are sensitive to the form of the model which he used. (3) If disaggregated data are used, a much gloomier picture emerges. Finally, it should be remembered that even if disaggregated data in every case yielded large negative coefficients for median income, a proponent of the backwash thesis could argue, and the data would not refute him, that within each category there exists a group of hard core poor, untouched by economic progress, whose presence justifies the passage of antipoverty legislation. And the debate about such programs would have to be resolved with other evidence than simple, or even multiple, regressions of the kind reviewed here.

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The Foundations of the "War on Poverty": Reply

Henry Aaron has indeed made a useful contribution to our knowledge of the incidence of poverty in the United States. For this we should be grateful and while I do not agree completely with his interpretations and conclusions let it be clearly stated for the record that I concur in his essential premise, viz., that the double log regression form is superior to the semi-log from the standpoint of shedding light on the relationship between poverty and economic growth in the United States. In fact, if anything, Aaron has been too modest in stating his case. Basically, he argues on conceptual grounds that the double log form is superior. This is, admittedly, a meaningful criterion. In addition, though, there are other criteria which might be employed in judging which of the two functional forms is more appropriate, viz., success in (1) explaining the past relationship between poverty and growth and (2) predicting the impact of growth on poverty levels.

The first of these criteria is a simple "goodness of fit" concept and on this score it is difficult to choose between the two regression forms. In fact, with this criterion a simple linear relationship performs quite well. However, the second criterion affords the possibility of a stronger test of the usefulness of the double log form vis-à-vis the semi-log. Essentially, this can be conducted by fitting both semi- and double log regressions to part of the data and using the result to predict the remaining observations.¹ I conducted such a test by fitting semi- and double log regressions to the 1947-59 data and from these predicting the 1960-64 poverty rates.² The results are shown in Table 1 and clearly indicate that the double log regression does a better job of predicting the 1960-64 poverty rates. Further, the bias in the semi-log predictions is in the direction of understating the actual amount of poverty.

The systematic tendency for the semi-log regression form to underpredict poverty in the 1960-1964 period casts doubt upon my earlier projections of future poverty rates. Aaron's projections seem much more reasonable. Those projections are in substantial agreement with those of the Council of Economic Advisers which is not surprising in that the poverty elimination rate dP/P used in those projections is perfectly consistent with the double log form of the relationship between poverty and aggregate economic conditions. Specifically, differentiating Aaron's double log equation yields the following:

$$(1) \quad \frac{dP}{P} = b \frac{dM}{M} + c \frac{dU}{U}$$

where P denotes the poverty rate, M median family income, and U unemployment. Thus, with constant unemployment dP/P depends solely on the

¹ This was done in part in my earlier paper [2] but only for purposes of predicting the 1963 poverty rate.

² I used Aaron's income series to make these projections. The income series in my original paper was erroneously labeled as being in 1963 prices. Actually, it is in 1957-59 prices.

rate of growth in median family income which is consistent with what the Council estimates had in mind.

On the basis of his double log poverty projections Aaron argues that I have been too hasty in rejecting the "backwash" thesis. Let me say at this point that I was not under the impression that I had categorically rejected it. Rather, I think that I did indicate that the significance of the "backwash" thesis depended upon its quantitative importance. This seemed to be somewhat minimal on the basis of my earlier projections. However, the double log projections indicate a stronger case for the thesis. In fact, by my own ground rules (as established in my earlier paper) it must be considered to be a significant thesis.

The portion of Aaron's paper dealing with 1959-64 subgroup poverty is quite interesting in that it provides insight into what I would call the "structure" of poverty. Aaron fitted his subgroup regressions in the semi-

TABLE 1—ACTUAL AND PREDICTED POVERTY RATES, 1960-1964

| Year | Predicted Rates | | Actual Rate |
|------|-----------------|------------|-------------|
| | Semi-Log | Double Log | |
| 1960 | 19.9 | 20.3 | 20.9 |
| 1961 | 19.7 | 20.1 | 20.9 |
| 1962 | 18.7 | 19.3 | 19.7 |
| 1963 | 17.5 | 18.3 | 18.5 |
| 1964 | 16.4 | 17.5 | 17.6 |

log form but I have taken the liberty of fitting them in double log form and predicting 1980 poverty levels for those subgroups in which the fit is significant at at least the five per cent level. These are four in number: white male nonfarm, white female nonfarm, white female farm, and nonwhite male nonfarm. The resultant projections are shown in Table 2 and indicate quite clearly that substantial changes are likely to take place in the composition of the poverty group in the United States over the next decade-and-a-half. Among these four subgroups, which account for over 90 per cent of 1964 households and 80 per cent of the 1964 poverty group, the white nonfarm females account for 42 per cent of the poverty group in 1964. By contrast, on the basis of the 1980 projections this group will account for 57 per cent of the poverty among these subgroups.³

These projections imply that the poverty problem in the future is likely to be more heavily concentrated among households with a female head. Thus, the "backwash" thesis appears to be most valid for the female household subgroup which, from the policy standpoint, argues for selective

³ These results are quite consistent with W. H. Locke Anderson's findings [1]. Further, if a projection is made for the female nonwhite nonfarm subgroup it also shows a building up of poverty. Thus, the general pattern of increasing female poverty is found in this group also.

programs aimed at improving the relative income status of this group. Further, these results are suggestive of the very real possibility that the "backwash" thesis is operative primarily in those groups whose attachment to the labor force tends to be relatively weak. This would not be surprising on a priori grounds for the greater the degree of labor force attachment among a subgroup the more likely they are to be affected by variations in general levels of economic activity. While this seems quite obvious it has very significant conceptual ramifications in that it implies that the validity of the "backwash" thesis does not depend upon any significant change in the relationship between work effort and income, i.e., the work-income nexus. In those subgroups whose labor force participation is high (males for example) there is little indication of a deterioration in the work-income relationship. Even among the male-nonwhite-nonfarm subgroup the 1980 favorable growth poverty projection indicates that between 50 and 60 per cent of poverty in this group can be eliminated by favorable growth condi-

TABLE 2—1964 ACTUAL AND 1980 PREDICTED POVERTY RATES, SELECTED SUBGROUPS

| Subgroup | 1964 Poverty Rate | Predicted 1980 Poverty Rate |
|-------------------------|-------------------|-----------------------------|
| Male, white, nonfarm | 10.5 | 4.4 |
| Male, nonwhite, nonfarm | 31.6 | 14.2 |
| Female, white, nonfarm | 40.3 | 28.0 |
| Female, white, farm | 35.5 | 4.9 |

tions. And, this assumes that the 1959-64 degree of discrimination against nonwhites and white-nonwhite educational differences exist in 1980.

In summary, Aaron's paper offers some very persuasive evidence as to the validity of the backwash thesis. At the same time it also suggests that the backwash thesis does not depend for its validity upon any assumption of a deterioration in the traditional work-income nexus in the United States. Rather, the backwash phenomenon seems to occur in precisely those subgroups in which the work-income relationship does not have the opportunity to function as strongly due to low labor force participation rates.

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On a Debt-Income Model of Consumer Instalment Credit Growth: Comment

The purpose of this note is to show that the controversy between F. R. Oliver and Alain Enthoven in their exchange in the June 1964 issue of this *Review* [3] [2] about the constancy of the borrowing-income ratio seems unnecessary on one point and yet still open to question on another. The exchange revolved around Section II of Enthoven's paper [2]. Enthoven's model can be summarized as follows:

$$(1) \quad Y(t) = Y(0)(1+r)^t,$$

where Y denotes income, r its constant relative rate of growth, and t the number of years which have elapsed since the base year. Then if $B(t)$ and $R(t)$ are new borrowing and repayments in year t ,

$$(2) \quad B(t) = bY(t),$$

and

$$(3) \quad R(t) = L[B(t), \dots, B(t-n)],$$

where b is constant and L represents some linear combination. By definition,

$$(4) \quad D(t) - D(t-1) = B(t) - R(t).$$

Using (2) and (3) in (4)

$$(5) \quad D(t) - D(t-1) = by(t) - L[bY(t), \dots, bY(t-n)].$$

As Oliver states, Enthoven assumes for the sake of definiteness that repayments are complete by the end of the third year; thus, (5) becomes:

$$(5a) \quad D(t) - D(t-1) = a_1Y(t) + a_2Y(t-1) + a_3Y(t-2) \\ = aY(t)$$

by using (1). Summing,

$$D(t) = \sum_{n=1}^t aY(n) + D(0) \\ = \frac{a(1+r)}{r} Y(0)[(1+r)^t - 1] + D(0).$$

Hence follows his basic conclusion:

$$(6) \quad \frac{D(t)}{Y(t)} = \frac{a(1+r)}{r} - \left[\frac{a(1+r)}{r} - \frac{D(0)}{Y(0)} \right] (1+r)^{-t},$$

and $D(t)/Y(t)$ appears to be approaching $a(1+r)/r$ as t tends to infinity.

In his model, Enthoven assumes that income grows by a constant proportion each year in (1), that borrowings in each year are a constant propor-

tion of income in (2), and that repayment conditions are constant in (3). Oliver argues in both words and algebra that under these restrictive assumptions, especially the last of the three, the debt-income ratio tends to a limit only in a "trivial" sense (our quotation); it always equals the "limit." He then points out that the constancy of the debt-income ratio implied by Enthoven's model is not found empirically, especially before the mid-1950s.

Enthoven replied that equations (2) and (3) were unnecessary for his model and only (1) and (4) were needed to maintain the conclusion. He conceded that Oliver's criticism was correct, but of no consequence, since he can afford to throw away (2) and (3). He then went on to demonstrate numerically that his model had performed well.

We have used the aggregate time series of borrowing, repayment, personal income, and disposable personal income¹ to estimate a few empirical relations between them. The following is what we found.

The aggregate time series of instalment credit extended, namely, new borrowing for each year 1946 through 1965, is very highly correlated linearly with either personal income or disposable income. The empirical results are as follows:

$$(7a) \quad \hat{B}(t) = -19.36 + 0.1730Y(t) \quad \text{with } r = 0.9921 \\ (1.66) \quad (0.0048)$$

$$(7b) \quad \hat{B}(t) = -20.94 + 0.2016Y'(t) \quad \text{with } r = 0.9937 \\ (1.63) \quad (0.0054)$$

where $B(t)$ is new borrowing during year t , $Y(t)$ personal income, and $Y'(t)$ disposable income (the numbers in parentheses under the coefficients are the respective standard errors). It is obvious from (7a) and (7b) that new borrowing is *not* proportional to income although certainly related linearly. These empirical results do not support Enthoven's postulate (2), $B(t) = bY(t)$, in that the constant (i.e., the intercept in billions of dollars) is definitely not zero. The t -ratio of the estimated intercept to its standard error is either -11.6 or -12.8 , both of which are highly significant, depending upon whether personal or disposable income is the regressor.²

Dividing both sides of (7a) by $Y(t)$ gives the following result:

$$\frac{\hat{B}(t)}{Y(t)} = -\frac{19.36}{Y(t)} + 0.173.$$

Both Oliver and Enthoven have observed that the borrowing-income ratio has been rising at a decreasing ("absolute") rate since 1946. This is due to the simple fact that the increasing $Y(t)$ over time reduces the magnitude of the ratio $-19.36/Y(t)$. The result is a borrowing-income ratio $B(t)/Y(t)$

¹ The data for these and all other calculations in this discussion are taken from [4]. The income data are personal income and disposable income from Table C-14, p. 226. The instalment credit data are from Table C-50, p. 269. The borrowings and repayments are from Table C-51, p. 270.

² If $B(t)=0$ in equation (7a), then $Y(t)=112$ which naively interpreted, simply means that there would be no new borrowing if personal income dropped back to 112 billion dollars.

increasing at a decreasing rate and approaching the constant 0.173 from below.

On the basis of the empirical finding, let Enthoven's postulate (2), the borrowing-income relation $B(t) = bY(t)$, be modified as $B(t) = k + bY(t)$ where $k \neq 0$. Would this modification impair his debt-income growth model? To answer this question, let

$$(2') \quad B(t) = k + bY(t),$$

$$(3') \quad R(t) = L[B(t), B(t-1), B(t-2)],$$

and

$$(4') \quad D(t) - D(t-1) = B(t) - R(t)$$

$$\begin{aligned} D(t) - D(t-1) &= k + bY(t) \\ &\quad - L[k + bY(t), k + bY(t-1), k + bY(t-2)] \\ &= k' + a_1Y(t) + a_2Y(t-1) + a_3Y(t-2) \\ &= k' + aY(t) \end{aligned}$$

where

$$k' = k - L[k] = L'[k] = 1k,$$

i.e., k' is proportional to k . Then

$$\begin{aligned} (5') \quad D(t) &= k' + aY(t) + D(t-1) \\ &= k' + aY(t) + k' + aY(t-1) + D(t-2) \\ &= \dots \\ &= tk' + a \sum_{n=1}^t Y(n) + D(0) \\ &= \frac{a(1+r)}{r} Y(0)[(1+r)^t - 1] + D(0) + tk'. \end{aligned}$$

The debt-income ratio (not borrowing-income ratio) is

$$\begin{aligned} (6') \quad \frac{D(t)}{Y(t)} &= \frac{\frac{a(1+r)}{r} Y(0)[(1+r)^t - 1] + D(0) + tk'}{Y(0)(1+r)^t} \\ &= \frac{a(1+r)}{r} - \frac{a(1+r)}{r(1+r)^t} + \frac{D(0) + tk'}{Y(0)(1+r)^t} \\ &= \frac{a(1+r)}{r} - \left[\frac{a(1+r)}{r} - \frac{D(0)}{Y(0)} \right] (1+r)^{-t} \\ &\quad + \frac{tk'}{Y(0)(1+r)^t}. \end{aligned}$$

This is the same as equation (6) above (or Enthoven's original equation (9) in his 1957 article in this *Review*) except that the last term here is new and comes about precisely from the nonzero intercept. The term $tk'/Y(0)(1+r)^t$ is a ratio of two functions of t . Will the term vanish as t tends to infinity? In other words, $\lim_{t \rightarrow \infty} tk'/Y(0)(1+r)^t = ?$ It is a so-called indefinite form of ∞/∞ . By l'Hospital's rule,

$$(7) \quad \lim_{t \rightarrow \infty} \frac{tk'}{Y(0)(1+r)^t} = \lim_{t \rightarrow \infty} \frac{d(tk')/dt}{d[Y(0)(1+r)^t]/dt}$$

$$\lim_{t \rightarrow \infty} \frac{k'}{Y(0)(1+r)^t \log_e t} = 0.$$

Thus, the limit of Enthoven's debt-income ratio remains intact even with the empirically determined non-zero intercept, that is

$$(8) \quad \lim_{t \rightarrow \infty} \frac{D(t)}{Y(t)} = \frac{a(1+r)}{r}.$$

The finding and argument above seem to explain the increasing borrowing-income ratio and yet support Enthoven's conclusion. However, we have found a rather poor empirical relation between the annual increase in debt as a function of income:

$$(9) \quad \Delta D(t) = -0.4957 + 0.01132 Y(t) \quad \text{with} \quad r = 0.5602$$

where

$$\Delta D(t) = D(t) - D(t-1).$$

This is an empirical estimation of equation (4) above (or Enthoven's original equation (6) in his 1957 article): $D(t) - D(t-1) = aY(t)$. Replacing $\Delta D(t)$ by its three-year moving averages as the regressand yields a simple correlation of 0.7063 which is not much better than 0.5602. Adding a $Y^2(t)$ term as a second regressor to the regression of $\Delta D(t)$ on $Y(t)$ merely improves the correlation from 0.5602 to 0.6647. These empirical results are particularly disappointing in view of the observed close³ relation between repayment and borrowings, current and lagged:

$$(10) \quad \hat{R}(t) = 0.588 + 0.371B(t) + 0.387B(t-1) + 0.213B(t-2),$$

with

$$R = 0.9995.$$

This corresponds to equation (3) above (or Enthoven's original equation (ii) in footnote 18 of his 1957 paper).

Oliver's concern about "a breakdown in one or more of Enthoven's assumptions, in particular, the constancy of the borrowing-income ratio" [3, p. 417], is borne out empirically. Enthoven's equation (4) is stated as:

³ The extremely high correlation of 0.9995 is partly spurious due to the high multicollinearity among the regressors.

$$(4) \quad D(t) - D(t-1) = aY(t)$$

and states in effect that the absolute increase in the stock of debt outstanding each year is proportional to a variable which grows at the same rate as income. In his *Reply* to Oliver, Enthoven presents (in Table 1) some empirical evidence to suggest the relative stability of "a" in equation (4) above. The empirical findings are based upon three-year averages; when values of "a" are computed for each of the post-World War II years, a significantly different pattern of stability (or instability) is in evidence.

TABLE 1—INDIVIDUAL VALUES OF THE RATIO OF CHANGE IN DEBT TO INCOME

| Year | $a(t) = \frac{D(t) - D(t-1)}{Y(t)}$ | Year | $a(t) = \frac{D(t) - D(t-1)}{Y(t)}$ |
|------|-------------------------------------|------|-------------------------------------|
| 1946 | .00957 | 1956 | .00854 |
| 1947 | .01319 | 1957 | .00612 |
| 1948 | .01095 | 1958 | — .00062 |
| 1949 | .01253 | 1959 | .01421 |
| 1950 | .01370 | 1960 | .00895 |
| 1951 | .00231 | 1961 | .00166 |
| 1952 | .01507 | 1962 | .01018 |
| 1953 | .01249 | 1963 | .01230 |
| 1954 | .00194 | 1964 | .01140 |
| 1955 | .01715 | 1965 | .01530 |

Mean value of "a" = 0.00985. Standard deviation of "a" = 0.00510.
Coefficient of variation = 51.8%

The data in Table 1 above and the measures of dispersion computed from those data suggest that the assumption of the constancy of "a" is, as Edward H. Chamberlain might term it, an "heroic assumption." In the latter respect, we share Oliver's concern.

Enthoven also advanced an interesting argument in his 1957 paper (especially footnote 19) that the relative variance of the differences [$B(t) - R(t)$ or $\Delta D(t)$] between two series [$B(t)$, $R(t)$] may be much greater than those of the two separate series. Our computations based upon the empirical data yield the following results:

| | | |
|---------------------------------|---------------------------|------------|
| Mean of borrowings | $= \bar{B}(t)$ | $= 37.79$ |
| Variance of borrowings | $= S_{B(t)}^2$ | $= 318.21$ |
| Relative variance of borrowings | $= S_{B(t)/\bar{B}(t)}^2$ | $= 8.42$ |
| Mean of repayments | $= \bar{R}(t)$ | $= 34.55$ |
| Variance of repayments | $= S_{R(t)}^2$ | $= 276.51$ |

| | | |
|---|---|------------|
| Relative variance of repayments | $= S_{R(t)/\bar{R}(t)}^2$ | $= 8.00$ |
| Covariance of borrowings and repayments | $= S_{B(t)R(t)}$ | $= 295.22$ |
| Variance of differences between borrowings and repayments | $= S_{B(t)-R(t)}^2 = S_{B(t)}^2 + S_{R(t)}^2 - 2S_{B(t)R(t)}$ | $= 4.28$ |
| Mean of differences | $= \bar{B}(t) - \bar{R}(t)$ | $= 3.24$ |
| Relative variance of differences | $= \frac{S_{B(t)-R(t)}^2}{\bar{B}(t) - \bar{R}(t)}$ | $= 1.32$ |

Since the relative variance of the differences (1.32) is much smaller than either the relative variance of the borrowings (8.42) or the relative variance of repayments (8.00), our empirical findings are diametrically opposed to Enthoven's argument on the basis of relative variances. Thus, Enthoven's derived equation, i.e.,

$$D(t) - D(t-1) = aY(t)$$

remains open to question, and as Enthoven admits, his model comes to naught if equation (4) is abandoned.

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Allocative Efficiency vs. "X-Efficiency": Comment

Professor Harvey Leibenstein's extension of the concept of efficiency to the "non-allocative" area of resource utilization within business firms [3] is a welcome addition to the literature of economics. The purpose of the present note is to comment briefly on one related issue: the limited role assigned by economists to competition as an efficiency-making force.

The burden of the evidence cited by Leibenstein is that entrepreneurs do not always maximize productive efficiency. That this is not a novel

proposition¹ in no way lessens its significance for the theory of the firm, for it undermines one of the most fundamental postulates of economic theory—the assumption that firms will combine the various factors of production in the most productive manner.

A curious double standard has always existed in microeconomic theory. While it has been assumed that the self-interest of businessmen would be an adequate guarantor of efficiency in resource use within the business firm, the competitive pressure of other firms, a force external to the firm itself, has been considered necessary to insure efficiency in the firm's dealings with the consuming public. It is this double standard which makes it necessary to distinguish between "allocative efficiency" and "X-efficiency." The difference between these types of efficiency is at bottom the difference between the market and the firm as institutions through which resources are organized and channeled for ultimate consumption.

Underlying the theory of the firm is the notion that, given the structure of the market, one firm is as efficient as another. Even the monopoly, albeit producing a smaller output and selling it at a higher price than would obtain under competitive conditions, is assumed to be producing in a "least costly" way, exactly as would the purely competitive firm. The internal efficiency of business organizations has not been challenged in economic theory. The so-called theory of the firm is, strictly speaking, not a theory of the firm at all but a theory of market structure.

Basic to the reasoning by which economists have limited the analysis of efficiency to the market is a concept of competition that has historically been limited to exchange. Despite Adam Smith's emphasis on division of labor as the fundamental element in the technique of production and the great source of economic growth, an emphasis quite in contrast to the earlier emphasis of the mercantilist writers on trade itself as the source of surplus, the mercantilist's overriding concern with price continued, with Smith and his successors, to be the central theme of economic inquiry. The purely analytical (as distinct from the ideological) function of competition was for Smith precisely what it had been for Sir James Steuart before him—to lower price to the minimum sustainable level [6, pp. 196-197], [5, Chap. 7]; not, as for Steuart, in order to stimulate exports, [6, p. 284], but rather in order to provide "cheapness of consumption" which, according to Smith, was the proper object of domestic policy [5, p. 706]. For Ricardo and the classical economists, competition served to bring market price down to long-run cost. That cost itself, however, even after economists stopped referring to it as "natural," was assumed to be minimal, and output was assumed to be produced in the most efficient manner.

¹That internal organization of firms might not be of maximum efficiency has, of course, been suggested by economists from time to time, but such suggestions have not been incorporated into the theory of the firm. Labor economists, in defending minimum wage legislation, have sometimes held that internal efficiency might be improved if firms were subjected to the "shock effect" of higher wage costs. Again, the ILO studies showing that "low productivity is frequently caused by top management's concern with the commercial and financial affairs of the firm rather than with the running of the factory," which "was frequently treated as a very subordinate task" [3, p. 406] is reminiscent of Veblen's distinction between making goods and making money [8].

Competition, in short, was exclusively with respect to price in the market; it did not relate to the technique of producing.

The incorporation into economic theory of a concept of competition enlarged to include method of production as well as price would help to bridge the gap between the two areas of economic inquiry for which Leibenstein's discussion of "X-efficiency" is most relevant: the theory of the firm and the problem of economic growth. The *Wealth of Nations*, of course, was a theory of society, not merely a theory of markets, and for Adam Smith questions of economic growth and resource allocation were integrally related both to each other and to the admixture of freedom and competition implicit in his use of the term "natural liberty." But the concept of competition was not well developed by Smith,² and its subsequent refinement came at the hands of economists whose concern was not with analyzing the changing economic structure of society but rather with developing, with mathematical rigor, the central question of resource allocation by means of a pricing system [7]. The theorist's concept of competition came increasingly to be tied to price, while questions of competition by means of productive and organizational technique, as part of the larger problem of economic change and growth, became the province of the economic historians. The latter have necessarily given a broader meaning to competition than that traditionally associated with the theory of the firm, such as developments affecting the internal organization of firms and vertical, horizontal, and conglomerate integration—developments frequently viewed by the theorist as deviations from competition.³

Any significant integration of the theory of resource allocation with the problems of economic growth—a step which would constitute a major advance in economic inquiry—must necessarily include, it would seem, an expansion of the concept of competition to take into account more fully than has so far been done the behavioral responses of firms to the internal organization of their competitors, apart altogether from market structure. In spite of the great advance made by E. H. Chamberlin [1] toward a more meaningful theory of competition, the interfirm differences that are well developed in economic theory remain differences of markets, i.e., of exchange conditions. Competition in the realm of production is still analytically unrefined—as is illustrated by the ambiguity of the "competition" and the "competitive pressures" to which Leibenstein refers at several points [3, pp. 408, 412, 413]. Despite the emphasis in recent years on nonprice competition by whatever name, it is hard to es-

² Smith was aware of the technological element in competition. Lowered prices and increased demand, he wrote "... encourages production, and thereby, increases the competition of producers, who, in order to undersell one another, have recourse to new divisions of labour and new improvements of art, which might never otherwise have been thought of" [5, p. 706]. But the essence of competition was the effort to undersell. Its subsequent development came to be limited to price, while its technological and organizational implications were "impounded" in the *ceteris paribus* assumption.

³ To the theorist, for example, the oligopolization of the meat packing industry by the big five was not a "competitive" development, for it resulted in a market structure dominated by a few firms. To the historian, on the other hand, it was the "competitive" response of Armour, Morris, Cudahy, and Schwarzschild & Sulzberger to the innovations in production and marketing introduced by Gustavus Swift [2, pp. 25-27].

cape the conclusion that the theory of the firm still rests on a concept of competition (or, as with the Chamberlinian model, on deviations therefrom) that is essentially what it was when it was developed by Jevons and Edgeworth almost a century ago.

Clearly, economic growth is the central question for our times. Equally clearly, competition "... within a rigid pattern of invariant conditions, methods of production and forms of industrial organization . . ." cannot be, as Schumpeter recognized and stressed [4, p. 84], competition that is meaningful for the analysis of economic growth. On the contrary, an economically significant concept of competition must, as he pointed out, pay increased attention to the managerial and organizational function of the firm, and "internal, especially technological, efficiency" [4, p. 106]. Leibenstein's findings that "... in a great many instances the amount to be gained by increasing allocative efficiency is trivial while the amount to be gained by increasing X-efficiency is frequently significant" [3, p. 413] is a cogent reminder of the relevance, in this respect, of Schumpeter's insight.

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Allocative Efficiency vs. "X-Efficiency": Comment

In his article on "Allocative Efficiency vs. 'X-Efficiency'" Professor Harvey Leibenstein pointed out that "economic theory focuses on allocative efficiency to the exclusion of other types of efficiencies which, in fact, are much more significant" [3, p. 392 fl.]. Economic theory presumes that managers of business firms are already making optimum input decisions so consequently it tends to focus on allocation of goods and services along a production surface as opposed to movements from a less efficient production surface to a higher one.¹

¹ Though this point is not relevant to the empirical data reported in this paper, and

The purpose of this paper is to report findings that are especially noteworthy because they involve an opportunity to test the "X-efficiency" hypothesis under well-controlled conditions involving 22 separate restaurants, and the results are determinant and meaningful in reinforcing the evidence cited by Leibenstein regarding the impact of managerial performance on efficiency of business firms. The data presented by Leibenstein cover different circumstances that are impressive in their totality and general consistency. However, the basic reports he mentions reveal, on careful study, enough vagueness in the underlying figures that a skeptic might still claim he is unconvinced of the existence of "X-efficiency," or its importance. Some changes in productivity that were noted took place over a long period of time and thus became associated with other coincident activities such as the acquisition of new equipment, technological innovations of a scientific nature, or a change to a better quality of worker.

The difficulties in making careful measurements of productivity are well known. They include such problems as: How to compare output when the product mix differs; how to measure the quality of output; how to isolate the impact created by such things as new equipment; how to adjust for the fact that the use of raw materials and fuel per unit of output may not be standardized.

The authors cited by Leibenstein were certainly aware of the difficulties, so a sampling of their studies contains such cautionary remarks as: "This method gives an essentially crude comparison of output per head" [4, p. 2]. "The results . . . may be taken as a rough measure of organizational input inasmuch as other factors were, to a large degree, constant" [2, p. 303]. "But the enquiries were handicapped in all cases by the fact that the fundamental changes had been made before any steps could be taken to obtain a first-hand account of the position before incentives were introduced" [1, p. 88]. "Although the choice of industries has been governed by the need to ensure some degree of accuracy for these estimates, there is nevertheless the possibility of serious errors. The output series in particular are subject to all the hazards encountered in the measurement of long-period production movements. Index-number biases and quality changes are inescapable sources of serious errors. . . . For these reasons, it is appropriate to regard the estimates simply as indications of the order of magnitude of the changes that have taken place" [5, p. 106].

In contrast, however, the circumstances reported in the present study hold almost everything constant except a change in management. Furthermore, there are enough observations that statistical validity can be implied. Briefly, the data came from a large company that operates, primarily on a franchise

may be unnecessarily argumentative, it is interesting to consider that this possible distortion in economic theory parallels and may help explain the sometimes-noted schism between university economics departments and business schools. The economics department tends to assume that firms operate on their most efficient production surface so the only issue is one of allocative efficiency; business schools on the other hand emphasize the need to increase efficiency within the firm, and may slight questions of allocative efficiency.

basis, a string of restaurants. Because the management of this company has confidence in its ability to guide the franchisee, and because of the franchisor's desire to have a uniformly good reputation that will be associated with every eating place despite the fact that each is operated by a different owner, the parent company provides a great deal of direction. The details are too lengthy to enumerate but a few examples will suggest the extent of company supervision. The parent company prescribes each item that will be on the menu, and its head chef determines optimum recipes so that each franchisee-operator is told exactly how many pounds of coffee to use in the coffee-making equipment, how many slices of bacon go with an order of eggs, how many ounces of meat to put in an entree, etc. Not only are the menus and recipes standardized, all ingredients, down to napkins and tableware, are supplied to the franchisee-operators by the parent company. A service manual used at each restaurant standardizes service. For example, when a waitress approaches the table for the first time her introductory remarks are specified exactly. Each unit operates from a company-designed restaurant, occupied on a lease basis. The parent company uses electronic computers to record sales and cost items, and relieves the franchisee of practically all bookkeeping details by collecting weekly all expense and revenue records. The franchisor pays the itemized bills for the franchisee so that the franchisee-operator is only required, so far as bookkeeping is concerned, to deposit his cash receipts daily and sign three checks per week—the amount of each check representing the total for three major cost categories, labor, supplies, and taxes. In short, the accounting records for each restaurant are completely uniform, and so handled as to relieve the franchisee of almost all detail. The management of the franchising company believes, in fact, that its operations are so thoroughly supervised and organized that a franchisee (who typically pays approximately \$30,000 for the privilege of becoming a franchisee in a large restaurant and \$10,000 in a snack shop) is selected only from people who have not had experience in the restaurant industry. The operating company has found that those with prior restaurant experience are likely to think they know so much about the business that they try to install practices other than those specified by the franchisor.

The parent company prefers to have each of its outlets operated by a franchisee-owner, who is an independent businessman legally as well as in terms of his remuneration. But from time to time the company finds it necessary to operate a franchise outlet itself. The franchising contract contains a provision allowing the franchisee to terminate his franchise and be repaid his original investment; also the franchisor-parent company has the right to terminate, with a similar repayment clause, if it can be proved that the franchisee has not been living up to food and service standards prescribed in the original contract. As a matter of practice, termination by the franchisor has proved difficult, so most of the situations where a franchisee owner quits arise from death, illness, or other personal reasons on the part of the franchisee. Until such time as the company can find a new franchisee who will own and operate the restaurant it is necessary for the company to continue the operation itself. For this purpose it assigns to the restaurant a manager from a group who have

worked with the parent franchisor for many years and are experienced in the restaurant operations of this chain.

As a result of installing electronic data processing equipment which makes extensive reports on the sales, costs, profits, and operations of each food outlet, the management has developed an accurate record of the performance for each restaurant during periods of franchisee-ownership or franchisor-management control.

These changes involve absolutely no alteration in capital investment, materials used, production technology, layout, or accounting records; the only variation is the employment relationship of the restaurant manager to the parent company. Since this chain imposes such close control over each outlet it was presumed that there would be very little shift in the efficiency or profitability of an outlet when it was being operated by a company-installed manager instead of a franchisee owner.

Because the assets invested in the restaurant are unchanged when the supervision shifts between franchisee-owner and company-manager three measures that can be used to evaluate a change in productivity or efficiency are: profit as a per cent of sales; total sales; or total profit. From basic records made available by the franchisor, the necessary figures for these three measures were calculated and tabulated for every restaurant which had experienced a change in managership since installing computing equipment. The results are presented in Table 1.

The following explanations will make the table more understandable. The restaurants are of two types: those listed A - M are large, fairly complete, family-eating establishments, those listed N - V are smaller, quick-snack places and have correspondingly smaller investment and sales. The company is very careful to measure profit in exactly the same way under either type of management. Profit is defined as revenue less all relevant expenses except manager's salary and income tax; this means the company-manager figure for profit is identical to the profit earned by the franchisee-owner. The restaurants are located in all parts of the United States, and are of varying size and tenure-of-establishment, so even within similar types of restaurant there is considerable variation in sales. In some cases, as can be seen from the table, franchisee-ownership (F.O.) preceded company-manager supervision (C.M.); in some cases F.O. followed C.M.; in some cases there was a sequence of changes from F.O. to C.M. and back to F.O. In short, the situations cover all conditions and circumstances faced by the parent company.

Those following general conclusions can be drawn from a study of Table 1.

(a) Out of 22 restaurants where the type of management changed, the shifts in profit margins were adverse to company managers except in three cases.

(b) Considering every case where the C.M. followed an F.O., weekly sales were, on average, 99.6 per cent of the sales rate achieved by the F.O. (If the special case where a C.M. operated a restaurant for only seven weeks that happened to include a July 4th holiday is eliminated, the ratio is 92.7 per cent or sales dropped 7.3 per cent. Considering every case where the F.O. followed a period of C.M., sales rose 5.8 per cent. (Again, if the time an F.O. followed

TABLE 1^a

| Restaurant | Type of Management | Number of Weeks | Average Weekly Sales (dollars) | Average Weekly Profit (dollars) | Profit Divided by Sales |
|------------|--------------------|-----------------|--------------------------------|---------------------------------|-------------------------|
| A | F.O. | 19 | 3,463 | 124.67 | 3.6 |
| | C.M. | 10 | 1,925 | 28.88 | 1.5 |
| B | F.O. | 6 | 3,514 | 383.03 | 10.9 |
| | C.M. ^b | 7 | 8,548 | 700.94 | 8.2 |
| | F.O. | 47 | 4,881 | 307.50 | 6.3 |
| C | F.O. | 33 | 2,702 | 86.46 | 3.2 |
| | C.M. | 5 | 2,372 | -203.99 | - 8.6 |
| | F.O. | 49 | 2,648 | 5.29 | 0.2 |
| | C.M. | 19 | 3,210 | -381.99 | -11.9 |
| | F.O. | 6 | 2,581 | -77.43 | -3.0 |
| D | F.O. | 141 | 2,665 | 149.24 | 5.6 |
| | C.M. | 12 | 2,353 | -425.89 | -18.1 |
| E | F.O. | 16 | 4,327 | 385.10 | 8.9 |
| | C.M. | 22 | 4,001 | 72.01 | 1.8 |
| | F.O. | 13 | 4,153 | 494.20 | 11.9 |
| F | F.O. | 18 | 3,922 | -270.62 | - 6.9 |
| | C.M. | 10 | 2,835 | -107.73 | - 3.8 |
| G | C.M. | 85 | 2,340 | -102.96 | - 4.4 |
| | F.O. | 60 | 2,449 | 115.10 | 4.7 |
| | C.M. | 33 | 2,758 | - 35.85 | - 1.3 |
| H | F.O. | 35 | 3,497 | 304.23 | 8.7 |
| | C.M. | 10 | 3,404 | - 44.25 | - 1.3 |
| | F.O. | 46 | 3,100 | 179.80 | 5.8 |
| I | F.O. | 28 | 4,030 | 245.83 | 6.1 |
| | C.M. | 17 | 4,612 | 599.56 | 13.0 |
| | F.O. | 59 | 4,790 | 507.74 | 10.6 |
| J | F.O. | 104 | 2,862 | 97.30 | 3.4 |
| | C.M. | 39 | 3,073 | - 70.67 | - 2.3 |
| K | C.M. | 60 | 4,034 | 411.46 | 10.2 |
| | F.O. | 21 | 4,479 | 613.62 | 13.7 |
| L | F.O. | 52 | 5,646 | 982.40 | 17.4 |
| | C.M. | 35 | 3,020 | 99.66 | 3.3 |
| | F.O. | 12 | 5,710 | 970.70 | 17.0 |

^a The abbreviation F.O. stands for franchisee-owner; C.M. indicates the restaurant was supervised by a company-employed manager. The number of weeks shows the duration of time under each type of management.

^b This seven-week period of company management happened to include a July 4th weekend that boosted sales volume abnormally.

TABLE 1^a—(Continued)

| Restaurant | Type of Management | Number of Weeks | Average Weekly Sales (dollars) | Average Weekly Profit (dollars) | Profit Divided by Sales |
|------------|--------------------|-----------------|--------------------------------|---------------------------------|-------------------------|
| M | F.O. | 7 | 4,673 | 570.10 | 12.2 |
| | C.M. | 42 | 2,997 | -113.88 | - 3.8 |
| N | F.O. | 26 | 1,353 | 345.01 | 25.5 |
| | C.M. | 23 | 1,066 | 211.06 | 19.8 |
| O | F.O. | 29 | 1,187 | 186.35 | 15.7 |
| | C.M. | 33 | 950 | 39.90 | 4.2 |
| P | F.O. | 27 | 1,027 | 190.77 | 9.9 |
| | C.M. | 35 | 938 | 53.46 | 5.7 |
| Q | F.O. | 15 | 451 | 51.41 | 11.4 |
| | C.M. | 15 | 445 | - 35.15 | - 7.9 |
| R | F.O. | 40 | 1,778 | 312.92 | 17.6 |
| | C.M. | 64 | 1,791 | 318.79 | 17.8 |
| S | F.O. | 13 | 424 | 61.48 | 14.5 |
| | C.M. | 91 | 634 | - 32.96 | - 5.2 |
| T | F.O. | 59 | 672 | 30.91 | 4.6 |
| | C.M. | 84 | 660 | 3.30 | .5 |
| U | F.O. | 41 | 1,767 | 321.59 | 18.2 |
| | C.M. | 23 | 1,571 | 285.92 | 18.2 |
| V | F.O. | 91 | 1,240 | 208.32 | 16.8 |
| | C.M. | 29 | 1,174 | 93.92 | 8.0 |

the C.M. who presided over the July 4th weekend is eliminated, the F.O.'s increased sales 19.1 per cent.)

(c) The sales-volume changes were slightly in favor of F.O.'s, but the profit patterns were more striking. The profit margins (profit ÷ sales) for F.O.'s averaged 9.5 per cent; the profit margins for C.M.'s averaged 1.8 per cent. The average weekly profit for all the restaurants during C.M. supervision was only \$56.81; under F.O.'s the profit was \$271.83, or 478 per cent greater. Only two of the 29 observations where F.O.'s were in charge showed losses; in contrast 11 of the 24 cases where C.M.'s were running the restaurants showed loss operations. These data were consistent with the beliefs of the parent company executives who said, "We don't think there is much change in sales when a restaurant is operated by a franchisee-owner instead of a company manager, but we do think profits go up. This is because franchisee-owners just watch the little things closer; they utilize the cooks and waitresses better; they reduce waste."

The evidence seems too persuasive to ignore. Despite detailed supervision which would seem to minimize opportunities for managerial initiative, restaurants operated by independent franchisee-owners outperformed those supervised by company managers, even though the company managers are paid on a basis that involves some incentive compensation for achieving profits. (The company managers can earn a bonus for profitable operation that could go as high as 33 per cent of their salary, and typically averages 15 per cent.) The impact of motivation, arising from circumstances where the franchisee-owner has invested his capital in the business and receives as income only profits, is revealed in data that strongly support the importance of X-efficiency.

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Some Estimates of the Cost of Capital to the Electric Utility Industry, 1954-57: Comment

A recent paper by Merton H. Miller and Franco Modigliani [4] (hereafter MM) represents an interesting empirical extension of their earlier theoretical work, which stipulated the invariance of the cost of capital to the capital structure of a business firm, abstracting from the effects of the corporate income tax. Their previously developed theory is used to set up an econometric model from which are derived estimates of the cost of capital to the electric utility industry for the years 1954, 1956, and 1957.

We have a number of reservations as to the practical relevance of the theoretical structure from which the invariance thesis is derived; and we suggest that previous empirical testing of this thesis fails to justify its utilization as a basis for measuring the cost of capital—particularly when the cost of capital can be estimated without prejudice as to the relationship between the cost of capital and the capital structure.¹ In this Com-

¹ The most comprehensive studies known to us of the effect of capital structure on the cost of capital are those by Alexander Barges [2], William White [6], William Beranek [3], and Fred David Arditti [1]. In the first of these studies, Barges points out the serious deficiencies in the

ment, we shall indicate further deficiencies in the specific econometric model used by MM which suggest that their estimates are subject to downward bias, and we shall present an alternative approach and alternative estimates of the cost of capital. However, we should like to stress that we do not regard our results as definitive but merely as indicative of a more useful methodology, and that any estimation of the cost of capital is subject to a substantial margin of error in view of its dependence on the measurement of investor expectations.

The statistical implementation of their basic theoretical model is carried out by MM with a considerable degree of sophistication and ingenuity. However, there are substantial shortcomings in the statistical model, of which the authors do not appear to be aware. Since these generally would be expected to lead to an overstatement of the earnings multiplier and thus to an understatement of the cost of capital, there may be a significant downward bias in the estimates of the latter.

First, we take exception to the manner in which size is introduced as a possible determinant of the value of the firm. If enterprise risk (and therefore the capitalization rate, p , for a pure equity earnings stream) is independent of size, then in the no-growth case we expect value (apart from that portion due to the tax advantage of debt), $V - \tau D$, to be proportional to normalized earnings, \bar{X} , as shown by line OC in Figure 1a.² However, if p declines as size increases, through some diversification effect, and if size is not explicitly taken into account, then because of the high correlation between size and dollar earnings we will have value rising more than proportionately with earnings as in the curved line in Figure 1b. If the model enforces linearity on the value-earnings relationship and if observations fall in the range \bar{X}_1 to \bar{X}_2 , the regression line obtained will be something like AB . MM are quite correct in pointing out that a negative intercept can be interpreted as evidence of a favorable effect of size on risk class. However, the slope of AB now overstates the true effect of earnings on value, given size, which is still represented by OC for firms of average size. To avoid this overstatement, a size variable must be explicitly included.

In the relations actually fitted, MM deflate all variables by total assets in order to reduce heteroskedasticity, so that the inclusion of a constant

earlier empirical work referred to by MM. Of the four, only Beranek obtains results interpreted as favorable to the MM theory and the test here is based on the incorrect assumption that this theory implies an ultimate downturn in the equity yield curve (as a function of leverage) whereas such a downturn is inconsistent with traditional theory. (On this point see Alexander A. Robichek and Stewart C. Myers [5, pp. 34-36].) The Arditti study is in certain respects the most satisfactory of the four referred to. It covers a large continuous cross-section of firms in many different industries over the 1946-63 period, is more multivariate than the earlier studies—including a dividend effect and numerous risk variables as well as the debt-equity ratio—and as compared with the Beranek study more plausibly interprets the MM hypothesis as implying simply a positive slope of the equity yield curve as a function of leverage even after adjustment for corporate taxes. The regression coefficient of the debt-equity ratio is generally negative (frequently significantly so) and never significantly positive. Similar results were obtained by White, who uses a book rather than market debt-equity ratio to measure leverage.

² Here V is the total value of the firm, D is the face value of debt and τ is the corporate profits tax rate.

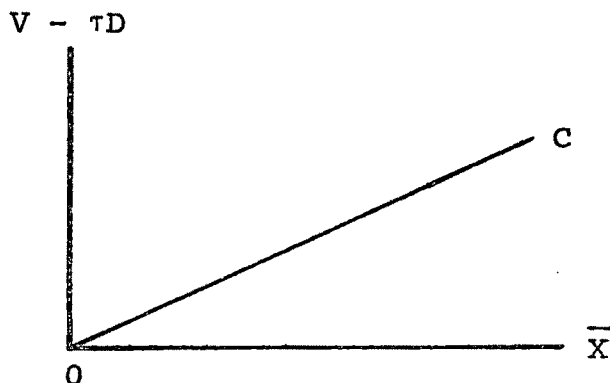


FIGURE 1a

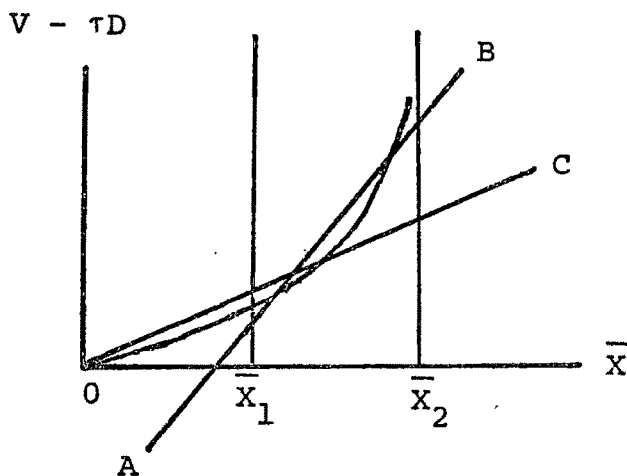


FIGURE 1b

term is equivalent to the introduction of total assets into the undeflated equations. Thus the suppression of the constant term in the lower panel of their Table 1 may lead to an overstatement of the earnings multipliers by assigning to earnings any favorable effects of size on risk, while the relatively low earnings multipliers in the top panel are free of this error. The constant terms in the top panel are significant in two of the three years, suggesting that p does indeed decline with size.³

Secondly, the replacement of actual earnings with a linear function of dividends and other variables may lead to upward bias in the earnings coefficient. There is considerable appeal in the argument that the theoretic-

³ The significantly negative coefficients of $1/A$, the counterparts of the constant terms in the undeflated equations, suggest that the effect of size is nonlinear, as we might expect on the supposition that it is systematically related to p and should therefore enter multiplicatively rather than additively into the value-earnings relationship.

cally relevant earnings variable of the MM model, the expected average future earnings on existing assets, may be better approximated by the level of disbursements to which management has committed itself (with varying degrees of firmness) for debt service and preferred and common dividends than it is by the level of current income. However, these disbursements may have independent effects on value and may contain other information in addition to that on normalized earnings. To the extent that high payout is preferred (or disliked) by the kind of investors who buy utility stocks, dividends may have an independent effect on value, for given normalized earnings. Furthermore, to the extent that enterprise risk varies within the industry, whether because of size differences or for any other reason, we may expect that the disbursements to which management commits itself will be higher relative to normalized earnings for the lower risk firms. Thus the level of disbursements contains information about enterprise risk as well as normalized earnings.⁴ It is not clear how the positive correlation of dividends with payout would affect the estimated cost of capital,⁵ but the negative correlation of dividends plus contractual disbursements with risk (given normalized earnings) would be expected to bias the normalized earnings coefficient upward and hence the cost of capital downward.⁶

MM attempt in their Table 5 to test the strength of any independent effect of payout on valuation by adding a linear dividend term (the deviation of actual dividends from those expected on the basis of an average payout ratio) to the value regression. This is not really a satisfactory test since neither the effect of payout per se nor that of dividends as a proxy for enterprise risk is likely to be linear or even monotonic; and it is not clear what implications the nonsignificance of the dividend coefficient might have as to bias in the two-stage estimate of the earnings multiplier. When current earnings are used as the earnings variable, the effect of dividends is always found to be positive and is significant in one of the three years, suggesting that payout does indeed contribute to the explanation of value, given earnings. However, it is not certain whether its contribution arises from the information it supplies as to normalized earnings

⁴ It should be noted that dividends are by far the most important ingredient in the linear function used to approximate normalized earnings, so that the two-stage value regressions are almost, but not quite, equivalent to substituting dividends for current earnings as a determinant of value. The regressions of earnings on the instrumental variables, excluding dividends, show very small correlations, except in 1957 when a significantly negative debt coefficient makes some contribution. However, the theoretical expectation is for a positive effect here.

⁵ If utility investors in the mid-1950s preferred dividends to retained earnings, the estimated cost of capital would be biased downward; if they preferred retained earnings, the bias would be reversed.

⁶ It is of some interest that the constant term becomes insignificant in the value regressions when computed earnings based on the instrumental variables are substituted for actual earnings. MM interpret this as indicating that they were right in suppressing this term, thus implicitly excluding assets from the undeflated relationship. Our interpretation is that the instrumental variables representing the level of disbursements to which management is committed contain most of the information as to cross-sectional differences in enterprise risk which a size variable, entering linearly, has to offer (as well as information on normalized earnings), so that when these are introduced the effect of assets in the undeflated relationship is no longer significant.

or from its ability to proxy for enterprise risk or from a payout effect per se. If only the first is involved, then the use of a computed earnings variable based on dividends will not lead to an overstatement of the effect of normalized earnings. If either of the last two is involved, then bias is to be expected in the two-stage estimate of the earnings multiplier.

When the earnings variable used in the test is a computed value based on the instrumental variables, the dividend effect becomes insignificantly negative. While this suggests that the payout effect per se either is not large or (as we believe) is nonlinear, it is not convincing evidence against the proposition that the two-stage earnings variable may proxy for enterprise risk, since the instrumental variables, which include disbursements for interest and preferred dividends as well as common dividends, clearly contain all the information as to both normalized earnings and risk which the dividend deviation could possibly supply, and considerably more in addition. It is the total disbursements for interest and preferred and common dividends which we expect to be high relative to normalized earnings when enterprise risk is low; and in fact the disbursements for the first two may be expected to rise relative to the third as risk falls, if as we suspect financial leverage tends to be inversely correlated with enterprise risk.

Finally, and most important, we have strong reservations about the growth variable used. While we are not entirely satisfied with MM's theoretical formulation of the growth term, we agree that it must depend on the three components they distinguish—the rate at which assets grow, the extent to which average return on new investment exceeds the cost of capital, and the number of years for which investment opportunities at rates substantially above the cost of capital are expected to be available. The variable actually used in their regressions reflects only the first of these. However, if there are significant cross-sectional differences in the average rate of return available on new investment, it seems entirely likely that this rate will be substantially correlated with the rate of return on existing assets. But this is precisely the earnings variable used in the deflated regressions. We expect therefore that the coefficient of this variable reflects not only the multiplier for expected earnings on existing assets but also some part of the second component of the growth effect, that which is related to the average rate of return on new investment. As a result, the normalized earnings coefficient is again biased upward and the cost of capital downward.

In summary we feel that the coefficients of earnings in the lower panel of MM's Table 3 are too high (and hence the cost of capital estimates too low) because they reflect not only (1) the effect on enterprise value of expected average future earnings from existing assets, but also (2) the informational content as to enterprise risk provided by assets, dividends and interest charges and (3) the effect on value of that component of the growth term which is related to return on new investment. The increase in earnings multipliers from the values of 13.7 to 15.7 appearing in the upper panel of Table 1 to the values of 16.1 to 19.7 appearing in the lower panel of Table 3 is attributable in part to the second of these considerations and only in

part to the informational content of the instrumental variables as to normalized earnings. Both sets of estimates are presumably biased upward by the inadequacies of the growth variable used and are subject to the basic limitation of this empirical approach which makes the unnecessary and potentially dangerous invariance assumption.⁷

Direct Estimates of the Cost of Capital

A preferable approach to estimating the cost of capital is to derive separately the required rate of return on common (after corporate taxes), i_e , the required rate of return on preferred (again after taxes), i_p , the required rate of return on bonds, i_b , and to obtain a before-tax overall cost of capital (prior to adjustment for underpricing) $i' = W_e(i_e/(1-t)) + W_p(i_p/(1-t)) + W_b i_b$, where i' indicates the before-tax basis, t is the corporate tax rate and W_e , W_p , and W_b are the ratios of the market value of common, preferred, and bonds to the total value of securities. This approach abstracts from the problem of measuring the effect of capital structure on the valuation of a firm (a problem to which we have no satisfactory answer). It simply assumes that the company has a target debt-equity ratio which is close to the actual ratio and that small or temporary deviations from this ratio do not significantly affect the overall cost of capital. There is of course no real difficulty in measuring i_b , i_p , W_b , W_p , and W_e , so that the only important problem is to estimate i_e .

Now i_e can be estimated through an indirect regression approach similar to that used by MM in estimating the overall cost of capital, but to avoid the serious difficulties of such an approach, we can compute i_e much more directly if we are willing to make a few simplifying assumptions. If the investor is assumed to expect a constant growth rate (g) in annual earnings (E) during the time period (n years) the investor plans to hold the stock, and the dividend payout ratio (d) is also expected to remain constant, then the after-tax return on equity which is required by the marginal stockholder at a given time is implicit in the existing market price per share, and may be deduced from the following equation relating this price P_0 to the discounted present value of dividends and capital return:

$$(1) \quad P_0 = \sum_{t=1}^n \frac{(1+g)^t d E_0}{(1+i_e)^t} + \frac{P_n}{E_n} \cdot \frac{E_0(1+g)^n}{(1+i_e)^n}$$

since $E_n = E_0(1+g)^n$.

If we make one further assumption, that for most companies for most periods of time investors expect the price-earnings multiple at the end of the period when they liquidate their investment will be the same as when

⁷ There is no presumption as to the direction or magnitude of error which might result if the invariance assumption is in fact incorrect. MM's Table 4 suggests, at least when the earnings variable is computed from the instrumental variables, that no large error is involved; but this test is not really convincing, since leverage is introduced linearly into the valuation regression. Leverage, if it affects value, certainly cannot be expected to do so linearly. The traditional theory would lead us to expect that, for given normalized earnings, increases in debt would initially increase but eventually decrease value.

they invest, the above equation simplifies to

$$(2) \quad i_e = g + \frac{E_0}{P_0} (1 + g)d.$$

The $(1+g)$ factor in the last term, which would disappear if dividends were paid and discounted continuously at the annual rates dE_t and i_e , can be omitted for convenience resulting in a very slight understatement of the cost of capital if as is usual dividends are paid quarterly whereas g is computed on an annual basis. For convenience dE_0 will be written as D_0 or the initial dividend rate per share. The required rate of return on equity can now be written as

$$(2') \quad i_e = \frac{D_0}{P_0} + g$$

or the sum of the initial dividend yield and the expected growth rate. The first of these is directly observable, while the second must be inferred from historical growth rates or in some other way.

The only significant assumption involved in this derivation which might be questioned is whether investors normally consider that $P_n/E_n = P_0/E_0$. For the market as a whole or for any very broad industry group, this assumption seems reasonable, except perhaps at cyclical extremes. It is equivalent to assuming (though investors may or may not think of it in this way) that the growth rate anticipated for the n year holding period is expected to continue indefinitely. If this assumption is regarded as unrealistic, it is simple enough to revert to equation (1) and allow for differences in growth rates, reflecting say rates of return on investment significantly above the cost of capital for a stipulated period, n years, and a smaller growth rate thereafter. MM [4, p. 385] obtain an estimated n for the electric utilities of 1 year in 1954, 6–12 years in 1956 and 11–51 years in 1957, which seems like an unreasonably large range. If a well-known 20-year forecast of a 5 per cent annual growth in earnings per share for the electric utilities made in the early 1960s by Charles Tatham, with an initial P/E ratio of 18 and dividend payout of somewhat below 70 per cent, is taken as more indicative of investor expectations (at least for that period), then we obtain an estimate for i_e of 8.8 per cent if the growth rate of 5 per cent is expected to continue indefinitely (or if $P/E=18$ at the end of 20 years), and this estimate is reduced only moderately to slightly over 8 per cent if the growth rate is expected to revert to 3 per cent at the end of the 20-year period.

In 1954, 1956, and 1957 the actual average D/P ratios for Moody's 24 Utilities were .048, .047, and .049 respectively, and .049, .047, and .046 if three-year centered averages are used. Average annual compound growth rates in earnings of these stocks for the same years are presumably indicative to some extent of the growth rates anticipated by investors at that time. These were .045, .066, and .054 over the preceding five-year period, and .060, .043, and .047 over the preceding ten years. The corresponding growth

rates in dividends which might be regarded as smoothed earnings rates were .051, .043, and .049 over five years, and .050, .050, and .045 over ten years. These past growth rates were quite close to the forecasts made by the investment community for the period ahead.⁸ The use of any of these sets of observed growth rates in equation (2') would imply an i_e averaging in the neighborhood of .10 for the three years without too much variation from year to year, but with the precise figure derived depending on the type of growth rate used. If a growth rate of .04 is substituted for those observed, on the grounds that stock prices do not fully reflect the past or predicted growth rate in earnings or that these growth rates are not regarded as enduring over very long periods, i_e would range from slightly under .09 in 1954 to over .085 in 1957. A growth rate of .03 would imply an i_e ranging from slightly under .08 to over .075. To place these growth rates in some perspective, they might be compared with the 5.9 per cent postwar growth rate in current dollar GNP, the 4.5 per cent growth rate in average annual earnings of the Cowles-Standard and Poor industrial stock indexes since the latter part of the 19th century, and the growth rates of 5.3 per cent in current dollar GNP and 3 per cent in constant dollar GNP since the early part of this century.

As one test of the reasonableness of our method, the required average rates of return on equity which we obtain, ranging from well over $7\frac{1}{2}$ per cent at an assumed $g=.03$ to well over $9\frac{1}{2}$ per cent at an assumed $g=.05$, might be compared with an average E_0/P_0 of .069 for the three-year period. Presumably a simple E_0/P_0 ratio represents a significant understatement of the required equity yield since P_0 must exceed E_0/i_e if future investment with returns above the cost of capital is anticipated. It is not possible to say with certainty how much higher i_e is than E_0/P_0 since this depends on investors' expectations which cannot be measured with any precision. However, an i_e of .075 would seem to be at the lower end of any reasonable range while .095 might be at the upper end.⁹

As a further check on our results, we may compare them with Arditti's indirect estimates of the required rate of return for AT&T [1],¹⁰ if we assume this is not heroically different from that for the electric utilities on the hypothesis that the higher leverage of the latter is associated with and may be offset by less industry risk. Adding the change in the capitalization rate

⁸ They were also reasonably close to the actual annual growth rates in earnings and in dividends for each of the five-year periods beginning in 1954, 1956 and 1957, which averaged well over 5 per cent in earnings and somewhat over 4 per cent in dividends for the three periods. The ten-year growth rates for the period ahead were available only for 1954 where they were considerably higher than the corresponding five-year growth rates.

⁹ The few institutional and professional investors we have talked to indicate they require or expect about a 10 per cent rate of return on industrial equities, with a somewhat lower rate on electric and gas utilities. They may of course be deluding themselves but 10 per cent is quite close to (though somewhat below) the average annual rate of return (including capital gains) experienced in the Standard and Poor industrial and composite price indexes for common stock for the 1926-65 period or to the average return obtained by splicing the Standard and Poor indexes to Cowles Commission stock data back to the latter part of the 19th century. A return of 10 per cent is well below the average rate in the postwar period.

¹⁰ No electric utilities are included in this analysis.

for AT&T over this period ($-.0377$) to the realized rate of return (.1611) gives a required rate of return of .123 [1, p. 134], which of course is well above the range presented here. Another indirect estimate of the required rate of return for AT&T can be obtained from Arditti's analysis relating the required rate of return for a large cross-section of equities to a number of measures of risk, including most notably the variance and skewness of the distribution of annual returns. This analysis implies a required rate of return of .085 or somewhat above, which is about the midpoint of our range [1, pp. 98 and 124]. It is interesting to note that AT&T stock (and presumably also stock of the electric utilities) has somewhat below average variance which lowers the required rate of return but also less positive skewness which raises the required rate of return.

A final check on the reasonableness of our results may be obtained by examining their implications as to the return on book expected by investors. The 4 per cent growth rate of earnings subsumed in the 8.5 per cent figure which is the midpoint of our range reflects not only growth arising from retained earnings but also the growth (per old share) arising from external financing. Much of the latter reflects the issuance of new shares at prices substantially higher than book, thus raising the rate base per share. If only 1 per cent of the 4 per cent total represents growth from such external sources, which we suspect represents an understatement, an 8.5 per cent required yield on equity would be associated with about a 10.3 per cent rate of return expected on book equity (in view of an average payout ratio of 71 per cent for the three-year period according to Moody's 24 Utilities). Similarly, a 7.5 per cent required yield on equity would be associated with about a 6.9 per cent return expected on book equity, a result which clearly seems too low. The actual yield on book equity in this period was 9.4 per cent for the Moody's 24 Utilities (and 10.6 per cent for the FPC Class A and B Utilities which had a 73 per cent payout).

In order to arrive at an overall cost of capital, comparable with the MM figures, there is still the problem of obtaining the market weights for bonds, preferred and common. Though we do not have such data directly, we can use MM's sample book ratios of 51 per cent for bonds, 11 per cent for preferred and 38 per cent for common, and then convert these ratios to a market base by adjusting them for the ratio of market to book value of common equity, which averaged 1.59 for their sample of electric utilities in the three-year period. The resulting market weights are estimated at 42 per cent for bonds, 9 per cent for preferred and 49 per cent for common. Applying these weights to required yields of .038 for bonds (the average yield on AAA public utility bonds in that period), .045 for preferred,¹¹ and a rough estimate of .085 for common gives a before-tax overall cost of capital (prior to adjustment for underpricing) of .107.¹² This may be con-

¹¹ Since we have not been able to find a preferred stock yield index for utilities, we have used the embodied cost figures given by MM which presumably represents a small understatement of the relevant current yield. The use of a AAA bond yield presumably also results in some understatement of the relevant bond yield.

¹² The after-tax yields on common and preferred have been divided by the complement of an assumed marginal corporate tax rate of .5.

sidered the marginal before-tax overall cost of financing if the same proportions of common, preferred and bonds are used in the new financing as in the existing capital structure.

This estimated before-tax cost of financing of .107 may be compared with a figure of .083 implicit in the MM results. Even if a relatively low i_d of .075 is assumed (corresponding to a .03 growth rate), our before-tax cost of capital is still somewhat over .097. This estimate might be further reduced to .093 if an embodied past rather than a marginal current cost of senior capital is used, but this does not seem to be consistent with any useful economic concept of the cost of capital. Our admittedly crude empirical results therefore again suggest a significant understatement in MM's estimate of the cost of capital, confirming our earlier conclusions drawn from an investigation of their econometric model.

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Some Estimates of the Cost of Capital to the Electric Utility Industry, 1954-57: Comment

Franco Modigliani and Merton H. Miller have been among the foremost contributors during the last decade to progress in the theory of cost of capital, security valuation, and corporation finance. The position they have earned in the field gives considerable authority to the empirical research they presented in a recent issue of this *Review* [12] on alternative theories in the area. Furthermore, the authority status of their conclusions is enhanced by the magnitude of their effort and by the categorical manner in which they stated many of their conclusions. It is therefore desirable that their findings be subject to a critical review, and since the work in which I have been involved has led to different conclusions, I felt drawn to the task.

In my analysis of their recent paper I find that the models MM employed are at best inadequate and in crucial instances are incorrect as instruments for testing their theorems. *Therefore, contrary to the conclusions they reached, MM's findings provide no empirical support for their theorems on security valuation, cost of capital, and corporation finance.*

My comments here are not concerned with arguments for or against the assumptions on which MM based their theorems; there is already a considerable literature on that subject. My concern is only with the validity of the models they employed as instruments for testing their theory, and I will describe their theory only insofar as this objective requires it.

I. The MM Model

In their classic 1958 paper [13], MM demonstrated that under certain assumptions the value of a corporation is independent of its leverage and dividend rates. That is, with S =value of its stock, D =value of its debt, V =value of its outstanding securities, \bar{X} =expected earnings before interest on its assets, and ρ =capitalization rate,

$$(1) \quad S + D \equiv V = \bar{X}/\rho.$$

The capitalization rate is a function solely of the corporation's nonfinancial risk, and for each corporation in a risk class, V will be the same multiple, $1/\rho$, of \bar{X} regardless of how the corporation is financed. The assumptions on which this theorem rests are: (1) investors are indifferent between leverage on personal and on corporate account; (2) there is no corporate income tax; (3) stockholders are indifferent as to how a corporation allocates its income between dividends and retention; and (4) corporations do not have investment opportunities with a rate of return greater than ρ .

In a subsequent note [14] that corrected the treatment of the corporate income tax in the above paper, MM showed that with corporate income taxes at the rate of τ , and with ρ on an after tax basis, equation (1) becomes

$$(2) \quad S + D \equiv V = \frac{(1 - \tau)\bar{X}}{\rho} + \tau D$$

\bar{X} now equals expected earnings before interest and taxes. Given \bar{X} , V increases with the leverage rate because interest is a tax exempt expense and because the government is certain to bear the fraction τ of any losses.

To test this theory, MM employed in [12] the following model¹ (and it is this model which is the subject of my concern):

$$(3) \quad \frac{V - \tau D}{A} = a_0 + a_1 \frac{(1 - \tau)\bar{X}}{A} + a_2 \frac{1}{A} + a_3 \frac{\Delta A}{A} + u.$$

MM explained the relation between equations (2) and (3) as follows. All

¹ See [12, eq. (14) p. 350]. The estimating equations used $\bar{X}^T - \tau R$ in place of $(1 - \tau)\bar{X}$, but as they explained [12, p. 356], $\bar{X}^T - \tau R$ is a convenient empirical approximation of $(1 - \tau)\bar{X}$.

the variables are divided by A , the book value of total assets, to avoid (in a cross section sample) correlation due to scale.² τD is transferred to the left side for convenience; this is permissible because its coefficient is equal to one under the theory. Also under the theory, the expected value of a_0 is zero and a_1 is an estimate of $1/\rho$. The variable $1/A$ is included because size may influence the value of a corporation, and $\overline{\Delta A}/A$ is included to allow withdrawal of the fourth assumption listed above, namely, corporations do not have investment opportunities with a rate of return greater than ρ .

MM were particularly interested in a_1 since $1/a_1$ is an estimate of ρ , which in their theory is a corporation's cost of equity capital. Its cost of debt capital is $(1-\tau)\rho$, and, if the corporation constrains its financing with some debt-asset ratio, q , its cost of capital is $\rho(1-\tau q)$. See [12, p. 342].

II. Objectives of the MM Paper

One objective of the MM paper was to demonstrate the advantage of two-stage least squares (2SLS) over direct least squares (DLS) as a means for estimating the parameters of equation (3). Using DLS, a_0 proved to be significantly positive, as shown in the first panel of Table 1. However, in theory a_0 should be zero, and MM reasoned that a possible explanation for its not being zero is the presence of errors in the observed values of $(1-\tau)\overline{X}$ as measurements of true expected earnings. In the event of such errors, a_0 would be biased upward and a_1 would be biased downward. One means of dealing with the problem, assuming the explanation is correct, is to constrain $a_0=0$, and the resultant parameter estimates appear in the second panel of Table 1.

However, the best way to eliminate bias in the parameter estimates is to eliminate errors in the measurement of $(1-\tau)\overline{X}$ and MM employed 2SLS to do this. With P =preferred stock and DIV =common dividends, they first ran the regression

$$(4) \quad \frac{(1-\tau)\overline{X}}{A} = b_0 + b_1 \frac{1}{A} + b_2 \frac{\overline{\Delta A}}{A} + b_3 \frac{D}{A} + b_4 \frac{P}{A} + b_5 \frac{DIV}{A} + w,$$

and then they used the values of $(1-\tau)\overline{X}/A$ provided by the regression as error-free measurements of that variable in equation (3). The resultant parameter estimates for equation (3) appear in the third and fourth panels of Table 1.

As the third panel shows, the 2SLS unconstrained estimates of a_0 are not significantly different than zero. From this MM inferred that $(1-\tau)\overline{X}$ is subject to measurement error as true expected earnings and that 2SLS improves the parameter estimates by reducing this error. My interpretation of the data presented in panel three is different: the net effect of the 2SLS

² In a curious slip MM stated that the alternative of deflating by $V-\tau D$ "which was the approach we adopted in our first paper and which was subsequently followed by Barges . . . is somewhat unesthetic . . . and will lead to biased estimates" [12, p. 349]. They were not followed by Barges [1]. The major if not sole thesis of his book was that using V or $V-\tau D$ as a deflator leads to biased estimates. He showed that the evidence MM found in support of the earlier formulation of their theory disappeared when A was substituted as the deflator.

TABLE 1—ALTERNATIVE REGRESSION ESTIMATES OF THE COEFFICIENTS AND THEIR STANDARD ERRORS OBTAINED BY MM WITH $(V-\tau D)/A$ THE DEPENDENT VARIABLE^a

| Year | Constant Term | Earnings $(1-\tau)\bar{X}/A$ | Size $1/A-10^7$ | Growth $\Delta A/A$ |
|--|---------------|---------------------------------|--------------------|------------------------|
| Panel 1—Direct Least Squares | | | | |
| 1957 | .16 | 15.7 | -.28 | 1.37 |
| | .06 | 1.2 | .08 | .24 |
| 1956 | .06 | 15.6 | -.12 | .85 |
| | .06 | 1.2 | .08 | .23 |
| 1954 | .27 | 13.7 | -.19 | .31 |
| | .06 | 1.3 | .06 | .15 |
| Panel 2—Direct Least Squares ^b | | | | |
| 1957 | | 16.0 | -.28 | 1.39 |
| | | .44 | .08 | .23 |
| 1956 | | 16.6 | -.11 | .93 |
| | | .39 | .07 | .21 |
| 1954 | | 19.2 | -.21 | .47 |
| | | .43 | .07 | .17 |
| Panel 3—Two Stage Least Squares | | | | |
| 1957 | .00 | 16.2 | -.28 | 1.37 |
| | .08 | 1.7 | .08 | .24 |
| 1956 | .05 | 15.6 | -.12 | .85 |
| | .08 | 1.6 | .07 | .23 |
| 1954 | .07 | 18.1 | -.23 | .29 |
| | .10 | 2.2 | .07 | .17 |
| Panel 4—Two Stage Least Squares ^b | | | | |
| 1957 | | 16.1 | -.28 | 1.36 |
| | | .46 | .08 | .23 |
| 1956 | | 16.7 | -.11 | .90 |
| | | .40 | .07 | .21 |
| 1954 | | 19.7 | -.24 | .30 |
| | | .45 | .07 | .18 |

^a Source: Tables 1 and 3 in MM [12].^b Constant Term Constrained at zero.

operations was to substitute a *multiple* of the dividend for $(1-\tau)\bar{X}$, and the changes in a_0 and a_1 are due to this substitution. The basis for this interpretation will be discussed later.

Another objective of the MM paper was to test their hypotheses that the value of a firm is independent, apart from the tax effect, of its leverage rate and that its value is also independent of its dividend payout rate. *Equation (3) may be used to estimate ρ and the cost of capital if these hypotheses are true, but it does not test the hypotheses because the parameter estimates are consistent with conflicting hypotheses on leverage and dividend policy.*³ Therefore, to test their hypotheses MM introduced additional variables, and we turn to their findings on these questions next.

³ Under the MM theory a_1 is an estimate of ρ , which enters into the determination of a corporation's cost of capital. However, it follows from every alternative theory that $V-\tau D$ is positively correlated with $(1-\tau)\bar{X}$, size, and growth. Also the theories do not disagree on the numerical values of the parameters in this model.

The final objective of their paper was to develop estimates of the cost of capital on the basis of their model, and these estimates are discussed in the last section of this comment.

III. *The Leverage Theorem*

Under the reasoning that led to equation (3) the subtraction of τD from V completely accounts for the influence of leverage on the value of a corporation. Accordingly, with debt and preferred stock added to the equation's independent variables, their coefficients should not differ significantly from zero.⁴ This is what MM found with their electric utility sample. However, using a utility sample actually gave results that were against their leverage theorem, since regulatory agencies treat the income tax as an expense in determining the income a utility is allowed to earn. Only if their sample had consisted of nonregulated industrial companies would their results have been evidence in support of their theorem.⁵

To see this point, recall [14, p. 435] that the proof of MM's leverage theorem under a corporate income tax began with the statement that

$$(5) \quad X^r = (1 - \tau)(X - R) + R = (1 - \tau)X + \tau R,$$

with X = earnings before interest and taxes, $R = iD$ = interest on outstanding debt, and X^r = earnings after taxes and before interest. Their use of this expression to prove that equation (2) and not equation (1) holds in the presence of a corporate income tax presumes that X is an exogenous random variable, independent of the corporation's tax and leverage rates. As a result, since X is independent of τ and R , and since R increases with leverage X^r increases with R .

In contrast, for a regulated utility X^r is the exogenous random variable. The regulatory agency sets the rates charged consumers in order to provide the utility with an earnings after taxes and before interest rate of return that the agency considers correct.⁶ Consequently, the correct expression for \bar{X}^r in the case of a public utility should be

$$(6) \quad \bar{X}^r \equiv \bar{x}^r A,$$

where \bar{x}^r is the expected after tax and before interest rate of return the utility is allowed to earn. It follows then that a change in a utility's tax

⁴ There is one slight qualification to this conclusion. Under the traditional view (see Barges [1]) V rises with leverage up to a leverage rate considered prudent and falls sharply when the leverage rate passes this level. Hence, it would be necessary to show that few if any corporations in the sample are not prudent.

⁵ MM recognized this fact [12, fn. 12, pp. 342-43], but its implications for the model they should have used to test their leverage theorem with a utility sample went unrecognized. I have recently become involved in a public utility rate case, and in all the testimony on this and other cases I have read, the rate of return in question is earnings with taxes excluded and interest included divided by total assets. See also Bonbright [3, pp. 242-43 and 404].

⁶ The switching or arbitrage operations by which MM proved their leverage theorem assumes two firms identical in every respect but their leverage rates. In particular two firms in the same risk class have the same \bar{X} and different leverage rates. However, two otherwise identical utilities with different leverage rates must have different values for \bar{X} . Their values for \bar{X}^r will be the same, and the MM switching analysis applies to \bar{X}^r .

bill, due either to a change in the tax rate or to a change in the utility's leverage rate, may be expected to cause the agency to change consumer rates and \bar{X} so as to leave \bar{X}^r unchanged. In other words, with \bar{X}^r and not \bar{X} independent of the tax rate and financial policy, the expected pre-tax earnings on a utility's total assets is

$$(7) \quad \bar{X} = \frac{\bar{X}^r - \tau R}{1 - \tau}.$$

A change in τ or R , the interest bill, changes \bar{X} and not \bar{X}^r .

Note that with \bar{X}^r independent of a utility's tax bill, it is also independent of its leverage rate, and \bar{X}^r has exactly the same properties as \bar{X} in the absence of a corporate income tax. Consequently, the deductive argument by which MM proved their leverage theorem leads not to equation (2) but to equation (1), with \bar{X}^r substituted for \bar{X} and with ρ on an after tax basis; it follows that the equation MM should have employed to test their theorem with a public utility sample is not equation (3) but

$$(8) \quad \frac{V}{A} = a_0 + a_1 \frac{\bar{X}^r}{A} + a_2 \frac{1}{A} + a_3 \frac{\Delta A}{A} + u,$$

in which V replaces $V - \tau D$ and \bar{X}^r replaces $(1 - \tau)\bar{X}$. Since the coefficients of the debt and preferred stock variables were not statistically significant when added to equation (3), it is very likely that they would be positive and significant when added to equation (8). A plausible explanation for this is that investors, in contradiction to the MM theorem, prefer leverage on corporate account.

IV. *The Dividend Rate Theorem*

The MM position on dividend policy is that "Under ideal conditions of perfect capital markets, rational investor behavior, and no tax discrimination . . . a firm's . . . dividend policy will have no effect whatsoever on the current market value of its shares or on its cost of capital; and that despite the impressions of some writers to the contrary . . . this conclusion is equally valid whether one is considering a world of certainty or of uncertainty" [12, p. 345]. This is not the place to review the reasoning whereby numerous writers, including myself, have reached different conclusions on the subject. On that, see [6] [7] [8] [9] [10].

Assumptions that are different but appear no less plausible than MM's have led me to conclude that the rate of return investors require on a share and the corporation's cost of capital both increase with the fraction of income the corporation retains. Strictly speaking, this differs from the traditional view that high dividend payouts increase the value of a firm's shares. However, when due allowance is made for the influence of other variables on the value of a corporation's stocks, variables such as leverage and the profitability of its investment opportunities, the traditional view becomes consistent with my conclusion.

To test the traditional view MM added a dividend payout variable to

equation (3). With $DIV/(1-\tau)\bar{X}$ defined as a corporation's dividend rate and with λ the sample average of $DIV/(1-\tau)\bar{X}$, MM added the variable $DIV/A - \lambda(1-\tau)\bar{X}/A$. Ignoring the deflation by A , this variable is simply the difference between the dividend and what the dividend would have been if the dividend rate, which is based on tax-adjusted total earnings rather than on common earnings, had been the same as λ , the sample average. According to MM, their dividend policy theorem is confirmed if the coefficient of $DIV/A - \lambda(1-\tau)\bar{X}/A$ does not differ significantly from zero.

MM's parameter estimates for this dividend variable, obtained both by direct least squares (DLS) and by two-stage least squares (2SLS), and with the constant term constrained at zero, where:

| <i>Year</i> | 1957 | 1956 | 1954 |
|-------------------|------------|-------------|------------|
| DLS Value (S.E.) | .92 (2.5) | .65 (2.3) | 6.4 (2.1) |
| 2SLS Value (S.E.) | -.10 (4.3) | -1.80 (4.0) | -2.2 (3.4) |

In commenting on the DLS data, MM stated that "Findings of this kind (which are quite typical of past valuation studies) have been the main empirical support of the traditional view . . ." [12, p. 368]. Their comment surprises me, since two of their three DLS coefficients are not significantly different than zero and since past valuation studies have actually provided rather strong evidence in support of the contrary hypothesis, i.e., the dividend rate has an influence on the value of a firm's shares.⁷

In view of these other studies, I wondered why their DLS dividend coefficients were so low, and my conclusion, set forth below, was that their model is poorly designed to test their dividend rate hypothesis. In my judgment, the following model provides a straightforward test and makes clear the limitations of equation (3) for the purpose:⁸

$$(9) \quad S = \alpha_0 Y^{a_1} (1 + h)^{a_2} (1 + e)^{a_3} (1 + DIV/Y)^{a_4}.$$

S and Y are, respectively, market value of and earnings on the common stock, h is the debt equity ratio, and e is the expected rate of growth in earnings due to investment opportunities that have a rate of return in excess of the cost of capital. To avoid correlation due to scale, S , Y and DIV could be taken on a per share basis or deflated in some other way. The interpretation of equation (9) is quite simple. The expected value of $a_1 = 1$, and when h , e and DIV/Y are equal to zero, S is the multiple α_0 of Y . a_2 should be negative and a_3 should be positive. Under the MM hypothesis on the dividend rate, a_4 should not differ significantly from zero.

A comparison of equations (9) and (3), with $DIV/A - \lambda(1-\tau)\bar{X}/A$ added to the latter, reveals two major limitations of equation (3) for testing the dividend rate hypothesis. First, since the hypothesis refers to the value

⁷ Empirical studies by Benishay [2], Durand [5], and Gordon [6, Chs. 11 and 12] are among those which found that the dividend rate influences the market value of a firm's stock.

⁸ This equation still leaves something to be desired for establishing whether or not the rate of return investors require on a share and the cost of capital are independent of the dividend rate. For a model designed to test these theorems and the results obtained, see Brigham and Gordon [4].

of a firm's shares and not of all securities, the dependent variable should be S , not $V - \tau D$ as it is in equation (3), and therefore Y , not $(1 - \tau)\bar{X}$, should be the earnings variable.

The other major objection to the model MM employed is their definition of the dividend policy variable. Assuming that the dividend policy hypothesis may be tested with $V - \tau D$ and not S the dependent variable, the dividend policy variable should be the relation between the dividend and what the dividend would be if the *average fraction of common earnings* were paid in dividends. However, $\lambda(1 - \tau)\bar{X}/A$ is not the average fraction of common earnings paid in dividends. It approximates the average fraction of earnings on all assets after taxes paid in dividends.⁹ The result is not merely a poor approximation of the relevant dividend policy variable, but a biased approximation that reduces the correlation with the dependent variable. To illustrate this bias, consider two firms with the same values for $(1 - \tau)\bar{X}$ and for DIV/Y but with different leverage rates. The difference in leverage rates will make their values of Y different, and the firm with the higher leverage rate will have the smaller value for DIV . Although both firms actually have the same dividend rate, DIV/Y , the one with the higher debt and smaller Y will have a smaller value for MM's dividend variable.

In general, MM's definition of the dividend rate introduces inverse correlation between their dividend variable and a firm's leverage rate. Since the value of the firm is positively correlated with its leverage, the inverse correlation between leverage and the MM dividend rate contributes to the latter's low correlation with the firm's value.¹⁰

V. True Expected Earnings

Because the coefficients of their dividend rate variable obtained by 2SLS were all negative and not significant, MM interpreted the data as evidence in support of their dividend policy hypothesis and as a further demonstration of the advantage of 2SLS. However, my interpretation of the data is quite different. To avoid error in the reported earnings and to obtain true expected earnings, MM adopted the hypothesis that the latter are a function of the variables in equation (4), and so they used, in the second-stage regression, the computed value of $(1 - \tau)\bar{X}$ from equation (4) in equation (3). However, an inspection of Table 2 in their paper [12, p. 361] reveals that the dominant explanatory variable in equation (4) is the dividend. Hence, in the second-stage regression their error free value of $(1 - \tau)\bar{X}$ differs little from a linear function of the dividend.

Furthermore, two other explanatory variables, D/A and P/A are highly

⁹ A still unsatisfactory but more reasonable dividend rate variable given that $V - \tau D$ is their dependent variable would have been dividends on preferred and common plus interest divided by $(1 - \tau)\bar{X}$.

¹⁰ Working out the following numerical example illustrates the point. Assume two corporations both have $A = \$1,000$, $\bar{X} = \$80$, and $DIV/Y = .8$. However, let $D = \$600$ for corporation I and $\$400$ for II. With $i = .05$ and $\tau = .5$, $(1 - \tau)\bar{X} = \$40$ for both, but $DIV = \$20$ for I and $\$24$ for II. With λ equal to the average of $DIV/(1 - \tau)\bar{X}$ for the two corporations, MM's dividend variable is $-\$2$ for I and $\$2$ for II. Both corporations have the same dividend rate, and I not II has the higher payout of dividends and interest.

correlated with the interest on debt and the preferred dividends. Let TPO =total payments to all security holders, PIV =preferred dividends and R =interest on debt. $TPO = DIV + PIV + R$, and it is easily shown that the MM earnings variable

$$(10) \quad (1 - \tau)\bar{X} = TPO + RET - \tau R$$

where RET =retained earnings. We see that $(1-\tau)\bar{X}$ differs slightly from TPO , and the error free value of $(1-\tau)\bar{X}$ based on equation (4) is practically identical with a linear function of TPO .

As a result, the regression equation MM employed to test the dividend rate hypothesis by means of 2SLS was not, in fact, equation (3) with a dividend variable added. In reality, it was

$$(11) \quad \frac{V - \tau D}{A} = a_1 \left[b_0 + b_1 \frac{TPO}{A} \right] + a_2 \frac{1}{A} + a_3 \frac{\bar{\Delta A}}{A} \\ + a_4 \left[\frac{DIV}{A} - \lambda \left(b_0 + b_1 \frac{TPO}{A} \right) \right]$$

The dividend policy variable, therefore, is not based on a difference between dividends on common and some earnings figure, regardless of how biased the earnings figure is. Instead, the variable is based on the difference between the dividends on common and the "dividends" on all securities. Consequently, the variable has no relevance for the dividend policy hypothesis.¹¹

MM have rejected all evidence in support of the hypothesis that the dividend rate influences the value of a firm's stock on the grounds that the evidence is an illusion, attributable to the dividend's "informational content" with respect to true expected earnings and to the measurement error in actual earnings. Perhaps they are right, but their 2SLS use of the dividend's informational content to measure true expected earnings leads them to use the dividend as the earnings variable. This switching operation would have been perfectly transparent if S and Y had been the value and earnings variables in place of V and $(1-\tau)\bar{X}$. MM may be right, but their 2SLS procedure both cannot prove it and *does not go beyond* the informational content of the dividend in establishing the influence of earnings on share value.

VI. Growth and the Cost of Capital

As demonstrated above, MM failed to prove either (1) that investors are indifferent between leverage on personal and leverage on corporate account, or (2) that they are indifferent to a corporation's dividend rate. Under the alternative theorems, a corporation's cost of capital is not a

¹¹ The reason why a_4 , the coefficient of the so-called dividend variable is very small in relation to its standard error is easy to explain. $V - \tau D$, TPO and DIV are all correlated with each other. However, as one would expect, $V - \tau D$ has a higher correlation with TPO , the "dividends" on all securities, than with DIV , the dividends on common stock. With TPO and DIV highly correlated a_4 will be small in relation to its standard error.

constant but is a function of both the method and the level of financing, and it is a function which is difficult to estimate. It is possible that for some purposes MM's constant cost of capital might serve as a satisfactory approximation—unsatisfactory for a corporation considering a change in its financing policy or in the level of its investment—but satisfactory for representing the average cost of capital for an entire industry, using equation (3) for nonregulated industries and equation (8) for regulated ones.

However, the cost of capital approximations in the MM paper cast doubt on the usefulness of their model even for this limited purpose. Using equation (3), MM found that the average tax-adjusted cost of capital for the electric utility industry was .046, .045 and .039 in 1957, 1956 and 1954. These figures were only slightly higher than AAA bond rates for those years, and it is very hard to believe that the cost of capital for the industry was that low. It is possible, but not likely, that using equation (8) would result in higher and more reasonable averages, but it is more likely that the treatment of growth in the MM model is responsible for the substantial downward bias in their cost of capital approximations.

In deriving equations (1) and (2), MM [13] [14] postulated a corporation for which the expected earnings are the same in every future period. This is a permissible assumption if the corporation is not expected to invest at a rate of return greater than its cost of capital, and if, therefore, its earnings are only expected to grow due to retention. However, it is not a correct assumption if earnings are expected to grow additionally due to investment opportunities with a rate of return above its cost of capital. To differentiate this additional source of growth from that due merely to retention of earnings, it will be called "super growth" in the following discussion.

It is perfectly clear that most corporations—even utilities—have super growth investment opportunities and that the extent to which any corporation has such opportunities materially influences its market value. Consequently, to test the theory represented by equation (2), MM were forced to add a variable to account for super growth, and they defined this variable as follows. If a corporation is expected to make investments in operating assets which have a tax-adjusted rate of return of ρ^* , and ρ^* is greater than its cost of capital, C , this expectation will raise its market value. To represent these growth opportunities, MM suggested the quantity $\Delta AT(\rho^* - C)/C(1 + C)$. In other words, the corporation is expected to invest ΔA per period for T years at a rate of return ρ^* and obtain, thereby, a periodic rent of $\Delta A(\rho^* - C)$. Division by $C/(1 + C)$ converts the periodic rent to its present value. However, measurement problems forced MM to substitute growth in assets, $\Delta A/A$, as a proxy for the above complex of variables that determine supergrowth.

The rationale for using asset growth as a measure of super growth is the fact that a firm's investment rate should be correlated with its opportunities to invest at a rate of return greater than its cost of capital. However, this correlation may be relatively weak, particularly in the case of utilities, where investment is governed in part by the public interest. The weakness of this correlation, combined with the fact that investors may use $(1 - \tau)\bar{X}$

as well as $\overline{\Delta A}/A$ for information on a utility's super growth, causes a downward bias in MM's estimate of the cost of capital in equation (3).

To see this, note that $(1-\tau)\overline{X}/A$ is actually the rate of return a corporation is earning on its existing assets. It is also true that for corporations in general the expected return on additional assets has some correlation with the return on existing assets. For utilities, regulated as they are to earn a predetermined rate of return on assets, the expected rate of return on investment is exactly the same as the return on assets, if the regulation is perfectly efficient and if the predetermined rate is not expected to change. Hence, the correlation of $(1-\tau)\overline{X}/A$ with super growth should compare favorably with that of $\overline{\Delta A}/A$.

With the coefficient of $(1-\tau)\overline{X}/A$ reflecting the influence of super growth as well as current earnings on the value of a firm, its coefficient, a_1 , should overestimate the value of $1/\rho$, and the estimate of ρ should be too low. MM's data bear this out. It is generally accepted that when super growth opportunities are present, the earnings yield, Y/S , is below ρ , the cost of equity capital, i.e., the return investors require on a company's common stock. The striking illustrations are Xerox and IBM. However, in Table 8 of MM's paper we find that, instead their estimates of ρ are below Y/S . In 1954, for example, $Y/S = .066$ and $1/a_1 = .051$.

In conclusion, to go from either equation (1) or (2) to approximations of the average cost of capital for an industry requires a more effective means for isolating the influence of super growth opportunities on a firm's value than that used by MM in equation (3). If this is not possible, some other means than equation (3) should be used to estimate ρ . One alternative is to use the sum of the dividend yield and the expected rate of growth in the dividend. As I have shown [6, Ch. 4], under reasonable assumptions this quantity is the rate of return investors require on a share, and under MM's dividend rate theorem, it is equal to ρ , the cost of equity capital.

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Some Estimates of the Cost of Capital to the Electric Utility Industry, 1954-57: Comment

In a recent article [1], Merton H. Miller and Franco Modigliani (MM) applied a variation of their well-known model in the statistical estimation of the cost of capital to the electric utility industry. While estimates obtained for the cost of capital by MM have some intuitive appeal, we conclude, on the basis of the work reported here, that these results are to be attributed more to circumstances prevailing at the time of the MM study than to the theoretical consistency of the model employed.

In this paper we report the results of an extension of the analysis used by MM to the years 1955 and 1958-64 inclusive. Although our data sources are slightly different (see Appendix A), we are convinced that the results for the three years reported by MM are not typical of those for the entire eleven-year period. Our tests indicate that after 1957 the estimates of the cost of capital derived from the MM model fall to unreasonably low levels, and the percentage of variance explained by the model decreases significantly. Further experimentation isolates the weaknesses in the MM formulation and gives strong support to the conclusion that the model is fundamentally misspecified. The results of these experiments are reported here.

Testing the MM Model in 1954-64

MM found that the value of the cost of capital (ρ) implied by their model was reasonably stable (in the area of 5 or 6 per cent) and generally increasing over the years 1954, 1956, and 1957. In applying MM's model in the years 1954-64 to the same sample of 63 companies as used by MM,¹ we used the same two-stage instrumental variables procedure described by MM to alleviate the problem of measurement error in the earnings term. The results are

¹ As given in Appendix A in [1].

TABLE 1—MM MODEL: SECOND STAGE ESTIMATES WITH COMPUTED EARNINGS

(Coefficients based on RMH Data)^aDependent Variable: $(V - \tau D)/A$

| Year | Coefficients of | | | Multiple R | ρ 1/(Earnings Coefficient) |
|------|--|--------------------------|------------------------------|---------------|---------------------------------------|
| | Computed Earnings $(\bar{X} - \tau \bar{R})/A$ | Size $1/A \cdot 10^7$ | Growth $\bar{\Delta A}/A$ | | |
| 1954 | 18.3 (0.42) | -.235 (.06) | .160 (.13) | .77 | .055 |
| 1955 | 17.8 (0.50) | -.027 (.09) | .411 (.17) | .71 | .056 |
| 1956 | 17.6 (0.59) | -.090 (.11) | .383 (.23) | .66 | .057 |
| 1957 | 16.5 (0.72) | -.242 (.13) | .768 (.30) | .66 | .061 |
| 1958 | 17.9 (0.84) | -.256 (.16) | .799 (.35) | .70 | .056 |
| 1959 | 18.7 (0.86) | -.322 (.19) | .856 (.38) | .66 | .053 |
| 1960 | 19.6 (0.95) | -.330 (.25) | .593 (.43) | .56 | .051 |
| 1961 | 22.5 (1.23) | -.573 (.35) | .804 (.63) | .51 | .044 |
| 1962 | 20.8 (0.89) | -.349 (.27) | .424 (.56) | .60 | .048 |
| 1963 | 21.9 (0.99) | -.506 (.31) | .826 (.79) | .60 | .046 |
| 1964 | 22.3 (0.93) | -.680 (.34) | .743 (.92) | .67 | .045 |

^a See Appendix A.

shown in Table 1. Even after correcting for the probable error in the growth term, as described in Appendix A, our estimates of the cost of capital closely parallel those obtained by MM for the 1954-57 period. However, the results for the 1958-64 period conflict with those of 1954-57 in several respects. First, the cost of capital (as defined by MM) implied by the coefficient of the earnings term declines from 6.1 per cent in 1957 to a level of 4.4 per cent in 1961. In 1961, the value of ρ implied by the MM model is less than the average yield on Aa-rated public utility bonds. Secondly, the percentage of the

variance explained by the model (the square of multiple R in Table 1) declines from .60 in 1954 to .26 in 1961.²

Because these results were at odds with economic logic we attempted to determine the possible causes. Toward this end, we investigated the specification of each of the basic variables in MM's structural model: the hypothesized relationship between leverage and value, the homogeneity assumption, the size factor, and the effect of growth on value. The results of this investigation are reported below.

Leverage and Value

The effect of leverage on the value of the firm is still a much debated issue. We tested a simple equity-only valuation model by subsuming an average leverage effect. The obtained regression estimates of the "cost of equity capital with leverage" for the 1954-64 period were slightly above those shown in Table 1, but not materially so. While the test was not designed to resolve the question of how leverage affects value, the results led us to conclude that MM's treatment of leverage was not the major cause for the unusual results derived from their model.

Homogeneity of the MM Equation

MM rationalize the suppression of the regression constant by stating that failure to suppress it "would imply that the book value of total assets appears . . . as one of the explanatory variables in the original equation—a specification that makes little sense from the standpoint of the theory of valuation" [1, p. 350]. To confirm this statement and to test the structural specification of their valuation equation, MM add a constant term to their structural equation with the expectation that it will not be statistically significant. In the three years tested by MM they found "considerable support for the hypothesis that the true constant term is zero" [1, p. 362].

A case can be made that because of multicollinearities among the independent variables it is not necessary to demonstrate that the regression estimates of the constant term are statistically insignificant. But, extending the estima-

²In order to ascertain that the causes for these results lay in the model and not in the use of RMH data, we replicated data for the year 1961 according to MM specifications and performed the regression analysis upon them. A comparison of data is given below. As can be seen, the results are not materially different.

MM MODEL: SECOND STAGE REGRESSION WITH COMPUTED EARNINGS—1961
Dependent Variable: $(V - \tau D)/A$

| | Computed Earnings | Size | Growth | Multiple R | ρ |
|----------|-------------------|---------------|---------------|--------------|--------|
| Our MM | 22.7 (1.32) | -.51 (.36) | 1.13 (.64) | .52 | .0441 |
| RMH data | 22.5 (1.23) | -.57 (.35) | .804 (.63) | .51 | .0444 |

TABLE 2—MM MODEL: SECOND STAGE ESTIMATES WITH COMPUTED EARNINGS
 (Coefficients based on RMH Data)
 Dependent Variable: $(V - \tau D)/A$

| Year | Coefficients of | | | | Multiple R | ρ 1/(Earnings Coefficient) |
|------|-----------------|--|--------------------------|------------------------------|---------------|---------------------------------------|
| | Constant | Computed Earnings $(\bar{X} - \tau \bar{R})/A$ | Size $1/A \cdot 10^7$ | Growth $\bar{\Delta A}/A$ | | |
| 1954 | .053 | 17.3 (1.9) | -.225 (.07) | .157 (.13) | .77 | .058 |
| 1955 | .088 | 16.1 (2.1) | -.032 (.09) | .378 (.18) | .72 | .062 |
| 1956 | .139 | 15.1 (2.3) | -.110 (.11) | .321 (.23) | .67 | .066 |
| 1957 | .052 | 15.5 (2.6) | -.240 (.13) | .743 (.31) | .66 | .065 |
| 1958 | -.223 | 22.0 (3.1) | -.258 (.15) | .944 (.36) | .71 | .046 |
| 1959 | -.500 | 27.6 (3.8) | -.361 (.19) | 1.29 (.41) | .69 | .036 |
| 1960 | -.635 | 31.0 (5.5) | -.359 (.24) | 1.04 (.47) | .60 | .032 |
| 1961 | -.930 | 38.7 (7.6) | -.600 (.34) | 1.73 (.74) | .56 | .026 |
| 1962 | -.619 | 31.1 (5.1) | -.300 (.26) | 1.13 (.64) | .63 | .032 |
| 1963 | -.430 | 29.0 (4.9) | -.458 (.31) | 1.38 (.87) | .62 | .035 |
| 1964 | -.426 | 29.2 (4.3) | -.595 (.34) | 1.19 (.94) | .68 | .034 |

tion of this coefficient to later years, we found that it becomes negative and increases in absolute magnitude (see Table 2). In 1961 the absolute value of the constant term is approximately equal to the mean of the dependent variable.

It might be argued that these results are simply the product of multicollinearity among variables and, therefore, that the nonhomogeneous test of specification is in error and should be ignored. While this reasoning has some logical appeal, it cannot be supported fully by the figures in Table 2. First, the continued stability and significance of the other explanatory variables and the measureable increase in the coefficient of determination indicate that the

constant term adds explanatory power in its own right. Second, the negative sign on the constant term from 1958 through 1964 implies that the book value of total assets appears to be negatively correlated with the market value of the firm.

We find it more reasonable to conclude that the regression estimates of the constant term may be reflecting determinants of corporate value omitted from the MM structural equation.

Size and Valuation

MM's treatment of the effect of size on value is, at best, ambiguous. The authors' motivation for including a size term is that "the results of previous valuation studies . . . suggest that the true market capitalization rate for expected earnings of large firms may possibly differ systematically from that of small firms in the same industry" [1, p. 347]. MM caution that the equation with a size term included "must be interpreted as the linear approximation . . . to the underlying nonlinear relation" [1, p. 348].

If we assume for a moment that $\Delta \bar{A} = 0$, the role of a_0 can be depicted graphically as in Figure 1.

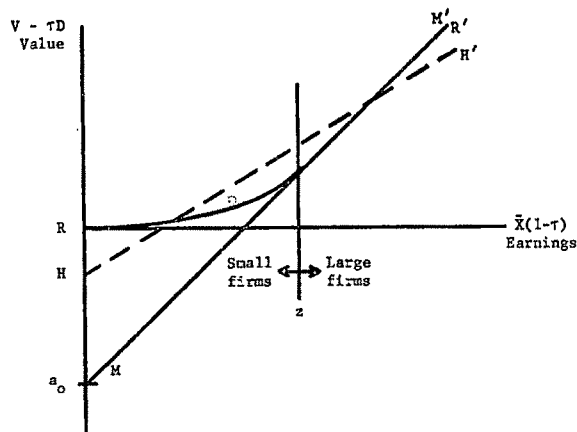


FIGURE 1

In Figure 1, the curved line (RR') represents the assumed true relationship between earnings and value as hypothesized by MM. The slope of RR' is the inverse of the cost of capital for various levels of earnings. Since earnings are normally highly correlated with firm size, the observations of small firms would be close to the origin. Assuming that individual firm observations will lie along the curve RR' , it is clear that in estimating the cost of capital of large firms a regression estimate including a size term (MM') is superior to one which is constrained to pass through the origin. The shape of RR' in Figure 1 implies increasing returns to scale in valuation, which is consistent with the negative values of a_0 observed in Table 1.

MM contend that beyond some minimum firm size a linear approximation

to the underlying nonlinear relationship is a reasonable estimate of the marginal cost of capital to large firms. It is, therefore, important that the MM sample consist entirely of firms which are large enough (i.e., to the right of point z in Figure 1) to be in the region where the linear approximation applies. If firms of smaller size are included in the sample, the resultant cost of capital estimates will not be unbiased estimates of the cost of capital for the large firms as sought by MM.³ For example, the inclusion of several small firms in the sample may give rise to an estimate of ρ as the inverse of the slope of the line HH' in Figure 1.

We do not object to the logic of this proposition, but we do question whether MM have been a bit careless in its application. More directly, we believe that the definition of what constitutes a large as opposed to a small firm might be determined within the context of the particular sample.

If the model were otherwise correctly specified, the appropriateness of the MM sample with regard to size could be tested. However, if the model were not correctly specified, such a test might merely confirm that the model is fundamentally misspecified. We tested the following hypothesis for 1957 and 1961 using data exactly as described by MM:

Rank the firms in the sample by total assets and divide them into smaller- and larger-firm groups. Let ρ_s and ρ_L equal the regression estimates of the cost of capital for the smaller-firm and larger-firm groups respectively. If the hypothesis that $\rho_s = \rho_L$ is satisfied, then the MM sample is sufficiently uniform with respect to asset size. If $\rho_s > \rho_L$, we can infer that the sample is not sufficiently uniform. If $\rho_s < \rho_L$, the specification of the model is to be suspected.

In both years, we found $\rho_s > \rho_L$, at a confidence level of 90 per cent in 1957 and 80 per cent in 1961.⁴ Thus, the problem of the appropriate size of the sample remains unanswered.

Growth and Valuation

MM imply, quite correctly, that growth has value only if the company is expected to earn a rate of return in excess of its cost of capital on its expected future net investments. But MM's formulation of the growth term is not designed to measure correctly the effect of growth on value. The "rate of change in assets" ($\Delta\bar{A}/\bar{A}$) is used as the surrogate variable for growth, *which necessitates the assumption that ρ^* (the allowed rate of return on new investments) is constant for all firms in the sample*. This assumption is unrealistic both on a priori and on empirical grounds. The inclusion of a few companies with differing rates of return may completely distort the values not only for

³The fact that the book value of total assets was not statistically significant as an independent variable in the years tested by MM might be construed as proof that the firms in the MM sample are sufficiently large. However, as we indicated in the preceding section, this fact is not generally true in later years.

⁴If the constant term in the normalized valuation equation is not suppressed, the results are exactly the opposite, i.e., $\rho_L < \rho_s$. While these results are more in accord with our expectations, we are unwilling to place much emphasis on them due to the questionable influence of the constant term as explained in the previous section.

the "growth" coefficient but also for the other coefficients in the regression equation. As a simple test of MM's failure to take this factor into account, we formulated and tested the following hypothesis: Divide the companies in the sample into a high-return and a low-return group on the basis of the rate of return earned on assets. The coefficient of the growth variable should be greater and more significant for the high-return group if growth has an important impact on value.

The results of our test are summarized in Table 3. A number of observations appear in order based on an analysis of the data in Table 3.

TABLE 3—SECOND-STAGE REGRESSION WITH COMPUTED EARNINGS:
COEFFICIENTS AND DATA MEANS 1957 AND 1961
HIGH- AND LOW-EARNING GROUPS (CONSTANT TERM SUPPRESSED)
(Our MM Data—Correct Growth)
Dependent Variable: $(V - \tau D)/A$

| | a_0 | Size | a_1 | Computed Earnings | a_2 | Growth | ρ (per cent) |
|--------------------|----------------|------|-----------------|-------------------|----------------|--------|-------------------|
| <i>1957</i> | | | | | | | |
| Combined sample | -.21 (.13) | .068 | 16.26 (.74) | .047 | .89 (.28) | .11 | 6.15 |
| High-earning group | -.22 (.16) | .078 | 16.46 (1.04) | .051 | .78 (.43) | .11 | 6.08 |
| Low-earning group | -.16 (.25) | .056 | 15.94 (1.18) | .044 | 1.01 (.41) | .11 | 6.27 |
| <i>1961</i> | | | | | | | |
| Combined sample | -.51 (.36) | .053 | 22.65 (1.32) | .049 | 1.13 (.64) | .09 | 4.41 |
| High-earning group | -.47 (.36) | .070 | 19.50 (1.83) | .052 | 4.06 (1.17) | .07 | 5.14 |
| Low-earning group | -.92 (1.24) | .036 | 22.78 (2.18) | .046 | .71 (.85) | .10 | 4.39 |

First, and foremost, in 1961 the coefficient for the growth variable for the high-earning group is five and a half times as large as the coefficient for the low-earning group and highly significant statistically. Secondly, in 1957, the differences in the growth coefficient for the two groups are relatively small and appear to be in the wrong direction, i.e., the coefficient for the low-earning group is higher than for the high-earning group. However, these differences are not statistically significant. Thirdly, it appears that the smaller companies are the lower-earning companies on the average (see the means of the size coefficients in Table 3). But, in 1961, the smaller, lower-earning companies were experiencing a higher rate of growth in assets than the larger, higher-earning group. This factor would tend to distort further the growth coefficient values for the combined sample.

Finally, the improper specification of the "growth" variable will, in general, affect the estimates of the coefficients for the other variables in the MM model. As evidence, note the differences in the values of the various coefficients for the high- and low-earning group for 1961 in Table 3. In our opinion, this particular error in specification is so fundamental that it calls into question the value of the model itself as an estimating tool.

Conclusion

The misspecification of the growth variable combined with the effects on value of the unknown factors embodied in the regression constant emerge as the main causes of the inadequacy of the MM model. We conclude that the deficiencies in MM's model are so fundamental in nature that none of the estimates of the cost of capital reported either by MM or here can be considered reliable estimates of the true cost of capital to electric utility firms.

APPENDIX A

The Test Methodology

We had access to financial data for the companies used in the MM study as prepared by Studley, Shupert & Co., Inc. However, these data, henceforth called RMH data, were not identical in form to the data used by MM. In particular, the RMH data contained book rather than market values for the long-term debt. Therefore, to test fairly MM's model for years subsequent to 1957, we felt it necessary first to attempt to reproduce their reported results.

We selected the year 1957 as the test year. In attempting to reproduce their results, we found data sample means and standard deviations essentially equivalent to those in MM's Appendix B for all the variables except the growth variable (see columns (a) and (c) in Table A-1).

While our size (assets) means were nearly equal, we found that our mean growth term, $\Delta \bar{A}/A$, was 10.9 per cent, whereas MM's was 8.2 per cent. After careful verification of asset figures in *Moody's Utility Manual*, we conclude that a probable error exists in MM's computation of the growth variable. If $\Delta \bar{A}/A$ is calculated as the quantity $[1/5(A_t - A_{t-4})/A_{t-4}]$ rather than the correct $[1/5(A_t - A_{t-5})/A_{t-5}]$, one obtains a mean and standard deviation of the growth term identical to MM's (compare columns (a) and (b) in Table A-1).⁵

⁵ The impact of this growth term error on the earnings term regression coefficient, hence on ρ , is relatively small. In 1957, for example, the value of ρ implied by MM's two-stage computation is 6.2 per cent, whereas with a corrected growth term ρ is 6.5 per cent (see our Table 3).

However, the impact of the apparent computational error on MM's interpretation of the valuation of growth is more significant. While MM caution that their estimates of implied time horizons for growth (in their Table 11) are not to be taken seriously, they nevertheless conclude that a general expansion of time horizons was indicated. A comparison of growth time horizons for 1957 implied by MM's model before and after correction of the error in computation shows that the estimate of the horizon declines from 11 years (as estimated by MM) to below 7 years.

TABLE A-1—MEANS AND STANDARD DEVIATIONS OF PRINCIPAL VARIABLES—1957

| Variable | MM ^a | Our MM Data ^b (Adjusted Growth) | Our MM Data ^c (Correct Growth) | RMH Data ^d |
|--|-----------------|---|--|-----------------------|
| Value $\left(\frac{V-\tau D}{A}\right)$ | .851 (.12) | .855 (.12) | .855 (.12) | .897 (.12) |
| Earnings $\left(\frac{\bar{X}^* - \tau \bar{K}}{A}\right)$ | .0471 (.006) | .0475 (.005) | .0475 (.005) | .0506 (.007) |
| Size $[1/A \cdot 10^7]$ | .0670 (.091) | .0675 (.092) | .0675 (.092) | .0723 (.098) |
| Growth $[\Delta A/A]$ | .082 (.030) | .082 (.030) | .109 (.040) | .106 (.041) |
| Debt/assets | .504 (.051) | .508 (.060) | .508 (.060) | .545 (.045) |
| Preferred/assets | .100 (.047) | .100 (.048) | .100 (.048) | .113 (.054) |
| Dividends/assets | .0243 (.004) | .0245 (.004) | .0245 (.004) | .0262 (.004) |

^a Results as presented in MM's Appendix B.

^b Results using data that we gathered precisely according to MM's specifications, but with the computational difference in growth term described in text.

^c Results using data as in b, with correct growth term.

^d Results using RMH data.

TABLE A-2—MM MODEL: SINGLE STAGE REGRESSION WITH MEASURED EARNINGS—1957
Dependent Variable: $(V-\tau D)/A$

| | Coefficients of | | | | Multiple R |
|--|-----------------|-------------------|----------------|---------------|------------|
| | Constant | Measured Earnings | Size | Growth | |
| MM ^a | .164 (.06) | 15.7 (1.2) | -.278 (.08) | 1.37 (.24) | .88 |
| Our MM ^b (adjusted growth) | .025 | 15.5 (1.2) | -.207 (.08) | 1.30 (.24) | .88 |

^a Results as presented in MM paper.

^b Results using data that we gathered precisely according to MM's specifications, but with the computational difference in growth term described in text.

Table A-2 reflects the degree to which we were successful in reproducing the MM regression results. This table compares the results for the single-stage regression with measured earnings obtained by MM (their Table 1) with results using "Our MM" data with the growth term as apparently calculated by MM. At this stage we were satisfied that our numbers closely paralleled those used by MM.

We then computed sample data means and standard deviations for the RMH data shown in column (d) in Table A-1. Before using these data to extend MM's results, we wanted to test the degree to which the analyses based on RMH data would approximate results obtained using data according to MM specification. Consequently, we performed the second-stage regressions with computed earnings for 1957 with three sets of data: (1) our MM (with adjusted growth), (2) our MM (correct growth), and (3) RMH data. The results of this analysis are shown in Table A-3.

TABLE A-3—MM MODEL: SECOND STAGE REGRESSION WITH COMPUTED EARNINGS—1957
Dependent Variable: $(V - \tau D)/A$

| | Coefficients of | | | | Multiple R | ρ (1/Earnings Coefficient) |
|--|-----------------|----------------------|----------------|---------------|---------------|---------------------------------------|
| | Constant | Computed Earnings | Size | Growth | | |
| MM ^a | -.004 (.08) | 16.2 (1.7) | -.280 (.08) | 1.37 (.24) | .87 | .062 |
| Our MM ^b (adjusted growth) | .012 | 15.8 (2.4) | -.210 (.12) | 1.30 (.37) | .70 | .063 |
| Our MM ^c (correct growth) | .046 | 15.4 (2.5) | -.208 (.13) | .85 (.30) | .66 | .065 |
| RMH data ^d | .052 | 15.5 (2.6) | -.240 (.13) | .74 (.31) | .66 | .065 |

^a Results as presented in MM paper.

^b Results using data that we gathered precisely according to MM's specifications, but with the computational difference in growth term described in text.

^c Results using data as in b, with correct growth term.

^d Results using RMH data.

A comparison of the regression results based on "Our MM-correct growth" data with the "RMH data" discloses no material differences. While we could not be absolutely certain that regression analyses based on RMH data would faithfully parallel results obtained with data under MM specification in other years, we were convinced that major trends would not be obscured by use of the RMH data.

APPENDIX B: MEANS OF PRINCIPAL VARIABLES—RMH DATA
1954-1964

| Years | Variable | | | $\frac{\bar{\Delta A}}{A}$ |
|-------|--------------|----------------------------|------------------|----------------------------|
| | $V - \tau D$ | $\bar{X}^r - \tau \bar{R}$ | $1/A \cdot 10^7$ | |
| | A | A | | |
| 1954 | .898 | .049 | .091 | .129 |
| 1955 | .945 | .050 | .084 | .120 |
| 1956 | .934 | .051 | .078 | .112 |
| 1957 | .897 | .051 | .072 | .106 |
| 1958 | .964 | .050 | .067 | .105 |
| 1959 | 1.024 | .051 | .063 | .100 |
| 1960 | 1.051 | .052 | .059 | .094 |
| 1961 | 1.124 | .053 | .057 | .085 |
| 1962 | 1.149 | .055 | .054 | .071 |
| 1963 | 1.232 | .055 | .053 | .059 |
| 1964 | 1.282 | .058 | .051 | .050 |

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REFERENCE

1. M. H. MILLER AND F. MODIGLIANI, "Some Estimates of the Cost of Capital to the Electric Utility Industry, 1954-57," *Am. Econ. Rev.*, June 1966, 56, 333-91.

Some Estimates of the Cost of Capital to the Electric Utility Industry, 1954-1957: Reply

Ever since our paper appeared in this *Review* people have been telling us: "Very interesting. But your estimates of the cost of capital are really much too low to be believed." We suspect that, to some extent, this impression is simply a misunderstanding of our estimates, especially those presented in the critical summary Table 10. To keep the exposition simple, we stated the required rate of return in terms of a useful but unfamiliar

concept of income which we have dubbed "tax-adjusted earnings." This concept is essentially an after-tax measure which also takes into account the tax subsidy to debt, a subsidy which is very substantial indeed in this industry because of the prevailing high levels of debt. Hence, our estimates are bound to seem low if gauged against other more familiar measures of yield which are not or cannot be similarly adjusted. Examples of just this sort of improper comparison are found in the present critique by Myron Gordon (see his Section IV, 2nd par.) as well as in an earlier critique by Irwin Friend.¹

The impression that our estimates are low is perhaps also reinforced by the fact that the measure given in Table 10 as well as in other tables was not adjusted for the effect of size of firm and hence applies strictly speaking only to firms of infinite size. In our paper we did make some attempt to guard against these misunderstandings by also recasting our results in terms of the implied *before-tax* required yield for a firm of size equal to the "average" of our sample. But this was done only in a footnote (n. 54, p. 380); and to make matters worse, the numbers we actually gave in that footnote—.073, .087, and .090 respectively for 1954, 1956, and 1957—are erroneous. That there must have been something wrong with these numbers might have been apparent to the reader since, the before-tax yield should equal our tax-adjusted yield of Table 10 divided by one minus the marginal tax rate, or .48. Thus, even without correction for the size effect, the figures of footnote 54 should have been more than twice those reported in Table 10. However, since we failed to spot the error we can hardly blame anybody else for doing the same. In any event, with the additional intended adjustment for the effect of size, the correct estimates of the before-tax required yield implied by our results are .079, .094, and .098 for 1954, 1956, and 1957 respectively.²

All this is not to suggest, of course, that the widespread feeling that our estimates are unreasonably low, and that therefore something must be wrong with our approach, rest entirely on misunderstandings or on the misprint in our footnote. There are some genuine issues of substance involved. We, therefore, welcome the very detailed critique by Crockett and Friend, which presents most of the standard objections to our techniques and estimates as well as an independent estimate of the cost of capital

¹ This critique, together with our reply, is in [1, pp. 257-67].

² These figures can be obtained in a variety of ways. The simplest is to start with the cost of equity capital (as in Table 8) and make the size correction indicated in Table 9. The results for the three years are .052, .061, and .064 respectively. To obtain the size-adjusted and tax-adjusted average cost of capital, multiply these figures by $[1 - \tau(\bar{D}/A)]$ using the value of $\tau(\bar{D}/A)$ already computed in Table 7, row 2. The results are .038, .045, and .047 respectively. Finally, dividing these figures, by .48 we arrive at the before-tax required yields given above.

We should like to take this opportunity to correct a few additional misprints that we have uncovered in the meantime. In Table 1, first panel, the entry for the constant term in column 1 for 1957 should read .0164 instead of .164. In Table 2, first panel, the entry for the constant term in 1954 in column 1 should be .041 rather than .01; in the same row, next column, the parentheses should be removed from the entry (.0090); and in the second panel the debt coefficient for 1957 should read -.006 rather than -.066. Finally, the estimates of the time horizons for growth given in Table II are somewhat too long. In this connection, see footnote 15 below.

for utilities during this period. We hope that this reply will give us an opportunity to allay most of the misgivings that have been voiced.

The Supposed Biases in Our Estimating Procedures

Let us first consider Crockett and Friend's list of "substantial shortcomings in the statistical model of which [we] do not appear to be aware" and which, they claim, have led us to underestimate substantially the cost of capital. One such is that our use of dividends, along with debt, and preferred stock as instrumental variables biases the earnings coefficient upward (and hence biases downward our estimate of the cost of capital) because of a supposed "negative correlation of dividends plus contractual disbursements with risk." We fear that this criticism involves a failure to appreciate fully the properties of instrumental variables estimation. Furthermore, if it were really true that an above-average pay-out and leverage is significantly associated with below-average risk, leading to a more favorable capitalization of earnings and hence to a higher market value, then the tests of Tables 4 and 5 in which these variables were introduced explicitly in addition to our "computed earnings" should have revealed at least some significant positive effects for these variables, contrary to what we actually found. However, there is no need to argue the point along these lines. Anticipating that for any of a number of reasons some readers would be made uneasy by our choice of instrumental variables and particularly by our use of dividends we took the precaution of providing not one, but two supplementary tests. One consisted of dropping dividends entirely from the list of instrumental variables. As reported in footnote 41 the estimate of the cost of capital turned out to be almost identical. The second consisted of direct least squares estimates using what we called the "yield formulation." This formulation, we showed, can surmount the problem of bias introduced by the measurement error in earnings without recourse to instrumental variables, but it has other compensating disadvantages that we feel limit its usefulness to that of serving as a check test. The results of using this very different type of test were quite close to our original estimates (cf. our Table 6 and surrounding text discussion) suggesting once again that if our estimates of the cost of capital are indeed too low, this cannot be attributed either to our instrumental variable approach or to the particular instrumental variables we chose to use.

Another supposed shortcoming is our method of controlling for the effect of size on valuation. Crockett and Friend point out that this method could lead to an overstatement of the average earnings coefficient if ρ declined sharply with size; and they recommend the use of an explicit size variable to take this possibility into account. Actually, of course, our choice of procedure for controlling for size was not made casually or carelessly. We did look first at the scatters of V on X ; we could see no evidence of strong curvature of the kind hypothesized in their Figure 1b. Nor does there even exist in this industry a relevant group of small firms lying far below the sample range (i.e., to the left of \bar{X}_1 in their diagram). In any event, to make doubly sure that our simple method of controlling for size was not

leading to any serious distortion we also ran check tests using a continuous size variable (specifically, the log of total assets). As we reported in our paper (see footnote 51) neither the relative importance (nor direction) of the size term nor the values of the other coefficients were noticeably affected.³

Still, Crockett and Friend are correct in asserting that the measures given in our tables tend to understate the average cost of capital in that they are derived from capitalization rates which, as we acknowledged, apply only to firms of infinite size. The average cost implied by our results for any specified size of firm can be readily computed from our estimate of a_0 , the coefficient of the size variable. But since our results indicate that the effect of size is rather small and because the firms in our sample are all relatively large, correction for size cannot affect our estimates materially. In particular, the difference between the capitalization rate at infinite size and that corresponding to the mean value of the size variable in our sample in each of the three years is less than 3 per cent (cf. footnote 2 above). In terms of Crockett and Friend's Figure 1b, the difference between the slope of the line AB and that of the line OC is quite tiny within the range of sizes included in our sample.⁴

Finally, there is the question of the variable we have used to measure valuable future growth opportunities and which comes in for some criticism in each of the three comments. The problem of measuring this important, but not directly observable, quantity is a most complex and vexing one for which no one to our knowledge has yet been able to offer a completely satisfactory solution. We were the first to point out (cf. n. 17, p. 345) that our own approach suffered from shortcomings which would make its general applicability very questionable. And while we argued that these shortcomings would be at a minimum for our industry over the specific period studied, we would not assert that they are altogether absent. In particular, we would not deny that in principle our approach could produce an upward bias in the coefficient of the earning variable, as suggested by Crockett and Friend, because the ratio of earnings to assets might be conveying informa-

³ Our results using a continuous size variable are thus in conflict with some results reported by Robichek *et al.* They find, for 1957 and 1961, significantly lower earnings coefficients for firms above the sample mean in size than for those below the mean (i.e., curvature in the reverse direction from ours and from the direction that would be expected a priori). The fact that our continuous size variable shows orthodox curvature would seem to suggest that their reverse curvature from splitting the sample, far from providing a more reliable picture of the relation between value and size in the industry as a whole, probably comes from forcing the separate slopes to fit some conspicuous deviants at either extreme.

⁴ It is perhaps also worth noting that, if in fact the rate at which the market capitalizes earning rises with the size of assets, then the yield required of an incremental investment will not be the "average" cost of capital. It will be instead a "marginal" cost, which is below the average as it reflects the favorable effect that the addition to assets will have on the capitalization of earnings from the initial assets (just as, for a monopolist, the incremental revenue resulting from an increase in sales is less than the average revenue). If one is prepared to regard our formulation as a bearable approximation to the effect of size in the relevant range, then the *marginal* capitalization rate is precisely a_1 , of our equation (14), and therefore the relevant marginal cost of capital is that given in our tables, without any need to correct for size. That is why we never expressed the capitalization factor itself as a function of size, and only took size into account in the tables showing the components of valuation for the "average" firm.

tion about inter-firm differences in ρ^* , the rate of return expected from future investments.

The issue, however, is not whether our measure of growth opportunities has shortcomings—which it certainly does—but whether these shortcomings have led to a substantial underestimate of the cost of capital. Here, we would like to point out that there is at least a *prima facie* inconsistency in arguing, as do Crockett and Friend, that we both underestimate the contribution of growth to value and (largely as a consequence) also underestimate the cost of capital. For, it is conceded that the growth component arises from an excess of the rate of return on investments over the cost of capital. Hence, the presence of important growth opportunities would mean that the rates of return allowed by the regulatory authorities were significantly higher than the cost of capital. As noted in our paper (cf. Table 11 and surrounding text) an allowed rate of return after taxes on the order of the traditional 6 per cent implies a tax-adjusted return of about 5.2 per cent. Thus, if we have substantially underestimated the growth component as claimed, it is hard to see how the cost of capital could have been appreciably higher at least in 1956 and 1957 than our estimate of about 4.6 per cent.

Summing up, we feel that Crockett and Friend have not succeeded in showing any fatal flaw in our procedures which would permit our estimates to be dismissed out of hand or which would even establish *a priori* a presumption of serious bias. This is not to claim, of course, that our procedures are unexceptionable. There may well be more insidious and unsuspected mechanisms at work which have led us to underestimate substantially the cost of capital to utilities during this period. Before speculating about these, however, it is worthwhile to ask whether there is any evidence that our estimates really are so wide of the mark as to have to be explained away.

Are Our Estimates Really Far Too Low?

Both Gordon and Crockett-Friend are convinced that they have such evidence. Gordon's is that our estimates of ρ are consistently below the earning-price ratio, whereas "it is generally accepted that when super growth opportunities [valuable growth, in our terminology] are present the earning yield, Y/S , is below ρ , the cost of equity capital." But this evidence is worthless since it compares the required rate of return for a firm financed entirely by equity funds with the observed yield on stock, a comparison which is especially treacherous for a highly levered industry like electric utilities. This point is brought out in our Table 9 which was devoted precisely to clarifying the relation between the two measures. The presence of valuable growth does of course tend to depress market yields on stock—to the tune of some 20 per cent in 1957—but this effect is more than offset by the very high leverage which results in the observed average yield on stock exceeding ρ , the yield on an unlevered stock, by some 25 per cent.

A more promising and enlightening way of assessing our estimates is to compare them with the results of other "sensible" methods of estimation. This is the task undertaken by Crockett and Friend in the last half of their

paper, the method used being that suggested by Friend in his earlier critique of our paper.

We shall not here comment on the many steps involved in what they choose to call their "direct" estimates partly for reasons of space and partly because they themselves would regard their "admittedly crude empirical results" as intended to be only suggestive. Let us instead turn directly to the results of the calculation on which they base their conclusion that our estimates are significantly understated.

At this point we must call attention to one seemingly small, but actually quite important, difference between the two methods of presenting the final estimates. In constructing their overall weighted average cost of capital from the separate yields they used *market*-value weights. Our estimate, on the other hand, weights the debt component by its *book*-value. Of the two weighting schemes, we feel that the book-value weights are the more appropriate ones to use when talking about the cost of new financing (as we indicated, p. 378); but that is not the issue. The critical point is simply that the weights have to be the same in both estimates so as to permit a meaningful comparison. Adjusting their estimates to ours by using book-value weights (and using the actual tax rate of 52 per cent, as we did, rather than their 50 per cent) we find that their best estimate of the average before-tax cost of capital for the years 1954-57 now becomes 9.7 per cent (as against their original 10.7 per cent); and their lower-bound estimate becomes 8.9 per cent (as against their original 9.5 per cent). By contrast, our supposedly "low" estimates of the before-tax cost of capital were shown to be 9.8 per cent for 1957, 9.4 per cent for 1956, and 7.9 per cent for 1954.

That the Crockett-Friend rough estimates do not turn out to be significantly different from ours should not really be surprising. There were a number of clues in the numbers presented that should have forewarned them. The key element in their computation is the assumed required after-tax yield on equity capital, and if their estimates were to differ substantially from ours it could only be because of a substantial difference between us in the value assigned to this figure.

After much careful weighing of the evidence, speculation about growth rates, and soundings of professional opinion, they settle on an average yield on equity for the middle and late 1950s of between 7.5 per cent and 8.5 per cent. Consulting our Table 9, they could have seen that the average required equity yields implicit in our calculation (the sum of the average earnings yield and the growth effect, as reported in Table 9) came to 8.4 per cent for 1957, 7.8 per cent in 1956, and 6.9 per cent in 1954.⁵ Perhaps they might have wanted to quarrel with our figure for 1954, although we could also point to the fact that our 1954 figure is by no means far out of line with interest rates during the period which were also quite low by later standards. But even with this reservation, it should have been clear that our estimates could not be so far off after all.

So much then for our supposed substantial underestimate of the cost of

⁵ These figures can also be reached, up to rounding errors, by summing the cost of equity capital, the size effect, and the leverage effect as reported in the same Table 9.

capital. We turn now to Gordon whose main concern is not so much with our estimates as with our interpretation of the results of certain supplementary tests of the effects of leverage and dividend policy on market valuation.⁶

The Leverage and Dividend Tests

On the question of leverage Gordon argues that the derivation of our fundamental valuation equation is not applicable to electric utilities because it relies on the assumption that before tax earnings X "is an exogenous random variable independent of the corporation's tax and leverage rates." This assumption, he suggests, is not applicable to electric utilities because the rate of return which regulation aims at establishing is based on after-tax earnings X^r , all gains from leverage thus accruing to the customers through lower before-tax earnings. Accordingly it is " X^r and not X [which is] independent of the tax rate and financial policy." He concludes that the relevant basic valuation formula, regardless of corporate taxes, is $V = (1/\rho)/X^r$ rather than our $V = (1/\rho)/X(1-\tau) + \tau D$. If so, and if there were no other gains from leverage, then the coefficient of debt in our valuation equation should be a substantial *negative* number. Our finding of a zero coefficient would thus actually imply a considerable independent value to leverage per se, contrary to our conclusion that no gain beyond the tax subsidy was detected.⁷

This argument is interesting though its formulation may be a bit

⁶ Crockett and Friend are also critical of these tests, but they present no bill of particulars beyond some references to the role of nonlinearities. We did not go into the question of curvature in our paper mainly because the paper was already quite long and because any detailed consideration of such second-order refinements would have carried us too far afield from our main task of providing estimates of the cost of capital. For what it may be worth, however, we can at least report that in the few tests of curvature we did carry out in anticipation of an eventual follow-up paper we found no compelling evidence of strong nonlinearities of a kind that would require any modification of our conclusions about the gains from leverage or from alternative dividend policies.

⁷ Some idea of the quantitative magnitudes involved can be obtained by observing first that, if there were neither tax nor nontax advantages to leverage, the fundamental valuation equation (neglecting growth or other control variables) would be

$$(1) \quad V = \frac{1}{\rho} X^r$$

where X^r = after-tax earnings. To get an equation comparable to the one we fitted, first subtract τD from both sides and then add and subtract $\tau R/\rho$ on the right obtaining:

$$(2) \quad V - \tau D = \frac{1}{\rho} (X^r - \tau R) + \tau \frac{R}{\rho} - \tau D = \frac{1}{\rho} X(1 - \tau) + \tau \left(\frac{R}{\rho} - D \right)$$

If we can approximate R as τD (i.e., some average rate of interest times the debt) we can rewrite (2) in terms of tax-adjusted income and debt as

$$(3) \quad V - \tau D \cong \frac{1}{\rho} (X^r - \tau R) - \tau \left(\frac{\rho - \tau}{\rho} \right) D.$$

Taking ρ as approximately .06, τ as approximately .04, and τ as .52, equation (3) implies that the coefficient of D in our test equation should be something on the order of $-.15$ to $-.20$.

misleading. Actually the arbitrage argument from which we derived our basic valuation equation for the case of a corporate income tax American-style does not make explicit reference to just what determines \bar{X} . What we do need to assume is that the random variable $Z = X/\bar{X}$ is the same for all firms in a risk class and hence in particular that it is independent of leverage (though not necessarily of tax rates). Under this assumption, as shown in [2] the distribution of X^r/\bar{X}^r will depend on leverage and tax rates and in particular its variance will be reduced by leverage. Thus our treatment of electric utilities as a risk class implicitly assumes that the random variable X/\bar{X} is independent of leverage, and this assumption may appear somewhat questionable in the light of Gordon's observations about the nature of regulation. On the other hand, Gordon's valuation formula is at least as doubtful. Indeed, for his reasoning to be valid it is required not only that before-tax earnings \bar{X} should be a linearly decreasing function of leverage, as indicated by his formula (7); it would also have to be true that the distribution of the random variable $G \equiv X^r/\bar{X}^r$ is independent of leverage. This property cannot readily be deduced from the nature of regulation. In fact, it has some peculiar implications about the *modus operandi* of regulation, e.g., that it would make the uncertainty of *before-tax earnings* an increasing function of leverage.⁸

On the whole one might be inclined to conclude that a reasonable approximation to the applicable valuation formula might lie somewhere between ours and Gordon's. But there are several pieces of evidence suggesting that in fact the relevant approximation is much closer to ours than to his. In the first place, if regulation actually aimed at, and succeeded in, enforcing his equation (6) then $\bar{X}^r - \tau R/A$ would have a strong and stable negative correlation with D/A . Yet the correlation varies widely from year to year being noticeably negative only in 1957 and actually being positive, though small, in 1954. (See our appendix Table B2, p. 389.) Gordon's interpretation would also imply that leverage obtained via non-tax-deductible preferred stock would be equivalent from the stockholders' point of view with that obtained from debt. Hence if, as Gordon argues, stockholders really do prefer leverage per se, over and above any of its tax effects, we should expect to see that preference demonstrated in the case of preferred stock as well as debt. Yet, as we noted in connection with our Table 4, the coefficients of preferred stock in our test equation were neither significantly different from zero nor even consistently of the same sign. All things con-

⁸ If $X^r/\bar{X}^r = G$ independent of leverage and tax rates then

$$X = \frac{X^r - \tau R}{1 - \tau} = \frac{G\bar{X}^r - \tau R}{1 - \tau} \quad \text{and} \quad Z = \frac{X}{\bar{X}} = \frac{G\bar{X}^r - \tau R}{(1 - \tau)\bar{X}}.$$

It follows in particular that

$$\sigma_Z = \frac{\bar{X}^r}{(1 - \tau)\bar{X}} \sigma_G = \sigma_G \left[1 + \frac{\bar{X}}{(1 - \tau)\bar{X}} \right]$$

would be an increasing function of leverage. That leverage fails to increase the market value of the firm in Gordon's model may be thought of as reflecting the fact that the tax benefits of leverage would thus be offset by an increased variance of before-tax earnings.

sidered, then, it would seem safe to characterize Gordon's interpretation of our leverage tests as ingenious, but of little practical consequence in the present context.

On the issue of the effects of dividend policy, Gordon objects strongly to the form of our tests which he regards both as a poor approximation to the dividend hypothesis being tested and one which is biased against the traditional view.⁹ His position boils down to the following: the hypothesis that a high payout increases the market value of shares implies that total value should be favorably affected by the relation of dividends to *profits*, not to tax-adjusted earnings. If there were no debt or if leverage were roughly the same for all firms, then a high level of dividends relative to earnings would also imply a high payout. But leverage is not constant and since we fail to control for it (except implicitly via the τD component of the dependent variable) our test will tend to attenuate the coefficient of dividends, biasing it towards zero and understating its significance.

In principle, we would accept this argument, but it clearly cannot account for our results. For one thing, it could not explain the consistently *negative* signs of the coefficients in the more relevant two-stage test since this type of bias can push an estimated coefficient toward zero, but not change its sign. Moreover we too were concerned about possible downward bias in the dividend coefficient for related but somewhat different reasons (see our footnote 46). Accordingly, we ran a check test in which we extended the two-stage approach to the dividend variable as well as to the earnings variable. The result was that in all 3 years the dividend coefficients were less negative than those of Table 5, and in 1957 the coefficient was actually positive though only of the same small order of magnitude as in the direct least squares test. Although these results tend to confirm the existence of bias in the coefficients of Table 5 they also show that the bias is not large enough to force any modification of our main conclusion, namely that the data show no sign of a strong, positive effect of dividends of the kind hypothesized by Gordon.¹⁰

The RMH Paper

We now turn briefly to the paper of Robichek, McDonald, and Higgins (hereafter RMH). This is basically a replication of some of our tests to

⁹ Another and quite different line of attack on our dividend results is contained in Section V of the Gordon paper. That argument, however, despite the very positive tone with which it is presented and the frequent use of such terms as "hence" and "consequently," is just hand-waving and will not stand up to any serious scrutiny.

¹⁰ The reader may wonder why we did not simply present a test in terms of the value of the shares, profits, and dividends that would have met Gordon's objection directly. The answer, as we pointed out (see our footnote 48), is that it was not possible to construct such a test given the necessity of using a two-stage instrumental variable approach and given the limited set of instrumental variables we had at hand. For, in an equation with the value of shares as dependent variable it is essential to include both debt and preferred stock as independent variables in the structural equation. To include dividends as well would mean that the only thing keeping the second-stage computed profits variable from being an exact linear combination of the other independent variables would be the constant term (included in the first stage and suppressed in the second). A glance at Table 2, second panel, makes it clear, however, why this was not enough to permit identification of the second-stage equation.

another sample of public utility data running from 1954 to 1964, in which great pains were taken to follow slavishly the details of our definitions and procedures.

For the period where the RMH data overlap ours (1954, 1956, and 1957), it turns out that their results are quite close to ours, and much closer still for their special MM test sample in 1957, a sample based on precisely the same firms and definitions we used.¹¹ Naturally, we are grateful for this independent audit and gratified by the closeness of the agreement during our sample period. This does at least remove one possible suspicion about the source of our "peculiar" results! However, as they extend the tests beyond our sample period, their results change quite substantially. In particular, the coefficient of the earnings variable keeps rising, implying a drop in the cost of capital between 1957 and 1964 by some 25 per cent, to an implausibly low value.¹² Unable to offer any explanation for this striking difference in performance before and after 1957, they call a plague on both houses and conclude that "none of the estimates of the cost of capital reported either by MM or here can be considered reliable estimates of the cost of capital to electric utility firms."

Actually these results hold little surprise to anyone who takes the pain to inspect the behavior of the sample means reported in their Appendix B. The puzzle is right in these basic data and not in the results one obtains by applying our specific approach to them. For the fact of the matter is that between 1957 and 1964 their measure of market value relative to the book value of assets rises by a hefty 43 per cent (from .80 to 1.28) while at the same time the measure of the rate of earnings on assets rises a modest 14 per cent (from .051 to .058).¹³ And, to top it off, the average rate of growth of assets computed over a five year period—which for all its shortcomings is still a relevant measure of growth, without which valuable growth cannot

¹¹ Readers should not be misled by the seeming large differences in the standard errors and multiple correlation coefficient in the second stage regression for 1957 reported in their Table A-3. Apparently RMH neglected to make the necessary correction for the standard errors explained in our footnote 39. If so, all the standard errors and values of R reported in their Tables 1 and 2 are also erroneous and conclusions based on them, e.g., in their first section, are unreliable.

¹² RMH also find that, when they estimate the equation in nonhomogeneous form, the constant term which was zeroish to slightly positive for our sample period becomes negative and very sizably so. The only suggestion they offer for the absurdly large negative intercepts is that they "may be reflecting determinants of corporate value omitted from the MM structural equation." As far as we can see, however, the major difference between our structural equation and anyone else's would be in our omission of leverage and dividends. It seems hard to believe that those omissions could produce anomalies of the order of magnitude found—for despite all the controversy, the various theories are not *that* different or the issues would have been settled long ago. But RMH didn't even try to eliminate some of these more obvious candidates.

We suspect that these anomalies might be traced to inadequacies of the instrumental variables in later years, a suspicion reinforced by the fact that the constant term takes on its largest negative values just when the value of R reported by them for Table 1 is lowest. But since RMH present neither standard errors for the constant term, nor first stage regressions, nor direct least square results we have no way of checking out this hunch.

¹³ These figures relate to market value and earnings adjusted for leverage. But since leverage is unlikely to have changed drastically in the period it seems safe to infer that most of the change is in the relation of the market value of the stock to net profits.

exist—declines by 55 per cent (from .106 to .050). It is hard to see how, confronted with these data, any model—as long as it incorporated the view, by now widely accepted, that the cost of capital is closely related to the way the market capitalizes earnings, with some adjustment for growth—could fail to yield the conclusion that the cost of capital must have fallen dramatically over this interval. Indeed, when the authors discarded our model and tried to estimate directly a capitalization rate for returns to equity capital they found a drop in this rate between 1957 and 1964 of some 36 per cent!¹⁴ This conclusion seems rather unavoidable—unless there is something wrong with the basic data.

At least two possible sources of bias come readily to mind. In our study in computing V we endeavored to estimate debt at market value. Because this procedure is very time consuming RHM used the book value of the debt. Since long-term interest rates rose between 1957 and 1964 one might expect some tendency for the market value of debt to fall relative to the book value, imparting an upward bias to their estimates of V . But we doubt that this bias could be very significant (especially since the increase in the long rate was largely over by 1959).

A potentially far more serious source of trouble is in the treatment of “charges for deferred taxes” in the measurement of earnings. These charges essentially represent the discrepancy between taxes actually paid and the higher taxes that would have been paid in the absence of various incentive tax measures permitting a faster depreciation of physical assets. There are strong reasons for holding that these charges should be added back to reported net of tax earnings in measuring the earnings the market capitalizes. As indicated, we experimented with this concept of earnings in our own analysis (see p. 356). But we ended by using the reported profits which gave a somewhat better fit, on the consideration that the relevant tax provisions were relatively new, and quite possibly, even by 1957, investors had not seen through this accounting complexity. But we would rather expect that in later years the market would have learned to make the needed adjustment, especially as the size of the adjustment became more important with the revised depreciation guidelines and the investment credit. Thus the measure of earnings which we found adequate for our sample period and which was presumably used also by RMH may well have become increasingly downward biased.

These remarks about the measurement of earnings serve also to point up what in our view is the fatal shortcoming of the RMH tests, robbing them of their potential usefulness. The authors fail to appreciate that the concepts of the model—especially such concepts as the market’s evaluation of future earnings and growth potential—do not have exact observable counterparts. The best that can be done is to approximate them somehow from the data at hand. A particularly glaring illustration of this failure is provided by the

¹⁴ These estimates which are alluded to in their section “Leverage and Value” were reported in a preliminary version of their comment circulated as *Research Report #118R*, Graduate School of Business, Stanford University, Table 6. The fall in the estimated capitalization rate for profits is from .078 in 1957 to .050 in 1964.

measurement of future investment opportunities, which we suggested proxying by the observed past growth of total assets. We chose to measure past growth by the growth in assets over the previous 4 years or the ratio $(A_t - A_{t-4})/(A_{t-4})$, because, as we candidly stated, this particular measure "happens to yield consistently higher gross and net correlations with total value than other simple smoothings we have tried" (p. 345). However, we wrongly stated that the measure used was $(A_t - A_{t-5})/A_{t-5}$.¹⁵ RMH discovered our error, and confirmed that the four-year measure actually used did perform a good deal better than the five-year measure. But they then proceeded merrily to use the latter, less effective, measure we erroneously said we had used, labelling it "correct growth," as though this particular approximation was an essential logical implication of our model. The fact of the matter is that we settled on our various approximations only after facing a large number of empirical questions. The RMH results suggest that, if we had looked at their data and period rather than ours, then our answers to some of these questions would have been different. But we fail to see how this entitles them to reject the answers we did find for our sample and the approximations based on those answers—unless, of course, they are also prepared to argue that approximations not valid *everywhere* are not valid *anywhere*.¹⁶

It might be useful in closing to attempt to summarize where we think matters now stand. In critical exchanges of this kind, in which so much of the emphasis is inevitably on technical details, it is easy to lose perspective and to overlook such progress toward agreement as has been made. In terms of substantive findings, for example, we feel there should now be agreement, at least, that our estimates of the cost of capital have been constructed with due care; that the more obvious sources of bias have been checked out; and that the numbers themselves, properly interpreted, are neither manifestly inconsistent with a priori expectations nor with the results obtained by other estimating techniques.

On the matter of the leverage and dividend tests, we would certainly agree with our critics that our results do not close the issue. The problems here are far too difficult to be settled by any one empirical study. We trust they will equally agree that our results show at least that the issue is still open.

¹⁵ In the tests actually presented we restated the growth ratio in annual terms; and, misled by our notational error we multiplied the four-year growth by $\frac{1}{4}$ instead of $\frac{1}{5}$. This scaling error, of course, is of no great consequence and leaves all the other coefficients, as well as the t -ratio of the growth coefficient itself, unaffected. It does, however, affect the attempted "unscrambling" of the growth term presented in our Table 11 by leading to an overstatement of the implied horizons.

¹⁶ That they may very well have some such principle in mind is suggested also by their discussion of their "experiment" with the growth variable in their Table 3. They find that splitting the sample on the basis of growth makes for no significant difference in any of the coefficients for 1957, which is one of our sample years. It does, however, make a considerable difference in 1961. From this they conclude not that our approximation works better in 1957 than 1961, or even that it doesn't work at all in 1961; but rather that "this particular error in specification is so fundamental that it calls into question the value of the model itself as an estimating tool."

As to methodological matters, we would hope that Crockett and Friend would concede that what they call our "indirect" method is at least as valid and valuable an approach to estimating the cost of capital as their "direct" method. Theirs may perhaps have the advantage of not requiring a prior confrontation of the leverage and dividend issues; but, if this is an advantage, it is obtained only at the cost of having a less useful estimate—a single average point estimate rather than the cost of capital function derivable by our procedure. In any event, there is no necessity for researchers in this area to restrict themselves to any one method. The two approaches are complementary and anyone undertaking a study along our lines would be well advised to check his results by alternative approaches, including possibly that suggested by Crockett-Friend.

As for the problem of controlling for the measurement errors in earnings, the instrumental variable approach would appear to be a useful tool in valuation studies, though, as the RMH results indicate, there is nothing automatic about it. Those planning to take up where we left off would be wise to devote considerable thought to the selection of the instrumental variables and especially to expanding the set of such variables considerably beyond the basic few we used so that adequate tests of specifications can be made (particularly tests involving the value of shares rather than the value of the firm).

Finally there is the vexing question of growth. Here there would certainly be agreement that the particular approximation to growth potential that we used is likely to be of only very limited usefulness as we conceded in our original paper. But this is no reason to despair; our variable, after all, was about the simplest possible approximation one might make; and it is at least encouraging that nature does not seem to be so hostile as to rule out simple methods even in easy cases such as ours. There are a large number of promising possibilities and approaches still to be tried; the next step is to try them.

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The Possibility of a Social Welfare Function: Comment

In his recent article, James S. Coleman seems to assert that there is ordinarily a stable equilibrium outcome to a vote trading situation with

many decisions and many voters.¹ The assertion is based largely on analogy to a market with many goods and many traders.

Ordinarily, I claim, no such stable outcome exists. Furthermore, if there is a stable outcome with vote trading, it must be exactly the same as the outcome without vote trading! These statements are made more precise in what follows.

Consider the following situation. There are n voters, X_j , $j=1, \dots, n$, and m measures up for consideration, A_i , $i=1, \dots, m$. Each measure is completely described by a vector of utility payoffs to each individual,

$$|a_{ij}| = [a_{i1}a_{i2} \dots a_{in}].$$

Payoffs are additions to or subtractions from an individual's utility, depending on whether a_{ij} is positive or negative. Ordinarily a measure will benefit some individuals at the expense of others; that is, for a given i , at least one a_{ij} is positive and one a_{ij} negative. However, other measures are not excluded from consideration.

Without vote trading, decisions are taken as follows. Each voter X_j is given a description of all measures A_i , that is, a matrix

$$\|a_{ij}\|$$

composed of the payoff vectors for all measures. Without being allowed to communicate with any of the others, each voter records his vote for or against each measure. The votes are then counted. Each measure that receives at least $(n+1)/2$ (if n is odd) or $n/2+1$ (if n is even) "for" votes is adopted, and all other measures are rejected.

With vote trading, decisions are taken in exactly the same way except that a bargaining period follows distribution of descriptions of the measures. During the bargaining period, each voter can enter binding agreements with others as to how they all will vote on particular measures. At the close of the bargaining period, votes are cast and counted to determine which measures are adopted, and which rejected, just as before.

Definition 1: A majority set is any set of voters X_j with at least $(n+1)/2$ members (if n is odd), or at least $n/2+1$ members (if n is even).

Definition 2: An outcome is a set of measures adopted. Denote the k^{th} outcome by T_k .

Assumption 1: The payoff vector of an outcome T_k ,

$$|a_{kj}|, = \sum_{i \in T_k} |a_{ij}|.$$

That is, utility payoffs are additive for each individual.

One can therefore form the payoff matrix for all outcomes,

$$\|a_{ki}\|,$$

from the payoff matrix for all measures,

¹ [2, pp. 1117-18; also note 9, p. 1114]. Tullock makes the same assertion: "If logrolling is the norm (and it will be no secret to the reader that we think it is), then the problem of the cyclical majority vanishes" [1, p. 330].

$$||a_{ij}||,$$

by adding payoff vectors for each possible combination of measures, from none of the A_i , through any one, any combination of two or more, on up to all m .

Assumption 2: For each voter X_j ,

$$a_{kj} \neq a_{k'j},$$

for all k and k' such that $k \neq k'$. That is, no individual is indifferent as between any two outcomes.

Assumption 3: Each voter X_j acts as if he wanted only to maximize the expectation of his own utility payoff.

Assumption 4: There are no tie measures, that is A_i that benefit exactly $n/2$ and harm exactly $n/2$ voters. This assumption is automatically satisfied if n is odd. For n even, allowing tie measures would weaken the results slightly and complicate the exposition a great deal.

Theorem 1: Without vote trading, there is a unique outcome which includes all A_i that benefit any majority set and excludes all A_i that harm any majority set. I denote that outcome by T_b .

Lemma 1: Without vote trading, each voter X_j votes for any measures A_i for which $a_{ij} > 0$, and against any A_i for which $a_{ij} < 0$.

Proof: X_j acts as though he were attempting to maximize his expectation of a_{kj} (Assumption 3). Since utility payoffs for each individual are additive by Assumption 1, the way for him to do this is to maximize his subjective probability for adoption of all A_i for which $a_{ij} > 0$, and minimize the probability of adoption of all A_i for which $a_{ij} < 0$. If X_j is at all uncertain about how others might vote on A_i , his vote for A_i will maximize the probability of adoption, and his vote against A_i will minimize the probability of adoption. If, however, X_j is subjectively certain as to how others will vote on A_i , he will see his own vote as being either decisive or immaterial. If he sees his vote as decisive, he will vote for A_i if $a_{ij} > 0$ and against it if $a_{ij} < 0$. If he sees his own vote as immaterial, he may cast it randomly for or against A_i . But he cannot rationally cast his own vote randomly without admitting the possibility that others might also do so. And that brings him back to the case in which he is uncertain as to how others may vote, and votes for A_i if $a_{ij} > 0$ and against it if $a_{ij} < 0$.

Lemma 2: For each voter X_j , all a_{ij} are either $>$ or < 0 .

Proof: Included in $||a_{kj}||$ is a payoff vector for rejection of all A_i . Rejection of all A_i leaves the status quo unchanged, so these payoffs are all zeros. Lemma 2 is thus just a statement of Assumption 2 for the special case in which T_k' is the rejection of all A_i , and T_k is the adoption of each A_i individually in turn.

Lemma 1 and Lemma 2 together imply that each X_j casts a vote either for or against each A_i .

Now there are three things to prove in Theorem 1: (a) T_b is unique, (b) T_b includes all A_i that benefit any majority set, and (c) T_b excludes all A_i that harm any majority set.

To prove (a), note that by Lemma 1 the number of votes for A_i is uniquely $N(a_{ij} > 0)$, that is, the number of positive payoffs in $|a_{ij}|$. And whether or not A_i is adopted is uniquely determined by whether or not $N(a_{ij} > 0) \geq (n+1)/2$ (if n odd) or $n/2+1$ (if n even). Thus T_b is uniquely determined given $||a_{ij}||$.

The proof of (b) is trivial: If A_i benefits any majority set, $N(a_{ij} > 0) \geq (n+1)/2$ (if n odd) or $n/2+1$ (if n even) by Definition 1, and so it is adopted.

To prove (c): If A_i harms any majority set, $N(a_{ij} < 0) \geq (n+1)/2$ (if n odd) or $n/2+1$ (if n even). Since all a_{ij} are either < 0 or > 0 (Lemma 2), $N(a_{ij} > 0) \leq n - (n+1)/2 = (n-1)/2$ (if n odd) and $n - (n/2+1) = n/2-1$ (if n even) and so A_i is rejected.

I note in passing that T_b may be such that $|a_{bj}| < 0$, that is the outcome without vote trading may hurt all voters.²

Definition 3: An outcome is unstable if there exists any group of voters that individually prefer another outcome, and collectively are able to act so as to assure the preferred outcome. All outcomes are either stable or unstable, but not both.

Theorem 2: A necessary condition for an outcome T_c to be stable with vote trading is that it be identical with the outcome without vote trading, T_b .

Proof: Assume that $T_c \neq T_b$, and show that T_c is unstable.

If $T_c \neq T_b$, then at least one of the following two statements is true: (a) T_c excludes at least one A_i that is included in T_b , (b) T_c includes at least one A_i that is excluded from T_b .

Consider (a) first. The A_i that is excluded from T_c but included in T_b benefits some majority set S , by Theorem 1. Therefore, all members of S prefer $T_d = T_c \cup A_i$ to T_c , because utility payoffs are additive for each individual by Assumption 1. Further, S is able to assure T_d by voting en masse for all measures in T_d and against all measures not in T_d . Therefore T_c is an unstable outcome, by Definition 3.

The argument if (b) is true is similar. The A_i that is included in T_c but excluded from T_b harms some majority set S , by Theorem 1 and Assumption 4. Therefore all members of S prefer $T_d = T_c - A_i$ to T_c , by Assumption 1. Further, S is able to assure T_d by voting en masse for all measures in

² An $||a_{ij}||$ that would result in such a T_b is

$$\begin{bmatrix} -2n & 1 & 1 & \cdots & 1 \\ 1 & -2n & 1 & \cdots & 1 \\ 1 & 1 & -2n & \cdots & 1 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ 1 & 1 & 1 & \cdots & -2n \end{bmatrix}$$

Clearly, it makes a big difference what measures come to a vote. It might be useful to regard political decision-making as a two-step process: (1) picking, from among the infinity of multi-dimensional continua of possible measures, a finite set to present for vote, and (2) deciding whether to adopt or reject each measure presented. The second step seems to have received a good deal more analytic attention than the first.

T_d and against all measures not in T_d . Therefore T_e is an unstable outcome, by Definition 3.

Theorem 3: If the introduction of vote trading is "beneficial" in the sense that some majority set can, with vote trading, improve the payoffs to all of its members over those without vote trading, there is no stable outcome with vote trading.

Proof: The "if" clause is just a statement that the outcome without vote trading, T_b , is not stable with vote trading (Definition 3). But by Theorem 2, if T_b is not stable with vote trading, there is *no* stable outcome with vote trading.

Note: Since T_b includes (excludes) all individual measures that benefit (harm) any majority set, the "if" clause is true if and only if T_b excludes (includes) some combination of two or more measures than benefits (harms) some majority set.

To what extent have I substantiated my opening remarks? I claimed first that no stable outcome ordinarily exists with vote trading. This is a statement with empirical content that can be refuted only by examination of $\|a_{ij}\|$'s that "ordinarily" characterize vote trading situations. Theorem 3 states a condition under which no stable outcome exists, and my claim is based upon the feeling that this condition is often fulfilled.

My second opening statement, that if there is a stable outcome with vote trading, it must be exactly the same as the outcome without vote trading, is just Theorem 2. The proof of Theorem 2 substantiates the statement, given the assumptions.

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The Possibility of a Social Welfare Function: Comment

It has been possible for some time to demonstrate that under certain conditions competitive forces will achieve a socially optimal allocation of private goods in an exchange economy through the operation of the price system. What has remained an insurmountable problem, however, has been to select a socially optimal allocation of resources once public goods are introduced into the model. The field of selection usually is limited to Pareto optimal points. By definition any shift from one Pareto optimal point to another involves a loss for some members of the community and a gain for others. The chief

problem in selecting *the* socially optimal allocation of goods has been to determine what these gains and losses are, and then devise a way by which the gainers can compensate the losers. The characteristics of the public good preclude the use of the price system for making these exchanges, since it is to the advantage of every individual to conceal his true preferences for this type of good.¹ The basic problem which must be solved when dealing with public goods, therefore, is to devise a method for determining individual preferences for these goods and then on the basis of these revealed preferences select the set of public goods which is socially optimal [4, pp. 131-132].

Professor Coleman has presented a model which argues that this problem can be solved by means of a voting process, if voters are allowed to enter into vote trading agreements [2]. Unfortunately, these vote trading agreements cannot be relied upon to reveal the true preferences of individuals. This will be demonstrated in Section I. The objections to Coleman's model will be removed with a simple extension of this model, which is presented in Section II. In Section III some additional conclusions are drawn regarding both models.

I. Coleman's Social Welfare Function

The demonstration that competition will bring about a socially optimal allocation of private goods rests on a rather restrictive set of assumptions. For our purposes the two most interesting necessary conditions are: first, that all agreed-upon transactions do in fact take place. It is because goods actually must be exchanged that individuals are forced to reveal their preferences for each commodity. A consumer can only obtain additional units of one commodity by actually giving up units of some other good. Second, the number of buyers and sellers must be large enough, and the initial distributions of goods and wants wide enough, so that no individual or group of individuals is capable of colluding and achieving monopoly or monopsony power over a good. Competition among buyers and sellers will produce an allocation of private goods which is socially optimal in the sense that the marginal social benefit received from any good equals the marginal social cost of supplying it. The equilibrium set of prices will produce an allocation of goods which is uniquely determined by the initial distribution of goods and consumer tastes.²

In Coleman's model, each decision to supply a public good (along with a tax scheme to finance it) is called an *event* [2, p. 1120]. Individuals are partially able to *control* these events by voting for or against them [2, p. 1119]. They may increase their control over certain public goods by entering into agreements to swap votes. The voter agrees to give up his votes on some issues (by voting against his wishes) for the pledges of other individuals to vote as he does on an issue he considers important. These agreements should increase the expected utility gain of the voter by increasing his control over the issues

¹This follows directly from its definition as a good which *must* be supplied in equal quantities to all the members of a community regardless of their contributions to its financing. For definitions of private and public goods, see Samuelson [6]. Also, note Head's refinements on these definitions [3].

²For a complete proof of the theorem stating all the necessary conditions, see [1, Ch. 3].

which are of the greatest interest to him. Given large numbers of issues and voters, one can picture the voter entering into a set of vote exchange agreements such that the last amount of expected utility sacrificed by giving up control over his vote on some issue equals the gain in expected utility from securing control over additional votes on other issues. If all voters enter trade agreements until this condition is met, a set of decisions on public goods is obtained which may be termed socially optimal owing to its similarity to the result for the private good market.³ Voter preferences are revealed by means of the vote trading agreements into which the votes enter.

Our criticism of Coleman's model is that these trading agreements cannot be relied upon to reveal individual preferences: first, because they do not remove the incentives each voter has to conceal his true preferences—which is the basic problem with which we began; second, because no one can be expected to keep these agreements once the number of voters is very large.

The difficulty with getting individuals to reveal their preferences for public goods arises because each individual must consume equal quantities of the good once the decision is made to supply it. If an individual is then approached by the tax collector and asked to assess the value of the public good to him, he naturally will tend to understate this value. The same incentives exist when voters enter trading agreements. If voter *B* approaches *A* and asks him whether he intends to vote for issue *X*, an issue *B* strongly favors, it is to *A*'s advantage to say no even if he too is for the issue. *B* may then offer to vote with *A* on some other issues *A* favors, if *A* will agree to vote for issue *X*. Of course, *A* too will have no way of knowing whether or not *B* really is "giving up" his votes or just claiming to sacrifice them on these issues. Hence, there can be no assurance that the vote trading agreements will reveal the true preferences of the voters. They will continue to have incentives to misstate their preferences when the agreements are being struck, in order that they obtain greater bargaining power.

In demonstrating the second point, it must be noted that it is a necessary condition for achieving a socially optimal set of decisions on public goods, that the number of voters be large. Otherwise, a voter is in a monopsonist's position on any issue of great importance to him. He will be able to gain complete control (a majority of votes over the issue) without sacrificing an equivalent loss in expected utility through trading his votes on other issues [2, p. 1118]. Unfortunately, the existence of a large number of voters precludes the formation of the coalitions and agreements which are also necessary for the vote trading activities of Coleman's model.

When voting decisions are made sequentially, the temptation to violate voting agreements is large. If two voters agree to swap votes on a pair of issues, as soon as the first issue is decided one of the voters will have fulfilled his part of the bargain. The second voter then has a greatly reduced incentive to live up to his part of the agreement and vote against his own wishes. If he does de-

³ This will be demonstrated in greater detail in Section II. Although this conclusion is never actually drawn in Coleman's paper, it is implicit in his statements at the end of Section III [2, p. 1120].

cide to violate the agreement, the only way the first voter can retaliate is by refusing to enter into further agreements with him or by voting against some issue the violator favors.

With a large number of voters, no single individual's vote will be considered decisive for the outcome of a vote on an issue. It also will be nearly impossible to single out a violator of an agreement for punishment without similarly punishing other members of the coalition (who have not broken the agreement) by voting against an issue they favor.⁴ The total loss in welfare for the members of the coalition from carrying out punishment is likely to be much greater than the expected gain from disciplining a single violator. Hence, the member of a large coalition must view the probability as rather small that the other members will retaliate against any vote he takes which is against the coalition's agreements. If, under these circumstances, voters are not willing to enter into and keep vote trading agreements we are left with the problem with which we started, the problem of trying to get individuals to reveal their true preferences towards bundles of public goods by means of a voting process.

We conclude that coalitions among large numbers of voters can be expected to be unstable for the same reason that collusive agreements among large numbers of sellers are thought to be unstable. Each individual does not expect his decision to alter the decisions of the rest of the group. If they abide by the agreement and he breaks it, he stands to gain. If they do not maintain the agreement and he does, he loses. If no one maintains the agreement, he is worse off than in the first situation, and better off than in the second. Since all other participants are thought to view the situation similarly, the likelihood is that most individuals will choose to break the agreement.

Indeed, it would be embarrassing if this were not the case. The demonstration that competition will produce a socially optimal allocation of private goods rests upon the assumption that large numbers of buyers and sellers *are not capable* of maintaining collusive agreements when it is to the advantage of each individual to break such agreements. The demonstration that voting will produce a socially optimal set of decisions regarding public goods cannot be left to rest on the premise that large numbers of voters *are capable* of reaching agreements when individuals stand to gain from violating such agreements.

Fortunately, this premise is not essential for the latter demonstration as now will be shown.

II. *Exchanging Votes in a Vote-Market*

The difficulty with Coleman's model is that voters do not actually exchange votes, but only agree to trade votes and hence have an incentive for concealing their true preferences at the time these agreements are formed. Furthermore, with a large number of voters the incentives to violate these agreements will

⁴The "nearly" may be dropped if voting is by secret ballot, since it then would be impossible to identify a violator. While Coleman's model is thus obviously limited to groups which vote by roll call, this is not the case for the model presented below in Section II.

be so large that few voters can be expected to abide by them. The solution is obviously to replace the set of agreements with a system in which votes literally are exchanged. That is, assume every voter has the right to cast one vote for each issue. He may either exercise his right to vote on a given issue or trade it to someone for the latter's right to vote on one or more issues. These transactions could be conducted in vote-markets in which proxies were exchanged. One might envisage that at first the votes on each pair of issues would be traded in a separate market. In time, the votes on one issue undoubtedly would become a medium of exchange and all purchases and sales of votes would be made for votes on this issue. Conceivably this even might be a token vote-money (e.g., a vote on whether or not the marigold is designated as the national flower). A set of vote-prices would be established for each issue, probably in terms of units of vote-money.

More formally, let

U_{ij} = the absolute value of the change in utility for individual j attached to the i^{th} issue,

c_{ij} = the number of votes j holds on issue i ,

$p_{ij} = f_{ij}(c_{ij})$ = j 's subjective probability that the outcome of the vote on issue i will be as he wishes, and

P_i = the vote-price on issue i in units of vote-money.

The U_{ij} are absolute values of the utilities because it is assumed that the voter is indifferent between voting for an issue which increases his welfare and voting against an issue which decreases it to the same degree. The probability of success (p_{ij}) is assumed to be a monotonically increasing function of the c_{ij} up to the point where j possesses 50 per cent of the votes on issue i (where it becomes 1.0). It is also essential that, within the increasing stretch of the curve, the second derivatives of these functions be negative. This is the analogue of the diminishing marginal utility assumption in consumer theory. It implies that successive increases in an individual's stock of votes on a given issue make smaller and smaller additions to his subjective probability of success. The assumption is plausible for a stretch of the probability schedule which just precedes the point at which the voter owns 50 per cent of the votes on an issue. Whether it also is true for the beginning portions of the curve is more debatable.⁵ We shall not take time to settle the question of increasing marginal probability here, however, but will simply assume that the f'' are everywhere negative.

If we further assume that each individual maximizes his expected net utility gain from public goods subject to the constraint imposed by the initial egalitarian distribution of votes, then (1) becomes the equilibrium condition for every voter j and every pair of issues i and k . The vote-market equivalent of the marginal rate of substitution is the ratio of the change in the subjective probability of success on issue i to the change in the subjective probability of success on issue k , each multiplied by the utilities attached to their respective issues.

⁵ For example, the assumption implies that at no other point on the curve will the voter's subjective probability of success be increased by the acquisition of another vote by an amount as great as his original probability of success when he has only one vote.

$$(1) \quad \frac{U_{ij} f'_{ij}(c_{ij})}{U_{kj} f'_{kj}(c_{kj})} = \frac{P_i}{P_k}$$

This will be called the marginal rate of probability substitution (MRPS). In equilibrium the MRPS for issues i and k will equal the ratio of the vote-prices for these two issues. A fall in the vote-price of issue i relative to the vote-price of k will induce the voter to increase the number of his votes on issue i (thereby reducing the marginal increase in the probability of success of an additional vote on i) and decrease his votes on k (thereby increasing f'_{ij}). This exchange of votes on k for votes on i will continue until the MRPS has been brought into equality with the lower vote-price ratio for i and k .

The similarities between this result for the vote-market for public goods and the traditional solution for the private good market should be obvious. First, the number of voters and issues must be large enough so that no individual or small group of individuals can obtain monopoly or monopsony power. Second, it is necessary that votes *actually are exchanged* if the true preferences of each voter are to be revealed. Third, among the set of all possible combinations of public goods the equilibrium set of vote-prices will determine a combination which is Pareto optimal.⁶ Finally, the exact combination of public goods chosen out of the set of all possible Pareto optimal combinations will be uniquely determined by: (1) the initial distribution of votes, (2) the voter preferences toward the public goods (U_{ij}), (3) the subjective probability schedules of the voters regarding each issue (f_{ij}), and (4) the voting rule employed.⁷

Before further comparing Coleman's model with this one, a brief look must be taken at the enigmatic question of income distribution. When discussing the proper mix of public and private goods, most analysts have followed Wicksell and assumed an equitable distribution of income is given at the start.⁸ Since few students of public finance are now willing to allow the initial income distribution to be determined by market forces alone, the proper redistribution of income also will have to be decided by voting. In terms of our model this means decisions on income distribution must be included as a subset of the set of all voting issues. An individual would be able to swap his votes on various aid-to-education programs for votes on redistributive tax schemes. He could increase his power to select a favorable distribution of in-

⁶ If all voters benefit from a given public good-tax combination, it already is included in the subset of chosen public goods. The addition of any public good not already in this subset, therefore, must involve a loss of welfare for at least one voter. For the initial subset not to be Pareto optimal this voter must be compensated for his loss with votes on some other issue. But if it is possible for a group of voters to exchange votes and have no voter worse off and at least one voter increase his welfare, then the original set of vote-prices could not have been the equilibrium set.

⁷ A switch from simple majority voting to a $\frac{2}{3}$ majority rule would change the voter subjective probability schedules and most likely would require a new set of vote prices for equilibrium.

⁸ This is the approach R. A. Musgrave favors [4, pp. 84-86].

come, but only by giving up his right to affect the decision on the level of government supported education *and the method by which it is financed*. Hence, in our model allocation and redistribution decisions would have to be made jointly. In this sense, the approach resembles that of Samuelson [6].⁹

III. *Some Further Thoughts on the Two Models*

On the basis of Coleman's analysis and the criticisms lodged against it here, one can conclude that, when voters are able to make and keep vote trading agreements, their welfare will be greater than if no agreements were made. On the other hand, the resulting set of policy decisions will fall far short of being in any sense socially optimal. If the number of voters is not so large as to preclude the formation of partially stable coalitions, it is too small to remove completely the monopsony power a voter will be able to enjoy over any issue of vital importance to him. Hence, logrolling agreements among legislators probably are the socially suboptimal set of decisions they long have been thought to be.

One must even qualify a conclusion, that the welfare of all voters is increased when the voters enter trading agreements. First, it is impossible to judge the extent to which the set of voting decisions is driven away from the Pareto optimal set, because the voters mis-state their preferences in order to increase their bargaining strength. A further element of uncertainty is present because of the instability of coalitions. This will result in an unpredictable number of violations of the trading agreements with an unmeasurable impact on the welfare of all the voters.¹⁰

For voter preferences to be revealed by means of vote-markets, voters would have to be able to trade votes on all important issues decided by the government, i.e., all of the issues currently decided in both the legislative and executive branches. This would place an impossible burden on the voter who attempted to undertake the amount of research which would be required for him to make rational vote trading decisions. Nor is the problem for him reduced greatly if a representative form of government is assumed. Even if the representatives are able to carry out their vote trading activities in an informed manner, the voter would have the difficult task of evaluating the alternative candidates for Congress on the basis of the sales and purchases of votes

⁹ Musgrave's separation of a Stabilization Branch of the government also would have to be abandoned in the context of this model [4]. Some people benefit from inflation, others lose. Decisions regarding stabilization policy would have to be decided by voting, and votes on this issue would be traded like votes on any other issue in the vote-markets.

When compared with the "ideal" procedure for selecting government policies as depicted here, Musgrave's Distribution, Allocation and Stabilization Branches appear to be, rather ironically, of greater practical value for evaluating actual government policies, than of theoretical interest as a schema which provides the format under which government decisions "should" be made.

¹⁰ For example, it is possible that a public good which would benefit all voters would not be approved, if a majority of them decided to vote against it as a means of punishing violators of previous agreements.

M. Olson compares the decision-making process of groups of coalitions to the price-making models of oligopoly theory and therefore regards the nature of the final decisions as a priori indeterminate [5, p. 43, n.].

they promised to make (or had in the past made) in the legislative vote-markets.

Viewed in this perspective, some of the criticisms of Coleman's model may appear unjust, for it has been formulated with the actual operations of the legislative process in mind and is certainly more attuned to the method by which government policies are in fact selected [2, p. 1116]. On the other hand, Coleman does stress the theoretical contribution of his model [2, p. 1121-22], and does draw the conclusion that a socially optimal set of public goods would be selected by means of the voting process, if voters were free to enter into trading agreements and the numbers of issues and voters were very large [2, pp. 1118-20]. This conclusion we have had to reject for the reasons given above, and hence must prefer, when judged on theoretical grounds alone, the vote-market model to Coleman's vote trading agreement model.

In developing the vote-market model we have chosen not only to abandon the real-world context in which Coleman's model was formulated, but also to employ a slightly different set of concepts and symbols from those Coleman introduced [2, p. 1119-20]. This was done so that the model could be developed as analogously as is possible to the competitive model of consumer theory. In doing this, we feel we have remained true to the major point Coleman wanted to make, "that, just as a free market with pure competition can be conceived in economic change, and used as a theoretical model from which actual systems can be examined, a similar model of pure competition can be conceived in collective decisions. Although actual social systems deviate from this model, it can nevertheless serve as a basis from which the deviations can be studied" [2, p. 1118].

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The Possibility of a Social Welfare Function: Reply

R. E. Park argues that no stable outcome exists in a vote trading situation unless it is the outcome that would occur without trading, and purports to prove this argument in his Theorem 2.

The central point in his argument can be shown by a simple example. Consider two issues and three voters, with utilities a_{ij} given by the table below:

| Voter | Decision | |
|-------|----------|------|
| | I | II |
| 1 | -1.0 | -1.0 |
| 2 | +1.9 | -0.1 |
| 3 | -0.1 | +1.9 |

A direct vote on the two issues without exchange will lead to rejection of both actions. Thus, Park argues that, if there is a stable outcome, it must be this outcome, which we will label $--$. But now we let vote trading take place, and assume that voters 2 and 3 make an exchange, agreeing to vote positively on both decisions. Park's argument can best be shown by evaluating all four possible outcomes, or each of the three voters.

| Voter | Outcome | | | |
|-------|---------|------|------|------|
| | $++$ | $+-$ | $-+$ | $--$ |
| 1 | -2.0 | -1.0 | -1.0 | 0.0 |
| 2 | +1.8 | +1.9 | -0.1 | 0.0 |
| 3 | +1.8 | -0.1 | +1.9 | 0.0 |

His proof of Theorem 2 is carried out through assuming the stability of an outcome other than $--$, arising through vote trading (in this case $++$), and then showing that it is in fact unstable. He shows, in effect, that a majority can be found who prefer a different outcome that is one decision closer to the no-trading outcome. In this case, that is illustrated by the $+-$ outcome, in comparison to the $++$ outcome. (The $-+$ outcome could be just as well used.) Voters 1 and 2 prefer the $+-$ outcome to the $++$ outcome, so that, according to the argument, they, acting as a majority, will reject the $++$ outcome, and take instead the $+-$ outcome. But then the process is iterative, so long as the outcome is not $--$: voters 1 and 3 prefer the $--$ outcome to the $+-$ outcome, and therefore as a majority, reject the $+-$ outcome.

However, it is the very instability of the new outcome, once the first agreement is broken, that raises questions. Consider the situation from the point of view of voter 2, once an agreement has been made with 3. By breaking this agreement, and making a new agreement with 1 (to vote $+-$), he stands to have a utility of 1.9, rather than 1.8, but only with that probability equal to his belief that 1 will not break this agreement to agree with 3 to vote $--$, in which case 2 would have 0.0 utility.

In short, he must assess the expected utility of two exchanges, the one he has, and the one he might make instead. If his belief that 3 will hold to the $++$ agreement is slightly higher than his belief that 1 will hold to the new agreement, then he will find the expected utility of the $++$ agreement greater. And there is reason to expect such a difference in subjective probabilities on 2's part: 1 has more to gain by breaking the $+-$ agreement than does 3 by breaking the $++$ agreement. The complete calculation of expected utilities would involve an infinite regress of subjective probabilities of others' actions, perceptions of the subjective probabilities they hold, and

so on, including estimates of his own probabilities of breaking an agreement. However, a truncation of this calculation would show that under plausible assumptions (i.e., that 1 has considerably more to gain than either himself or 3 by breaking the agreement in question), then 2 (and 3 as well) has higher expected utilities under the + + agreement than under any other.¹

If, then, stability of a given outcome is defined not in terms of a determinate utility calculation of voters, acting under the assumption that a given agreement will be implemented, but in terms of expected utilities of voters estimating the probabilities of broken agreements, Park's Theorem 2 does not hold. Furthermore, since Park's theorem depends upon first making and then breaking agreements, it is inconsistent with a utility calculation that evaluates the outcome under an agreement as if it had zero probability of being broken (which is equivalent to the utility-under-certainty calculation implied in his statement that "all members in S prefer T_d to T_c ", and the unstated assumption that they will therefore not vote T_c).

Park's theorem follows from his assumption about voters, and his consequent definition of stability. However, it is these assumptions, and the stability definition that follows from them, that I suggest constitute an inappropriate definition of rationality in the complex situation raised by the making and breaking of vote agreements. His theorem shows that a different definition of stability is necessary for voting situations involving vote exchanges. The definition of stability must instead be something like this: An outcome in a voting situation that allows vote exchanges is unstable if there exists a majority, each of whose expected utility calculations for a different outcome is greater than that for the outcome in question, taking into account the probability of agreements being broken.

But this is not a very satisfactory result, for I have given no general procedures for calculating these expected utilities that take into account the probability of broken agreements. It appears clear that such calculations, however crudely made, are the basis for the stability that often (but not always) occurs in voting situations involving vote trading; but it is not clear just how to develop a model that appropriately generates these subjective probabilities isomorphic with the way a rational voter would arrive at them.

It is useful to note that by an argument almost identical to that of Park, one could show that the only equilibrium in a private goods market is the one in which all individuals left the market with exactly those goods they entered it with. Excluding individual 1 from the above example, we arrive at the usual exchange situation in a private market. Suppose decision I is "individual 3 giving up his good to individual 2," and decision II is "individual 2 giving up his good to individual 3." Utilities appropriate to an exchange situation would be those given earlier:

¹ It is useful to note that in a 3-person game in which the division of 1 unit is to be determined by a majority vote, the Von Neumann-Morgenstern solution set for the game consists of the three permutations of the outcome $(0, \frac{1}{2}, \frac{1}{2})$. As J. M. Buchanan has reminded me, the rationale for this is precisely the rationale for 2 and 3 maintaining their trade in the example given in the text. The dominance of this outcome over a division $(\frac{1}{3}, \frac{2}{3}, 0)$ lies in the fact that this illusory temptation of 2 by 1 would likely be followed by a defecting agreement of 1 with 3 to give $(\frac{1}{2}, 0, \frac{1}{2})$. See [1, Sec. 22.1].

| Trader | Decision | |
|--------|----------|------|
| | I | II |
| 2 | 1.9 | -0.1 |
| 3 | -0.1 | 1.9 |

Assume that either individual can reject any positive action (i.e., a unanimity rule is required for an action). If we consider again the four outcomes $++$, $+ -$, $- +$, and $--$, then the utilities are:

| Trader | Outcome | | | |
|--------|---------|-------|-------|------|
| | $++$ | $+ -$ | $- +$ | $--$ |
| 2 | 1.8 | 1.9 | -0.1 | 0.0 |
| 3 | 1.8 | -0.1 | 1.9 | 0.0 |

As before, individual 2 prefers outcome $+ -$ to $++$, so that he can reject the $++$ outcome in favor of $+ -$, but then 3 can reject that in favor of $--$. Thus no exchange would occur.²

In this case of two-party exchange of private goods, the absence of equilibrium occurs because each party has the power to break an exchange agreement. He cannot ordinarily (though he can in cases of delayed physical transfers) realize the outcome he most prefers (gaining without giving up anything), because the other party has veto power. But equally, in the voting case, he has no assurance of being able to realize the outcome he most desires, because it is in the interest of others to break the new agreement. Park's Theorem 2, although it does not prove the instability it purports to prove, for the reasons given above, does show that stability of exchange in voting systems may be more difficult to reach. The theorem says, in effect, that there is always a set of voters interested in offering to a member or members of one side of an effective exchange (an exchange which can control the vote outcome) all that he has, or they have, under the present exchange without the cost it involves.

In private markets as well as in voting systems, there must be security measures to insure the equilibrium that would otherwise be unstable. Perhaps the most important in both cases is the continued existence of the collectivity, which allows persons who have suffered from broken agreements to punish offenders by refusing further trades. In private markets, the institution of escrow is another safeguard, and laws regarding contracts are a third. In legislatures, a system of standing political credit, with a party leader or floor officer

²The exchange of private goods is in fact a special case of the prisoner's dilemma, where the two alternatives for each party are to keep the good or give it up. The matrix in this example would be:

| | Keep | Give up |
|---------|------------|------------|
| Keep | 0 / 0 | -0.1 / 1.9 |
| Give up | 1.9 / -0.1 | 1.8 / 1.8 |

Without some mechanism of trust or enforcement of the agreement, the only rational action for each individual is to keep his good.

acting as the central intermediary, often exists. Another possible institution, which does not exist in most voting systems, is that of physical exchange of vote proxies, proposed by Dennis C. Mueller.

Mueller in his comment raises problems, all of which stem from the fact that a collective action is a public good. Thus if a vote is conceived as a resource that can be exchanged, it too is a public good, since it constitutes some fraction of control over the collective action which is a public good.

Mueller suggests that this fact leads a voter A to misrepresent voting intentions, so as to trade off a vote to a voter B who will use it as A would have done (thus providing no benefit to B, and no cost to A). He suggests also that the absence of physical exchange (and not the public-good nature of the vote) will lead to wholesale breaking of agreements.

The latter point is not difficult, I believe, to dispose of. In a collectivity with a sequence of collective actions or events extending indefinitely into the future, any violation of an agreement need not be punished, as Mueller supposes, by a coalition's voting against an outcome favored by the offender. It can instead be punished merely by the refusal to engage in further agreements. It becomes greatly to the interest of others also to learn of such defections so as to avoid agreements with unreliable people, thus extending the punishment. Just as in private-goods markets where goods are not physically transferred simultaneously, some means of insuring payment is necessary. Because the collectivity extends into the future, such potential punishment can serve just as well in vote markets as in private-goods markets.

However, the misrepresentation of a voting intention is a serious difficulty, no different in nature than the understatement of benefits that would invalidate Wicksell's taxation voting scheme. There is no reason, however, to suppose that the physical exchange of a vote proposed by Mueller would itself prevent that. What would prevent it is physical exchange plus negotiability of the exchanged vote. Thus it is not enough for A to be able to sell his proxy to B for B's proxies on other issues; what is necessary is for B to be able to trade off that proxy in turn. If this negotiability existed, then when A sells his vote, he has no knowledge of how it will be used, and must assume a cost to him proportional to his interest in the collective action (i.e., proportional to U_{ij} , the absolute value of the utility difference between outcomes). With this amendment then A is no longer able to profit by misrepresenting his intentions. This I see as a major step forward in the removal of "public-goods effects" from collective actions. The resulting system of vote markets is, as Mueller indicates, wholly in accord with my major aim, that of developing a theory of pure competition in collective decisions comparable to that of the perfect market for private goods. (I would also expect such full negotiability of votes to constitute a practical advance toward achieving social optimality in actions of real legislatures.)

However, this is unfortunately not the only way the public-good property of a collective action affects vote markets. There is a second problem. This arises from the fact that a vote exchange is made on the basis of the preferences of the two parties to the exchange; but because the votes are public goods, the exchange has consequences for the other voters as well. This is

made evident by using Mueller's equation (1), which gives the marginal rate of probability substitution. Assume first that an individualistic equilibrium is reached, so that this marginal rate of substitution is for all individuals equal to the ratio of vote prices as indicated in equation (1). But now two voters, j and h , who are on the same side on actions i and k communicate, so that for each the probability that the outcome of the vote will favor his interests becomes $f(c_{ij} + c_{ih})$ and $f(c_{kj} + c_{kh})$. This changes the marginal rate of probability substitution for each, and leads each to seek out more votes on one or the other issue. If a number of persons communicate, this upsets the equilibrium of vote prices. Thus any description of the perfect system of vote exchanges, with Mueller's modification of it, is valid only in the case where individuals act wholly individualistically, and have no knowledge of vote commitments obtained by others. Even in this case, it is not perfectly clear that the externalities involved in every vote exchange are eliminated by the assumption of a perfect (individualistic) market.

Still another problem exists. The use of a marginal rate of probability substitution in markets for private goods implies, as Mueller indicates, that the marginal probability of success due to a vote commitment decreases with the number of votes controlled. However, I have investigated the shape of this marginal curve under very plausible assumptions, and it is clear that under most conditions it does not decrease. The shape of the curve depends upon the votes controlled by opponents and allies, and it is only in the case where the votes controlled by opponents is zero that the marginal probability everywhere decreases. Indeed, when the number of votes controlled by the opposition increases at the same rate as one's own, the marginal probability of a vote commitment *increases* as the number of votes controlled increases.

This can lead to behavior quite different from that which occurs in private markets. Rather than buying a "market basket" of votes for different decisions, an individual may use all his resources for the decision which interests him most. And the normal functioning of the price mechanism to establish a new equilibrium would not work in the same way.

For such analysis as this, a useful decision rule to examine at the outset is the probabilistic rule I discussed in my paper. In this decision rule, the probability of success is proportional to the number of votes for and against the action. This is the only decision rule in which a voter can reasonably assess the marginal probability of success due to a vote commitment, independently of the distribution of vote commitments pro and con held by others. With this decision rule, the marginal probability is always $1/n$. This means that the marginal rate of substitution for a voter between actions i and k is always U_{ij}/U_{kj} , and he will always buy i rather than k if this ratio exceeds the ratio of prices, never reaching an equilibrium. But the effect of this behavior upon the establishment of a set of prices is itself unclear. It may be that a set of equilibrium prices could not become established under such conditions of nondecreasing marginal utility curves.

An added complication arises when markets are mixed, that is, when some of the resources that are used to buy votes are private goods, and some are public goods in the form of votes. In a legislature with committee structures

and privileges of various sorts that can be dispensed by one person for the private use of another, and personal favors, such a mixture exists. When private goods, such as personal favors or money, can be exchanged for a vote commitment, then the problem of paying the cost of a public good arises. For if a vote commitment is purchased, to be cast in favor of a collective action, then all who favor the action gain, but only the individual who purchases the commitment pays the cost. As Olson's work implies, this can lead to failure to purchase vote commitments which are too costly for one individual to purchase, but which would provide a benefit to each if costs were appropriately allocated.

If all the goods used in exchange are votes in actions of collectivity, the problem of paying the cost of obtaining a vote commitment is less serious, though it still exists. If A gives up a vote on one action for a vote on a second, he incurs a utility loss in loss of control of the first votes. He is paying the cost of the public good, but it is a public cost, since the vote he gives up is a public good. It is a cost experienced by all those who agree with him on the action whose vote he gave up, just as there is a gain experienced by all those who agree with him on the action whose vote he gained. Thus there is a sharing of costs just as there is a sharing of benefits of the exchange. But those who oppose him on the first action but agree with him on the second action experience a gain in both actions, and those who agree with him on the first and oppose him on the second experience a loss on both. Thus we cannot say that there is a sharing of costs and benefits, as is necessary in the rational provision of a public good.

Altogether, I have no quarrel with Mueller, and fully accept his modification of the theory I proposed in my paper (with the amendment about full negotiability of vote proxies). However, there remain a number of difficult problems that neither he in his comments nor I in my paper have addressed, as I have indicated in my comments above. I believe these are not insoluble, so that we will in time have a precise theory of perfect vote markets.

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The Economic Status of American Economists, 1966: A Preliminary Report

Thanks to the cooperation of the members of the profession in responding to the last two National Register questionnaires it is now possible to summarize certain trends in the structure of economists' salaries and employment between 1964 and 1966.¹ For example, preliminary comparison of the data

¹ Economists were included by the National Science Foundation as part of the biennial

gathered during these two years reveals at least ten features of general interest:

1. The average (median) salary of economists rose by 9 per cent, from \$12,000 in 1964 to \$13,000 in 1966.

2. The \$13,100 median salary of the economists was the highest reported for any profession, the most nearly similar medians being those for statisticians (\$12,800) and for physicists (\$12,500).²

3. The largest percentage increases in salary were obtained by those economists at the higher salary levels, with the result that the range of salaries became even wider in 1966 than it had been in 1964.

4. Graduate degrees are becoming increasingly numerous among economists and those with a Ph.D. degree obtained a larger increase in median salary (11.6 per cent) than those with lesser academic degrees.

5. The economics profession is experiencing a rapid influx of new members, as is indicated by the high rates of increase in the numbers of economists with less than ten years of professional experience.

6. While levels of salary continue to be strongly related to length of professional experience, the largest *increases* in median salary were generally obtained by those with relatively short periods of previous experience.

7. Educational institutions and the state and local governments were the two types of employer that showed the greatest rates of increase in the numbers of registered economists as well as the largest increases in median salaries.

8. Research activity (including industrial as well as academic research) engaged an 18 per cent larger number of registered economists in 1966 than in 1964, as compared with an increase of only about 7 per cent in the number of economists engaged in teaching and in the total of all activities.

9. In spite of the influx of new personnel, the median salary of those engaged in research rose by 9.7 per cent, or by slightly more than the average for economists as a whole.

10. The relatively low median salary of economists engaged in teaching and the relatively high median salary of those engaged in management both rose at the slightly less-than-average rate of about 8 per cent between 1964 and 1966.

This brief note on the highlights of the 1966 data is to be followed by a more detailed report in a forthcoming *Supplement* to the *American Economic Review*, which will contain a series of analyses of external and internal relationships of both salaries and incomes of economists in 1964 and 1966.³ To provide the data necessary to update changes in the employment and salary structure of economists, members are urged to respond to the forthcoming 1968 National Register questionnaire which will be circulated, as before, through the American

compiled National Register of Scientific and Technical Personnel for the first time in 1964, and the first detailed report on the data relating to economists was prepared by the Committee on the NSF Report on the Profession, N. Arnold Tolles (chairman), Ewan Clague, and Alice Hanson Jones, and published by the American Economic Association [2].

² Cf. [1, p. 2]

³ Acknowledgements to those guiding and assisting the author in these analyses will be found in the forthcoming *Supplement*. Meanwhile, the author will endeavor to respond to inquiries regarding the National Register data on economics.

TABLE 1—DISTRIBUTION OF ECONOMISTS' SALARIES, 1964 AND 1966

| | Salary (\$000's) | | Per cent Change 1964-1966 |
|----------------------|------------------|------|------------------------------|
| | 1966 | 1964 | |
| Median | 13.1 | 12.0 | 9.2 |
| Lowest decile | 8.5 | 7.8 | 9.0 |
| Lower quartile | 10.1 | 9.3 | 8.6 |
| Upper quartile | 17.6 | 16.0 | 10.0 |
| Highest decile | 23.0 | 20.0 | 15.0 |
| Inter-quartile range | 7.5 | 6.7 | 11.9 |
| Inter-decile range | 14.5 | 12.2 | 18.9 |

Economic Association and other cooperating associations.

Summary of Salary Distributions. The summary in Table 1 shows how the range of economists' salaries became even wider in 1966 than in 1964.

Salaries of the lowest paid one-tenth of the economists advanced, between 1964 and 1966, at about the same rate as did the median salary (9.0 per cent), but the lower paid quarter showed a less-than-average advance (8.6 per cent), and in 1966 the lowest decile and lower quartile salaries of the economists were actually lower than the corresponding salaries of the statisticians (\$8,700 and \$10,200, respectively).⁴ In contrast, even in percentage terms, the increases in the higher salaries were greater than the percentage increase in the median salary. Particularly striking was the rise in the salary of the highest paid one-tenth of the economists who had salaries of \$23,000 or more in 1966, which was 15 per cent greater than the \$20,000 or more received by the corresponding group in 1964. Thus the lowest salary of the highest paid one-tenth of the economists in 1966 was \$14,500 above the highest salary of the lowest paid one-tenth of them. This inter-decile range of \$14,500 in 1966 contrasted with the corresponding range of \$12,200 in 1964.⁵

Academic Degree. The increasing importance of graduate degrees in the economics profession and the superior salary increase for those with the Ph.D. degree are shown in Table 2.

In two years, 1964-66, the number of Ph.D.'s in the profession increased by 10 per cent and the number of master's decreased by 11 per cent, whereas there was practically no change in the numbers of economists with bachelor's degrees. Thus 85 per cent of the registered economists in 1966 had earned a graduate degree (11,000 of 13,000). The reasons for the relatively low level of salaries of those with master's degrees (\$12,000 in 1966) and the relatively high salary levels of those with bachelor's degrees or less (\$14,700 and \$16,500 respectively in 1966) have been described in an earlier report [2, pp. 34-38, 46]. Between 1964 and 1966, the relatively low median salary of those with master's de-

⁴ Cf. [1].

⁵ In 1966 the inter-decile range was 1.11 times the median, whereas in 1964, it was only 1.02 times the median.

TABLE 2—ACADEMIC DEGREE AND SALARY INCREASES OF ECONOMISTS, 1964-1966

| Academic Degree | 1966 Register | | Per Cent Changes 1964-1966 | |
|------------------------|---------------------------------|--|-------------------------------|-------------------------------|
| | Number Registered (000's) | Median Salary ^a (\$000's) | Number Registered | Median Salary ^a |
| Total reporting degree | 13.0 | 13.1 | 9.0 ^b | 9.2 |
| Ph.D. | 5.6 | 13.5 | 9.9 | 11.6 |
| Master's | 4.6 | 12.0 | 10.8 | 9.1 |
| Bachelor's | 2.7 | 14.7 | 1.8 | 8.9 |
| Less than Bachelor's | 0.1 | 16.5 | -13.3 | -1.2 |

^a Among employed respondents reporting salary and degree (11,079 in 1966 and 9,883 in 1964).

^b Derived from exact, rather than rounded, numbers (12,989 in 1966 and 11,998 in 1964).

degrees rose by 9.1 per cent or about the average per cent increase for all economists, while the relatively high median of those with a bachelor's degree showed a below-average rise (8.9 per cent) and the median salary of the small group without a college degree actually fell (-1.2 per cent). Economists with Ph.D. degrees, on the other hand, enjoyed an 11.6 per cent increase in average (median) salary over the two years, 1964-66.

Professional Experience. The variance of economists' salaries in 1964 has been shown to be more closely associated with length of professional experience than with any of the other six characteristics of economists thus far analyzed [2, pp. 64-70, 43-51]. While detailed analysis of the 1966 data is not complete, Table 3 does show the regular upward progression of economists' median salaries with lengthening professional experience up to 29 years of such experience (col. 2).

In 1966 economists with less than two years' experience had a median salary of \$9,000 whereas those with 20 or more years of experience had a \$17,400 salary median. The fuller detail available for 1966, but not for 1964, shows that average salaries contrived to rise with increased experience up to a median of \$18,200 for those with 25-29 years in the profession. However, economists with 30 or more years of experience earned somewhat smaller average salaries than did the 25-29 year group (\$18,000 and \$17,500 respectively).

The high rates of increase in the numbers of economists with less than ten years of experience between 1964 and 1966 appear to show that the profession has been receiving a considerable influx of new members (cf. col. 3, Table 3).^c Indeed, a considerable turnover among economists seems to be taking place. Thus there appears to have been an actual decline of 11.5 per cent between 1964 and 1966 in the number of economists with 10 to 14 years' previous ex-

^c This and all other conclusions which are based on relative numbers of registered economists in 1964 and 1966 may be subject to variations in the rate of response to questionnaires in the two years—a potential influence which has not yet been tested.

TABLE 3—PROFESSIONAL EXPERIENCE AND SALARY INCREASES OF ECONOMISTS, 1964–1966

| Years of Professional Experience | 1966 Register | | Per Cent Changes 1964–1966 | |
|----------------------------------|---------------------------|--------------------------------------|----------------------------|----------------------------|
| | Number Registered (000's) | Median Salary ^a (\$000's) | Number Registered | Median Salary ^a |
| Total reporting experience | 12.5 | 13.1 | 11.0 ^b | 9.2 |
| Under two years | 0.6 | 9.0 | 54.3 | 15.4 |
| 2–4 years | 1.7 | 9.3 | 21.0 | 10.7 |
| 5–9 years | 2.2 | 11.0 | 12.9 | 10.0 |
| 10–14 years | 1.8 | 13.0 | –11.5 | 8.3 |
| 15–19 years | 2.1 | 15.0 | 24.4 | 11.1 |
| 20 years and over | 4.1 | 17.4 ^c | 8.3 | 8.8 |
| 20–24 years | 1.2 | 16.1 | — | — |
| 25–29 years | 1.1 | 18.2 | — | — |
| 30–34 years | 0.8 | 18.0 | — | — |
| 35–39 years | 0.5 | 17.5 | — | — |
| 40 years and over | 0.5 | 17.5 | — | — |

^a Among employed respondents reporting salary and years of experience (10,806 in 1966 and 9,688 in 1964).

^b Derived from exact, rather than rounded, numbers (12,509 in 1966 and 11,269 in 1964).

^c Medians of separate experience in groups shown on the following lines, weighted by corresponding numbers shown in column 1.

perience and the rates of increase of those with 20 or more years' experience were below the average increase in the numbers in the profession. The more experienced economists evidently were not being replaced as they died, retired or left the profession. On the other hand, the rates of increase in the net numbers of the least experienced members were very high, ranging downward from a 54 per cent increase of (the small number) of those with less than two years of professional experience to a 13 per cent increase in the net number of those with five to nine years of experience.

Taken by itself, the fact that the economics profession was less experienced in 1966 than in 1964 would have tended to reduce the average rise in economists' salaries. However, it was the least experienced members of the profession who received the larger salary increases between 1964 and 1966. As compared with the 9 per cent overall rise in economists' salaries, the entering group with less than two years experience had a 15 per cent higher median salary in 1966 than did the same experience group in 1964, while those with from two to four years' experience received an 11 per cent higher median salary in 1966 than did those with similar length of experience in 1964. While it is still true that salaries are strongly related to length of experience, this positive relationship was somewhat weakened between 1964 and 1966 by the above-average salary increases provided to the relatively new recruits to the economics profession.

TABLE 4—TYPE OF EMPLOYER AND SALARY INCREASES OF ECONOMISTS, 1964-1966

| Type of Employer | 1966 Register | | Per Cent Changes, 1964-1966 | |
|------------------------------------|---------------------------------|--|--------------------------------|-------------------------------|
| | Number Registered (000's) | Median Salary ^a (\$000's) | Number Registered | Median Salary ^a |
| Total registered | 13.1 | — | 8.3 | — |
| Total report specified employer | 12.0 | 13.1 | 7.2 ^b | 9.2 |
| Educational institutions | 5.6 | 11.1 ^c | 11.2 | 7.8 ^d |
| State and local governments | 0.5 | 13.0 | 78.1 | 11.1 |
| Federal government | 1.3 | 14.7 | 6.8 | 7.3 |
| Industry and business | 4.1 | 15.3 | 3.6 | 6.3 |
| Non-profit organizations | 0.5 | 16.2 | 0.4 | 8.0 |

^a Among employed respondents reporting salary and type of employer (11,145 in 1966 and 9,941 in 1964).

^b 11,976 in 1966 and 10,922 in 1964.

^c Estimated from a weighted average of the \$10,500 median salary received by 2,953 who reported 9-10 month salaries, and the \$13,000 median salary received by 1,609 who reported 11-12 month salaries (excluding 1,037 respondents who did not report salary or did not report whether salary was for 9-10 or for 11-12 months).

^d Derived from estimate in column 2 and recorded median of \$10,300 in 1964.

Type of Employer. The greatest rates of growth in the employment of economists and the largest increases in median salaries, between 1964 and 1966, occurred in educational institutions and in state and local governments, as is shown in Table 4.

The employment of economists by educational institutions increased by 564 or 11.2 per cent while the relatively low median salary of this group rose at the below-average rate of about 8 per cent. State and local governments, although small employers of economists, increased their employment of members of this profession by 215, which represented a 78 per cent increase, while raising the median salary of their economists by 11 per cent between 1964 and 1966. The Federal government, private industry and non-profit organizations each paid the higher median salary, both in 1964 and 1966, but over the two-year period, these better paying types of employment all experienced below-average rates of growth of economists' employment and below-average rates of income in the median salaries of economists.

Work Activity. The outstanding increase in economists' employment between 1964 and 1966 consisted of employment on research, as is shown by Table 5.

The employment of economists on research increased by 18.3 per cent over the two years, 1964-1966. The salary inducements to engage in research are suggested by the slightly higher-than-average (9.7 per cent) increase in the median research salary which occurred in spite of the dampening effect on salary of

TABLE 5—WORK ACTIVITY AND SALARY INCREASES OF ECONOMISTS, 1964-1966

| Work Activity | 1966 Register | | Per Cent Changes, 1964-1966 | |
|----------------------------------|---------------------------------|--|--------------------------------|-------------------------------|
| | Number Registered (000's) | Median Salary ^a (\$000's) | Number Registered | Median Salary ^a |
| Total registered | 13.2 | — | 8.3 | — |
| Total reporting work activity | 12.0 | 13.1 | 6.5 | 9.2 |
| Teaching | 3.7 | 10.5 ^b | 7.2 | 8.2 ^c |
| Research | 2.3 | 12.5 | 18.3 | 9.7 |
| Production | 1.1 | 13.0 | -8.0 | 11.1 |
| Other work activity | 1.1 | 14.0 | 0.6 | 9.2 |
| Management | 3.8 | 17.4 | 6.2 | 8.1 |

^a Among employed respondents reporting salary and class of work activity (10,572 in 1966 and 9,744 in 1964).

^b Estimated from a weighted average of \$10,000 median salary received by 2,508 who reported 9-10 month salaries and the \$12,100 median salary received by 621 who reported 11-12 month salaries (excluding 591 who did not report salary or did not report whether salary was for 9-10 or 11-12 months).

^c Derived from estimate in column 2 and recorded median of \$9,700 in 1964.

the new entrants into this activity. Teaching employment increased by 7.2 per cent, slightly greater than the 6.5 per cent increase in employment of economists generally but slightly smaller than the increase in the total registration of economists. Due to a change in classification, the exact rise in the teachers' median salary is not known at this time, but the estimated rise of 8.2 per cent was smaller than that average 9.2 per cent rise for economists as a whole. The relatively high salaried activity of management also showed a less-than-average 8.1 per cent rise in median salary between 1964 and 1966 and a less than average 6.2 per cent increase in the numbers of economists employed on management work.

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BOOK REVIEWS

General Economics; Methodology

Our Free Enterprise Economy. By MORRIS A. COPELAND. New York: The Macmillan Company, 1965. Pp. vii, 302. \$5.95.

In the preface to this medium-length book Professor Copeland says that it "was written from an institutionalist viewpoint"; and, "Hitherto, there has been no adequate systematic statement of the way institutions organize economic activity in an industrialized free enterprise economy that has no general economic manager." In brief, the book is a partly orthodox, partly unorthodox, introductory discussion of the United States free enterprise economy. Exactly to whom the book is addressed is not clear from the preface but Copeland is presumably thinking chiefly of introductory students and intelligent laymen, and hoping, perhaps, to persuade a wider group of economists of the value of the institutionalist approach. Some of the technical terms such as open-market operations receive a briefer exposition than is customary but a glossary at the end of the book partially justifies this procedure.

The book covers most of the topics one would expect in a work of this sort though the general approach and the set of subjects discussed reflect the institutional point of view. It casts a rather wide net. Thus, Copeland gives brief attention to such wide-ranging topics as the effect of the FBI and CIA on personal freedom, and the desirability of some sort of federal financing of election campaigns. He gives some attention to what sometimes seems to be almost innumerable matters but emphasizes two "major problem areas": maintaining adequate aggregate demand, and developing better labor-management institutions. A prominent feature of the book (which, however, does not dominate the entire exposition) is its emphasis on ten "central management functions" which are performed mainly by nineteen "social, legal, and economic institutions." A brief listing of these using Copeland's summary titles will not satisfactorily convey the full nature of the discussion and analysis, but will suggest its flavor. The three basic central management functions are: the main programming function, the personnel function, and the distribution function; the four ancillary functions are: the resource-use-regulating function, the progress-promoting function, the capital-accumulation function, and the level-of-output-determining function; three supplementary functions of relatively recent origin made necessary by the way in which the basic functions are performed are: the account-keeping function, the terms-of-trade-regulating function, and the authority-and-discipline function. The nineteen institutions are: freedom of calling, trade, the market, the price system, the profit system, the free enterprise system, the business corporation, property, contract, the debtor-creditor relation, money, the income and money circuit, competition, the wage system, the business accountancy, capitalism, business mores, private wrongs, and bankruptcy.

My criticisms fall into two major classes. First, Copeland discusses many

matters in a rather cursory manner and/or a manner which gives little sense of other, often more penetrating, discussions in the literature. Thus, the analysis of problems relating to property rights appears at several different points and is not carried very far. The brief explanation and evaluation of the Pigou effect (p. 84) is not felicitous. The rather lengthy discussion—at least in a book of this type—of the theory of economic replacement of durable goods (pp. 183-91) seems rather elementary and inadequate to this reviewer (who has spent considerable time in that literature). The only literature cited at all extensively is J. M. Clark's writings on overhead costs and workable competition.

Second, although Copeland's discussion of institutions is often interesting and relevant, the logical structure of the argument is left unclear. Is Copeland's list reasonably complete and free of overlapping? Are there relationships among the institutions and their roles? The reader must supply his own answers.

It would be uncharitable and intellectually dishonest not to point out the book's desirable attributes. Copeland wants to throw light on the role institutions play in economic life and wants to acquaint the reader with a wider range of subjects than is found in the usual economic treatise. He shows good sense at many points in the exposition. He emphasizes the virtues and problems of a free enterprise economy in a way that this reader approves.

There are undoubtedly economists who will have no reservations about making this book the central core reading in an introductory course. Some will probably use it with the idea that they can tighten up some of the reasoning. It will be of interest to many others only as an example of what one author can achieve in an introductory analysis of our economy from an institutionalist point of view.

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Income Theory and Related Empirical Studies

Economic Activity. By G. C. HARCOURT, P. H. KARMEL AND R. H. WALLACE.
Cambridge: Cambridge University Press, 1967. Pp. 323. \$9.50; paper, \$3.25.

Economic Activity provides an elementary treatment of macroeconomic theory with particular emphasis on the Australian economy. The text has not been designed for use beyond the introductory level—either in content or presentation—and would be unsuitable for such.

The book opens with a description of the Australian National Income Accounts and proceeds through the usual two-sector model of real income determination. In later chapters, the authors discuss consumption and investment behavior, the multiplier, the role of the government sector, inflation, trade and employment.

With few exceptions, the text is standard fare. The authors distinguish carefully between *ex ante* and *ex post* concepts and the section on consumption is

pleasantly broad for an elementary study and encompasses discussion of normal, relative and permanent income. Unfortunately, these hypotheses are disregarded in the discussion of the multiplier and their implications are never explored.

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Economic History; Economic Development; National Economies

Modern Economic Growth: Rate, Structure and Spread. By SIMON KUZNETS.
New Haven and London: Yale University Press, 1966. Pp. xvii, 529.
\$12.50; paper, \$2.95.

This book, the culmination of one phase of a lifetime study of growth by a distinguished scholar, discusses so many subjects, often in quite a fresh fashion, that it is difficult to visualize an economist who will not find something to interest, challenge, and stimulate him. As seen by Kuznets, modern economic growth began in the 1780's in Great Britain, not before the late 1830's anywhere else, reached 15 or 16 countries early enough (50 years ago) to permit study of long term trends, and has now penetrated countries containing one-fifth to one-fourth of the world's population. Its dominant and distinctive feature is application of science to problems of economic production. Scientific advance and exploitation of its potential for growth made possible most of the rise observed in per capita product, and required new attitudes and changes in social institutions. In Kuznets' view, causes, characteristics, and consequences of modern growth are in large degree common to all countries experiencing it; this book is devoted to discovering common patterns, examining why they appear and the process by which they took place, and exploring their consequences and interrelationships.

The author ranges too widely over subject matter, time, and space to permit even a listing of the many interesting and often novel questions discussed, but some indication of scope must be attempted. The first six chapters deal with the past growth of developed countries, although the underdeveloped world is often brought in for contrast or to draw inferences about the past. Kuznets finds that modern growth has been characterized by a much faster population rise than in previous times, an increase of per capita product at unprecedented rates of from nearly 15 to 30 per cent per decade, and a rise in total product of from 20 to nearly 50 per cent per decade. Rapid population growth resulted from and, on balance, probably also contributed to higher per capita product—several ways in addition to scale economies are noted—although it may have adversely affected the saving rate. The per capita rise in man-hours worked and capital was small and can account for less than one-fifth of the rise in per capita product; the rest is due to new knowledge, changes in the quality of inputs, and scale economies. The share of agriculture in total product has declined, that of industry has risen, and that of services has not changed much or consistently. Output per man in the three sectors rose by av-

erage, above-average, and below-average amounts, respectively, but a high growth rate of efficiency has characterized all major producing sectors. Changes in the industrial distribution of employment, including the effect on urbanization, had great social and economic consequences which are discussed. The size of enterprises has increased. The share of labor in income has risen and that of capital and land declined. Income inequality has been reduced, though only since World War I or II, and the turnover among size classes has increased. Shares of government consumption and gross capital formation in total product have increased at the expense of private consumption and there have been important shifts within these aggregates. Major changes in international aspects of growth, including interruption of the rise in the ratio of foreign trade to national product, occurred around 1913 but these were not so great as to require distinguishing the pre-1913 and post-1913 periods as distinct for analysis of growth.

Kuznets next turns to the underdeveloped countries. He classifies the areas of the world by income in 1958 and considers the significance and appropriate modification of income comparisons when differences are great. Underdeveloped and developed countries are compared with respect to much the same aspects of economic and social structure as were considered in examining changes in developed countries. Many differences seem similar in direction to those observed in the process of growth but some are not, and Kuznets stresses that technological innovation and changes in consumer tastes greatly qualify the possibility of inferring past or future temporal changes from cross-sectional comparisons. The question of whether 175 years should be considered a long or short period for growth to spread from 1 to 20 or 25 per cent of world population is asked; after consideration of the necessary time lags and difficulties, the period seems to Kuznets a short one. A final chapter provides a summary and discusses three general qualifications that could profitably be read immediately after Chapter 1.

If the reader is more intrigued than convinced by Kuznets' conclusions on controversial points, it is likely to be for two reasons. First, interpretations of historical developments rest to an unusual extent on the author's personal research, especially his articles in *Economic Development and Cultural Change*; economic historians, especially, may feel discussion of some topics pays insufficient attention to other investigations. Second, marshaling of statistical evidence to establish patterns is exceptionally informal. For each topic the sample consists of those countries in a loosely specified category for which relevant statistics are available; it varies from topic to topic. For intertemporal comparisons, the same is true of time periods except that they must usually be long or changes pronounced. In the analysis of modern growth, periods before it began in each country seem usually to be excluded when income changes, but not population changes, are examined. Findings are sometimes based on inspection of data for several individual countries. More often, especially in cross-sectional comparisons, they are based on unweighted averages of data (usually percentage distributions) for available countries. Sometimes aggregates implying a weighted average of countries, with shifting country weights,

are used instead. Where only averages or aggregates are presented, the reader has no opportunity to observe dispersion. Countries are usually compared or combined at the same dates, irrespective of the elapsed period of modern growth. Kuznets suggests the date of entering modern growth probably affects the speed of growth and, by implication, of associated changes, but this point does not enter the analysis systematically, nor are these dates indicated for more than a few countries.

Kuznets' procedures might be quite satisfactory for growth analysis if all changes were uniform and continuous, once short fluctuations are ironed out. Uniformity is often sufficient to permit a clear statement with respect to usual direction of change or difference. Confidence in findings with respect to typical *magnitudes* must rest heavily on faith not only in the skill and artistry of the investigator—here the reviewer's confidence is high—but also in the *existence* of typical magnitudes.

Where data are utterly lacking, Kuznets frequently uses illustrative calculations, based on reasonable or extreme assumptions, to establish what the historical record must have been. These are ingenious and usually persuasive; when they raise a doubt it is usually because they use values (e.g., a 15 per cent per decade growth rate of per capita product) that are regarded as representative but are hard to identify with any particular place or period.

One such calculation (p. 394) raises an interesting point of emphasis. It yields the result that the ratio of per capita product in presently developed countries to that in countries with 1958 per capita product under \$100 was 7 to 1 a hundred years ago and 18.5 to 1 now. "Thus almost six-tenths of the present differential is due to . . . growth . . . over the last century." The same ratios imply per capita product in the low income countries was already 86 per cent below the advanced countries a century ago and is 95 per cent below now; would it not be equally valid to say that nine-tenths of the present relative differential had already emerged a century ago?

Finally, three short observations. A calculation on page 78 erroneously assumes that a 25 per cent drop in the capital-product ratio over a long period means the growth rate of capital was 25 per cent lower than that of product; with a product growth rate of 3.05 per cent annually over 70 years (for example) the difference in growth rates would actually be only 14 per cent. In analyzing income shares, Kuznets states (p. 184) that gains in "residual efficiency" are explicitly or implicitly so assigned as to raise the labor share; this appears to derive from confusion between current and constant price data. The comparisons between advanced and underdeveloped countries of productivity by sector in Chapter 8 are extremely precarious because the method does not allow for differences in relative prices.

Kuznets has now carried the study of long term growth on an international basis about as far as the extensive approach used in this book can take it. With his active participation the appropriate next phase, comparison of detailed country studies conducted along uniform lines, is well under way under the aegis of the Social Science Research Council.

EDWARD F. DENISON

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Estimating Foreign Resource Needs for Economic Development: Theory, Method and a Case Study of Colombia. By JAROSLAV VANEK WITH RICHARD BILSBORROW. New York: McGraw-Hill, 1967. Pp. x, 180. \$8.95.

This valuable book is mainly an exercise in the two-gap method introduced by Chenery, containing theoretical as well as applied contributions for the case of Colombia, which is studied in great detail. It consists of three parts: Part 1 providing what is called the "Groundwork"; Part 2, the Estimation Structure and Projections for Colombia"; and Part 3 an "Analysis of Results." The "two gaps" referred to are known as the savings gap and the trade gap; the former constituting the gap between investment desired and savings the nation is supposed to be able to make, and the latter the gap between imports needed and exports supposed to be attainable. If defined sufficiently widely, the two gaps must be identical "ex post." They are estimated in advance (or "ex ante") in order to determine the country's need for foreign finance. The equations on which these estimates are based need not contain the variables which in reality take care of the adjustment of one gap to the other. In the Chenery approach they indeed do not contain them. This then leads to two different figures for the two gaps. Chenery calls the bigger one among them the dominant gap. He has presented arguments for believing that in many developing countries at first the savings gap is dominant and later on the trade gap.

Vanek's book first of all sets out (in Part 1) the method and the initial situation in Colombia, by which phrase he indicates the figures available for the period 1954-1963. Next, in Part 2, the estimation of the various relations needed to determine the two gaps is given. This is followed by a number of "key estimates" for 1965 and 1970. Key estimate group A is based on work done by the Committee of Nine of the Alliance for Progress. It is a group, since for various parameters different estimates are used. Group B is based on the recent performance of the Colombian economy. As group C a set of estimates representing a low alternative for growth is shown. Finally, group D is based on Colombia's national development plan sectoral rates of growth. Comparisons of the key estimates with some estimates by the International Bank for Reconstruction and Development are made. Finally, in the same Part 2 one chapter is devoted to what is known as sensitivity analysis: how much are the gap figures influenced by variations in a number of the parameters used? Since the influence of no less than 50 parameters is studied, a tremendous number of alternatives have been calculated for which the author must be given credit.

Part 3, the "Analysis of Results" supplies some further theory in Chapter 6, to which we will return later. It also contains three more chapters; one about the use of the gross capital-output coefficient; one about savings and the terms of trade; and, to end, Chapter 9, summarizing the evidence for Colombia.

Clearly the book makes contributions of two types. On the one hand it contains an enormous amount of figures, observed as well as estimated (for future periods), for Colombia, which constitute very valuable information about the

past and the expectations for the future of Colombia. The various groups of key estimates with all their alternatives are carefully described and argued and represent, with the final choices made, a precious documentation about Colombia.

The other type of contribution the book makes are contributions to the two-gap theory. Since this is a young branch of economic thinking, it can hardly be expected to have reached already a stage of established doctrine. In the reviewer's opinion the author has not yet provided us with what a textbook on this theory three years from now should contain; but the book does have some useful new concepts and vistas. Compared with classical theories or models on the subject, the basic new element brought by Chenery, namely the possibility of the inequality of two gaps which "should" be equal (and will be equal) derives from similar possibilities in a linear programming model. It is equivalent to the non-existence, in the short run, of equilibrium, where it was assumed to exist in classical theories. One recognizes elements of various other theoretical structures; for instance, dynamic theories or theories in which non-fulfilled expectations play a part (as also in the theory of imperfect competition). As so often, the innovating author himself is not always clear in his formulations. Vanek has added several concepts which will contribute to more clarity. Thus, he distinguishes between two adaptation processes, one which leads to the equality *ex post* of the two gaps and another which adapts either of the gaps to what he calls the minimum requirements. The author also contributes by making a distinction between autonomous and policy-induced adaptation processes. In essence this is equivalent, the reviewer feels, to distinguishing between various types of additional variables which must be added to the estimating model in order to make it complete. Among these variables there may be policy instruments and there may be "free variables" whose values are the result of market forces (or of private decisions, which is more or less the same).

Yet the text is not yet quite satisfactory to this reviewer. It remains desirable that a still more systematic treatment emerge, in which the various types of variables are explicitly listed and in which, in addition, the types of problems are listed which are treated. Thus, in the end, it will become possible to answer all questions which the new theory (or method) evokes. One such question is whether the *ex post* gap need necessarily show a value inside the interval of the two *ex ante* gaps. I see a possibility for the *ex post* gap to be outside that interval. Another question is how many categories of estimates it is useful to distinguish: is it at all useful to distinguish between minimum and non-minimum requirements and by variations in the type of assumptions used? With the large number of variables and assumptions at stake we may finally end in confusion. These are a few questions one hopes will be answered in the ideal textbook three years from now about the subject. Even so Professor Vanek produced a most valuable book.

It was also a good idea to add Chapter 9, which is easy to understand for the practitioner in development aid. One question that came to the reviewer's mind is whether we can be content with the rates of growth of the Colombian

economy considered in this volume. Even the official plan is not very ambitious with its 6.1 per cent per annum.

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Europe's Postwar Growth—The Role of Labor Supply. By CHARLES P. KINDLEBERGER. Cambridge, Mass.: Harvard University Press, 1967. Pp. xi, 270. \$6.50.

Charles Kindleberger, with this work, has given us another interesting, useful, and in some ways rather different book.

The task Kindleberger set himself was to "explain," so far as organized statistics and educated insights permit, the rates of growth of most Western European economies, rapid, normal, and slow, in terms of the availability of extra labor on favorable terms. This is the theme that prevents the book from becoming a hodgepodge of statistics, facts, and observations, arranged by country, but with little other sense or order. If this theme seems overworked at times, the reader should be tolerant.

At the outset the author necessarily presents Arthur Lewis' thesis of growth through unlimited supplies of labor as outlined in two well-known articles in *The Manchester School*. Lewis there holds that developing economies go through two distinct stages. During the first, when labor is available in infinitely elastic supply to a capitalizing sector (probably industry but possibly modern agriculture), additional investment (from domestic or foreign sources) results in more workers being employed at unchanging capital to labor ratios. Later, as extra labor from backward, subsistence, rural sectors becomes available only at higher real wages, capital *deepening* substitutes for capital *widening*. In effect, as Lewis stresses, the old classical labor cost theory of value tends to prevail in Stage I, during which period development heavily depends on extra capital, financed from profits. In Stage 2, profits and wages depend on marginal productivity, and savings and investment can flow from wages as well as profits.

With this theme in mind, namely, that rapid growth requires available extra labor at little if any extra real cost, Kindleberger examines the postwar record, to 1964 approximately, of (1) fast growing developed economies with expanding labor supply, namely Germany, Italy, Switzerland, and the Netherlands; (2) fast growers with limited labor, Austria and France; (3) slow growers with limited supplies of labor, Scandinavia, Belgium, and the United Kingdom; (4) undeveloped countries of emigration, Portugal, Spain, Greece, and Turkey. What he has to say about these economies is invariably interesting, and he pieces a great number of varied facts together in a skillful and usually persuasive way. Whether this has anything to do really with the Lewis schematic, is not apparent, but also is perhaps unimportant.

The main complaint this reviewer has concerns Kindleberger's indiscriminate use of incremental capital to output ratios (ICORs). Practically, an aggregate ICOR is obtained by taking some period such as a decade for a national economy, and dividing any extra output into any extra capital stock

(i.e. investment). The resultant quotient has of course no operational meaning. For instance, if this ratio is 3, it does *not* mean that \$3 million of new investment will *cause* \$1 million of extra output. Extra output also comes from extra labor force and productive innovations. It would be no more meaningless, and perhaps less, to attribute all extra output to extra labor employed over time. However, if Kindleberger is analytically careless in this one regard, he is in numerous company.

In some respects, as the author disarmingly admits, this book is several works between one pair of covers. Thus the main contribution is the treatment of the various Western European countries (Chs. II through VII), but there are also some very illuminating if general chapters on labor migration in Europe (Chs. VIII through XI), while the last two chapters are a sort of afterthought on some economic aspects of the United States. Some chapters (e.g. Ch. X on the Costs and Benefits of Labor Migration) could well stand alone as journal articles.

Neglecting these caveats, *Europe's Postwar Growth* with its special reference to differences in extra labor supply from country to country, rewards careful reading. It is brimful of selected data. It is hard to suggest any other single work of late, except Denison's *Why Growth Rates Differ* (Brookings, 1967), that gives a superficially informed reader so much to absorb and ponder.

Lastly, Kindleberger offers throughout an honest balancing of arguments and a modest scepticism, uncommon virtues among many economic development writers.

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The Challenge of Development: Theory and Practice. Edited by RICHARD J. WARD. Chicago: Aldine Publishing Co., 1967. Pp. viii, 500. \$6.95.

The forty-five articles making up this volume were selected from a much larger number of papers delivered in 1963 at a United Nations conference on "The Application of Science and Technology for the Benefit of the Less Developed Areas." This collection is distinguished by the unusually wide range of subjects covered and the heavy emphasis on descriptive technology and case studies of new enterprises and social overhead projects. A third of the book is devoted to general planning models and to human resources as factors in development. The remaining two-thirds of the book covers less familiar material ranging from the role of plant genetics in food production to highly specific essays on how to build a railroad, how to organize a textile industry, etc. Finally, in the last fifty pages there is a return to general models but with the emphasis on social and cultural variables. The clear intent of the editor was to provide a kind of miniature encyclopedia of concepts and case problems in applied development work suited to the needs of the pragmatic minded administrator. My comments, though limited to a sample of the articles, will keep this purpose in view.

Hollis Chenery's deftly executed "Model of Development Alternatives" demonstrates with equations and graphics the consistent sets of alternatives

open to development planners given the constraints of the expected inflow of foreign capital, the rate of domestic saving, the composition of internal and external demand, etc. However, in the process of empirical implementation, the initial equations are necessarily simplified and one begins to wonder whether the limits to growth derived from the model are as much a result of the constraints imposed through such simplifications as they are attributable to the resource limits of the country discussed. The administrator reading the Chenery article gets a picture of the development process in terms of an aggregate production function with one independent variable (capital stock) which increases through additions from domestic saving or foreign capital inflow (p. 42). Of course an elaboration of the model proceeds from this base but there is little recognition of the problem of underutilization of the existing capital stock or of effective utilization of additions to the capital stock. This omission is remedied in part by P. N. Rosenstein-Rodin ("Planning the Use of External Resources") who discusses the "absorptive capacity" of a country, i.e., its ability to use capital productively. The emphasis given to the problem of utilizing foreign capital and the measuring of absorptive capacity is a needed corrective to much of the thinking about development in terms of simplified schema. Of the remaining articles in the section, two seemed of general theoretical interest: E. P. Holland, "Principles of Simulation" describes the application of this technique to development problems. While the article starts out with some promising statements of purpose, it leads through a rambling discussion to the results of "simulation runs" which at least as presented here seem to offer little of value to the development planner. Howard S. Ellis in a brief theoretical discussion of the effects of regional economic integration succinctly evaluates the possible gains from integration, and shows the relation of national development planning to the kind of gains that can be achieved.

Part two on "Developing Human Resources" is the most disappointing section of the volume in that it shows little recognition of the theoretical work being done on the economics of investment in public health, education, etc. There are brief reports on the setting up of an occupational health unit in Peru, a manpower training program in Africa, and a fragment on "Improving Public Management." Bert Hoselitz contributes a readable essay on the "Entrepreneurial Element in Economic Development" using psychological factors stressed by McClelland along with an explanation in terms of socio-economic structure. The main article by P. M. Hauser on "Population and Labor Force Resources" competently reviews familiar material on the high cost of population growth to an underdeveloped country. But the analysis of the effect of high birth rates on age structure, and the consequent dependency burden of less developed countries, is much too limited. It is true that the population "under 15 years" may be proportionately greater in the underdeveloped countries but the labor force participation rate of children in subsistence agriculture is very high while children in industrialized countries produce little and require a costly education. We cannot measure dependency burdens simply by looking at age composition tables from the *Demographic Yearbook*!

The case studies in parts three and four are written by specialists who are knowledgeable about the industry or project discussed, but engineering or in-

dustry data must be related to broader economic concepts if maximum benefits are to result. A comparison of two articles will illustrate the point. J. Wickersham's article on food processing seems to reason as follows: milk is a very important food; the underdeveloped countries have low per capita consumption of milk; hence it is clearly desirable to set up milk processing plants, details of which are then presented. No consideration is given to the alternative possibilities of supplying needed proteins at lower cost through say the production of fish meal or other high protein flour additives. By contrast, John Gallagher's "Markets as a Basis for Industrial Development," which describes the operations of Sears Roebuck in Latin America, shows an awareness of the strategic economic elements: Sears used its buying power to develop local sources of consumer goods, increasing domestic employment and capital formation, and allowing more foreign exchange to be used for capital imports. Small manufacturers were assisted through a careful projection of future demand schedules and by prompt payment for goods which reduced the need for borrowed capital. The article offers much useful and detailed information along these lines.

The many articles on aspects of agricultural development are too numerous to even list, but I can report that they are generally well done and informative. The three articles by Todd, Greenman, and Hagan on hydrology, reclamation, and irrigation along with the Sherman Johnson and R. P. Christensen essay on the efficient use of agricultural resources merit particular attention.

Despite the limitations noted, this volume is worth having on one's shelf, if only as a reminder of the staggering range of specialized knowledge and skills which must be brought to bear on the problem of development. A reading of the book is a good antidote to the common preoccupation with general models and simple policy formulas.

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Numerical Models of Economic Development. By A. QAYUM. Rotterdam: Rotterdam University Press, 1966. Pp. ix, 106. \$21.50.

A reviewer may serve a useful purpose by drawing attention to a significant new contribution to the literature. Or he may serve an equally useful purpose by pointing out a book that the reader can well afford to pass up. Professor Qayum's book can easily be passed up.

The book contains 10 essays dealing with various aspects of economic growth, with particular reference to developing countries. The issues raised by the author are central to the study of economic growth. However, much of the book is characterized by a relatively sterile and unoriginal application of mathematics to economic problems; and where the author attempts an original contribution, his analysis is frequently muddled.

For example, consider the second essay, "Theory of Income Generation and Economic Growth." Here we have what looks like a straightforward multiplier-accelerator model. Income in period t , (Y_t) , is written as the sum

of outlays on investment in period t , (O_t), and effective demand in period $t-1$, (X_{t-1}),

$$(1) \quad Y_t = O_t + X_{t-1}.$$

Then we have

$$(2) \quad X_t = \delta Y_{t-1}$$

$$(3) \quad O_t = \beta(X_t - X_{t-1})$$

where β (despite the author's assurances to the contrary) looks suspiciously like an accelerator. The reader is then told that " O_t is open, and it can be raised or lowered, and Y_t will correspondingly change." The author does not try to reconcile this with equation (3). Qayum also denies that (1) is a Keynesian identity. Indeed, the Keynesian identity "is not relevant." Equation (1) represents "a major shift of emphasis from a static identity to a dynamic equation."

Or consider the last essay, "Life of Capital and Economic Growth." Here the author examines the effect of factor substitution on the level of output. He uses a notion of capital (after Johansen [1]) where "there are substitution possibilities *ex ante* but not *ex post*." New capital goods are combined with uncommitted labor according to a chosen factor ratio. But the factor ratio, once selected, remains constant for the life of the capital. According to Johansen, capital is built to employ a technique that will achieve continuous full employment of capital and labor. Qayum, however, has a better idea. Instead of building capital in period t to take account of labor availability in period t , the capital should reflect labor availabilities in periods $t, t+1, \dots, t+h$ where h is the lifetime of the capital. This may sound good, but it ignores the fact that capital built in periods $t+1, t+2, \dots, t+h$ absorbs labor becoming available in those periods, so that building unemployment into the model involves pure waste. The author presents a numerical example and a "proof" to support his contention. However, he has furnished us with a proof of the wrong theorem. His analysis is appropriate for the contemporaneous allocation of labor among firms: given the same production function in all firms, and given each firm's capital stock, it pays to allocate labor so as to equate the capital-labor ratio everywhere. The problem of allocation over time has an important difference. If labor is set aside in period 1 to be used in period 2, there is a loss that is never regained. Indeed, using Qayum's example, and any plausible parameter values, full employment of labor in both periods is the best alternative. Qayum reaches contrary conclusions only because he has formulated the problem incorrectly. (There is also a computational error in his numerical example.) Qayum's treatment of this problem reveals an underlying misunderstanding about the notion of relative factor scarcities in a growing economy. If capital and labor are growing exponentially, then the labor intensity built into each new unit of capital should reflect the incremental factor ratio obtaining at the time the capital is built. It is irrelevant how the factor ratio changes over time, given that new investment is continuously taking place.

The other essays are less muddled than the two discussed here. However, the reader may be puzzled to find such statements as: "Labor-intensive meth-

ods are all right, if the increase in effective demand can be met by goods produced by *fresh* investment outlays in successive periods" (*italics his*). Or "A trifling but stubborn fact of the present day world is that the countries which are in short supply of capital goods are also in short supply of consumption goods. This shows that there is a large degree of interdependence and substitution between the two." One more: "The usual models, which are sensitive to changes in consumption only, cannot account for developments that are initiated by changes in the demand for capital goods (to start with)."

This reviewer feels that the reader is unlikely to learn much or to gain many new insights from reading this book.

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REFERENCE

1. LEIF JOHANSEN, "Substitution versus Fixed Production Coefficients in the Theory of Economic Growth: A Synthesis," *Econometrica*, April 1959, 27, 157-76.

Peasant Agriculture, Government, and Economic Growth in Nigeria. By GERALD K. HELLEINER. Homewood: Richard D. Irwin, 1966. Pp. xx, 600. \$9.25.

Agriculture and Nigerian Economic Development. By H. A. OLUWASANMI. New York and London: Oxford University Press for the University of Ibadan, 1966. Pp. vi, 240. \$7.20.

While these two studies seemingly cover the same topic, in actuality both the content and approach are dissimilar. Helleiner's book, a volume in the Yale University Economic Growth Center series, is research oriented and heavily quantitative; that of Oluwasanmi is an expanded and very readable, largely qualitative essay. In content, Helleiner incorporates an exhaustive examination of the role of the government in economic growth and, although data in Oluwasanmi do not go beyond 1961, Helleiner includes material as late as 1965. Both emphasize, however, that the impetus in Nigerian economic growth from the turn of the century and until recently has come from the export of "peasant" agricultural products. In development literature, industrialization and the so-called enclave economies have received widespread attention. It is refreshing and surprisingly provocative to see serious attempts to evaluate the oft overlooked place of traditional agriculture in accelerating and maintaining economic growth.

In the areas of agriculture and government upon which Helleiner concentrates, his findings and analysis offer the most detailed and complete data since those of the IBRD's report on its Mission to Nigeria published in 1955. The 350 pages of text are accompanied by 31 pages of useful bibliography and some 204 pages of statistical appendix. The first two chapters of *Peasant Agriculture, Government, and Economic Growth in Nigeria* are devoted to the economic history of growth since 1900, pointing out that until World War II governmental attempts at stabilization were negligible. This characteristic was reversed during the war years and last two decades.

The following three chapters should be of particular interest to the agricultural and land economists as they investigate peasant agriculture, its nature, principal exports and its position in the economic development of Nigeria. These sections give, in essence, a case study of Nigerian agriculture which dovetails well with the recent outstanding two volume *Experiences with Agricultural Development in Tropical Africa* (by John C. DeWilde *et al.* Baltimore 1967) which concentrated upon Kenya, Mali, Uganda, Tanzania, Upper Volta, Chad, and the Ivory Coast. Helleiner points out that the basis of Nigerian agriculture was free from the frequently found enclave-plantation feature and rested firmly among the peasants. It was traditional in form, technologically underdeveloped, and functioned with a communal form of land tenure which had the social advantage of no tenant-landlord relationships or a landed aristocracy. However, this tenure system has been breaking down in favor of individual peasant ownership.

The author successfully argues against the commonly held position of many development economists that the African peasant either fails to respond or responds perversely to economic incentives. An additional characteristic is the regional overspecialization of the farmer, i.e., cocoa in the West, oil palm in the South, groundnut and cotton in the North, rubber in the Midwest. Although Helleiner estimates that from 1900 to 1962 the export sector's volume increased more than sixteen-fold and accordingly provided the fuel for economic growth, present economic indicators reflect some limitations for agricultural export expansion as income elasticity of demand for all Nigerian agricultural exports is relatively low and specifically that price elasticity for cocoa is among the lowest but still higher than unity. Yet real prices are influenced by a combination of world market forces and government fiscal policy.

Chapters 6 through 14 will hold the attention of development and public finance economists. Helleiner's analysis of the Marketing Boards, their evolution and use as a tool of government fiscal policy, is the best available. Since the outbreak of World War II, such Boards as statutory monopolies have controlled the purchase of agricultural products domestically and sale on world markets while establishing prices paid to Nigerian producers on a basis of nonmarket criteria. In the mid-1940's Nigerian Marketing boards existed for specific crops (cocoa, groundnuts, oil palm produce, cotton) and later were broken into Regional Boards on a cross-commodity basis. Helleiner notes that economic interest has been directed toward the stabilizing role of these boards but "Growth should always have carried greater weight in Nigerian policy formation than stability, and there exists no conclusive evidence that the two are correlated" (page 159). The Boards' surpluses have been designated for stabilization and for research and development with the latter progressively expanded only recently. In the balance, the Boards were more effective in acting as earners of tax revenues than in their stabilization objectives which included producer incomes and prices, with the latter more attainable than the former.

While peasant agriculture spurred growth in the first half of the century, apparently the public sector will be, at least for the time being, the prime mover toward development. It is noted in discussing revenues that, even by African standards, the government sector in Nigeria is small relative to the

GDP. However, government expenditures in the 1960's reached 15 per cent of the GDP resulting in recurrent deficits rather than the former surpluses. Public borrowing both domestic and foreign is increasing. The foreign trade sector remains a dominant contributor to revenues through import and export duties. There is some blurring on the share of direct taxes in public revenues in the last decade or so which could use clarification (page 219, text and page 210, Table 50). One might have found beneficial a fuller breakdown so that the growing impact of oil royalties and income taxes on this industry could be assessed. (The same omission regarding oil is also noticed in Oluwasanmi's work.)

Helleiner concludes with a largely descriptive chapter on the National Development Plan, 1962-1968. Among other targets, 15 per cent of the national output is earmarked for investment to achieve the required 4 per cent growth rate with half the cost of public investment and the private investment to be financed from abroad. Yet at the halfway point, the obstacles to successful implementation are growing serious as the external debt servicing commitments are approaching over 10 per cent of the projected exports. Short of extreme austerity measures, the key may be held in the exploitation of oil.

The first five chapters in *Agriculture and Nigerian Economic Development* cover agriculture and the national economy, Nigerian agriculture in historical perspective, land use and population. These chapters require careful reading because of their brevity unless one is well versed in the socio-economic conditions of that country. The author dramatizes his subject by closely relating Nigerian development to its most primitive and backward sector, peasant agriculture. Commercialization of agriculture became the most essential characteristic, explaining early Nigerian economic growth as synonymous with this process. Commercialization was stimulated by a dual policy of taxation assisting the monetization of the economy and the introduction of new crops. Oluwasanmi concludes that, in the past, investment of scarce capital resources in agriculture to generate growth in other sectors was the most profitable decision considering the limitation of social conditions and institutions.

On the subject of land tenure, this work differs from Helleiner's in that the treatment is extended to include the sociological and regional implications as well as the strictly economic, which points up Oluwasanmi's intimate knowledge of indigenous conditions. While the overall Nigerian agricultural picture is not unlike that of other developing countries, nonetheless, it is not the scarcity of land but the low level of technical know-how and implements which restricts the acreage of peasant farms to that workable by the family unit. The author maintains that Nigeria as a whole is not overpopulated but that social barriers present a very real problem to rational land resource allocation.

In subsequent chapters Oluwasanmi deals with patterns of production, marketing, and finally agricultural development and industrialization. Emphasis is centered upon avoiding so-called pure research and concentrating on pragmatic aspects à la Theodore Schultz. This view is reinforced by the limitations on available funds and the redundancy in federal and regional agencies. Throughout this work the awareness of the complexities of change makes for a sobering outlook. The author gives the impression that he is under no illusion

of magically moving the Nigerian economy to an industrialized status. The closing position which Oluwasanmi advances challenges the belief that industrialization *per se* is the panacea for underdevelopment in Nigeria. His book provides the development economist with the stimulating and still fresh view of the key role of modern agriculture in the development process and, indeed, the essential support the agricultural sector can offer industrialization.

For those in African studies, development, and agricultural economics the decision is not which of these two studies should be read, but rather in what order. This reviewer would suggest first the broader, more interpretive volume by Oluwasanmi followed by Helleiner's book which contains a wealth of economic data and analysis.

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The Economics of African Development. By ANDREW M. KAMARCK. New York and London: Frederick A. Praeger, 1967. Pp. xiv, 294. \$7.50; paper, \$2.50.

In his preface of somewhat dubious value, Pierre Moussa correctly observes that very few syntheses have been written on the economic development of Africa as a whole, covering all sectors: "This is because it is difficult to know all fifty countries . . . of Africa; very often comparable information is lacking from one country to another and it is therefore very difficult to make meaningful generalizations" (p. vii). The inherent difficulty of this task stems from the continent's vast size and its large absolute number of political and economic units, from the present paucity but rapidly proliferating availability of "facts" (including but not solely statistical data), and from the tremendous variety that is Africa, a diversity manifest in the geographical, political, and cultural as well as economic realms. In undertaking this survey of economic characteristics and development problems of sub-Saharan Africa, Dr. Kamarck has drawn upon his broad experience of over a decade of economic work on Africa for the World Bank Group. Specialists will find here many particulars to fault, but the whole succeeds better than its parts. This is a comprehensive and well-structured introduction to the subject.

The book is addressed to a broad audience of general readers and social science students (hence the textbook format and paperback edition), and requires of its readers very little background in economics. Kamarck's approach is largely descriptive and derivative. There is no underlying or unifying theoretical model or theme, and the analyses are eclectic. A few simple analytic tools are employed (e.g., the backward-bending supply curve), and these are generally efficiently and adequately explained. By and large, the text is well written in uncluttered prose and noneconomists will find it relatively free of academic jargon. Readers may learn much here about African development, but little about economics *per se*. What is original is not the analysis but rather the summarizing and synthesizing of a wide range of sources bearing on the whole scope of African development problems.

The text consists of twelve chapters including four useful maps, a thirty page statistical appendix, and an adequate index. The first chapter briefly

touches on Africa's economic history, and the next three chapters describe the structural characteristics of African economies. Each of the four following chapters focuses on a particular sector: agriculture, mining, industry, and infrastructure. Other chapters are concerned with foreign investment and aid, plans and planning, internal and external political considerations, and prospects for the next generation. As may be inferred from this organization, references to individual countries are scattered throughout. The book is not intended to be and it is not a suitable vehicle for learning about any particular country. Unfortunately, it also cannot be regarded as the reliable source of facts and data which one might have hoped for; more on this point below.

Kamarck is generally more concerned with describing things as they are now than with tracing the course of how they came to be that way. His focus is on the emergence of African economies into "the mainstream of world commerce and life." Consequently, both his historical perspective and his opening chapter on that topic are very short. One paragraph passes over the economic history of the entire period from ancient Egypt to the European entry into the slave trade. The history of the slave trade and its impact receives three pages, while the role of chartered companies is also passed over lightly. More background on developments in the first half of the 20th century is provided in various chapters, and some attention is given to both costs and benefits of the colonial heritage, but the reader will acquire only little appreciation of the significant variations in legacies arising from the installations, institutions, and influences passed on to Africans by their colonizers. In Kamarck's view, World War II represented a turning point in African development, and his main focus is on the causes and effects of the generally respectable growth rates achieved in the postwar period.

The three chapters on structural characteristics of African economies are the most useful part of the book. Here are collected the most important comparisons and generalizations on the composition of output and trade, the distribution of income, the balance of payments, money and banking arrangements, and common denominators of African economic experience and attitudes. Kamarck shows clearly that there is as yet no such thing as "the African economy" in the sense of an economically integrated continent. "Economically and financially, African nations are more like a series of islands lying off the coast of Western Europe than like parts of a single continent" (p. 21). In his view, "the propulsive and dominant sectors are external: exports, foreign investment, and other expenditures financed from abroad. These essentially still determine the size and growth pace of African economies" (p. 27). These conclusions are well supported in an excellent chapter on Africa's position on the world scene. In addition to documenting the continent's great dependence on foreign trade and on non-African capital and personnel, this chapter includes an attempt to construct an over-all balance of payments account. This account for 1963 and 1964 interestingly reveals, for example, that sub-Saharan African countries together appear to balance out in their commodity trade account; the deficit on services and invisibles is just matched by long-term capital inflows.

Notwithstanding the great diversity among the various national economies, Kamarck observes several common denominators which distinguish the problems, processes, and prospects of African development from other continents. These include "a greater sense among Africans of belonging to a single entity," a high rate of migration by workers across national boundaries, the relative absence of population pressure on the land, the "openness" of the economies, the historical importance of the government sector, the egalitarianism and the openness to innovation of most African societies, and various legacies of colonialism, including the large number of "micro-states." The statement that the total population is "probably . . . between 200 million and 250 million in 1966" brings to mind Stolper's observation that the "two major aspects that appear to distinguish the problems of development in Africa . . . from the problems found elsewhere [are]: the lack of hard facts in crucial fields and the absence of a population problem" (*Planning without Facts*, Cambridge 1966, p. 4). The very great wealth of information collected and summarized by Kamarck might itself seem to belie Stolper's point about the lack of facts, but Kamarck himself stresses how sketchy and tenuous our information is on many critical aspects of African development. In fact, this theme dominates his chapter on agriculture; here he laments both our technical ignorance (e.g., about soils and diseases) and our even greater lack of knowledge of the economics of the African farm (including social and cultural factors). Such considerations underlie his warnings against both large-scale transformation projects in agriculture and overly aggregative economic plans. While his long chapter on agriculture is full of information and examples, however, the chapter on planning is itself too aggregative. Here one might have hoped for more on the planning experiences of particular countries such as Ghana and Guinea rather than for so many broad homilies on the possible pitfalls in the making of economic plans in general.

It has been noted that this book's strength is in its scope rather than in its suitability as a source. This is a summary, not a sequel to W. M. Hailey's *An African Survey*. It is not a controversial book, but it is inevitable in a work of this type that specialists will find the generalizations too general. Some errors are simply egregious, as, for example, in Table 8 in the Statistical Appendix, which is not accurately reproduced from Department of Commerce sources. More significantly, economic historians may well dispute the view that "the economic history of the years since World War II has demonstrated that there were no economic reasons for the continuation of European colonial empires" (p. 68); some observers may question whether "emphasis on the importance of economic development . . . coincides with the African *Weltanschauung*" (p. 49); economic anthropologists may fault the lack of consideration for ethnographical factors; economists may criticize the spurious accuracy in the calculation of rates of return on investment. Everyone may regret that there are no footnotes and that the selected bibliographies at the end of each chapter are short and unannotated.

Whatever its shortcomings, however, this sympathetic but realistic appraisal of African development is a significant contribution. It is clearly the

best available introduction to this field and it can be recommended enthusiastically.

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Economics of Development—With Special Reference to India. By D. BRIGHT SINGH. New York: Asia Publishing House, distributed by Taplinger, 1966. Pp. xiii, 611. \$9.75.

Economic development is an area where textbook writing is still extremely difficult. The discipline is in an unusual state of flux, the literature is predominantly descriptive but vast, and many contributions are not easily accessible because they are made in foreign languages. Even so, we have been exposed in recent years to many economic development textbooks by economists residing in developed countries. One of the new dimensions introduced by the present text is that it comes from a university professor from India.

Both the approach and the selection of topics is orthodox. In eighteen long chapters are covered the most important and popular topics of the development literature up to 1963. The author starts out with two general chapters on the meaning of underdevelopment and theories of growth. The first chapter, with its detailed and systematic examination of the nature of underdevelopment, provides insights that make it one of the best in the book and the literature.

The next three chapters examine in detail and in a lucid way three sets of factors affecting development: institutional and sociological elements, population, and capital formation. By synthesizing the literature, and making extensive use of statistics, Singh is successful in providing, both here and in subsequent chapters, a panorama of the development process rather than a panorama of personal views of development economists as found in Benjamin Higgins' book.

The author assigns a key role to capital formation and demonstrates its importance by an extensive treatment of the factors or aspects of economic development related to investment and capital formation. In the following eight chapters, Singh deals with investment financing (Ch. 6), investment criteria (Ch. 7), monetary (Ch. 8) and tax policy (Ch. 9), deficit financing (Ch. 10), and the external sector (Chs. 11-13). The treatment of the external sector is one of the most detailed available. The last five chapters deal with agriculture, stability, the role of government, planning and the book concludes with a chapter exclusively dedicated to planning in India.

The area of development textbooks is one of differentiated oligopoly. Different types of textbooks can and are being written. One type aims to provide the student with the "basic tools of the trade." Such a textbook is hardly possible in economic development, since the tools are only partly known. Singh recognizes this limitation and makes no attempt to deal exclusively with development theory. Another type of textbook combines the existing body of theory with extensive references to the literature and statistical material. The book by Singh, which systematically summarizes the literature and incorpo-

rates a vast amount of statistical material, falls clearly into this category. Another type of textbook classification, or textbook variation, is important in understanding and evaluating the Singh book. Some texts are written on the supposition that supplementary reading material is both abundantly available and required of the students. Most U.S. texts belong to this category. Other texts, however, are written explicitly with the aim of saving the student the laborious efforts of exploring the literature by himself. Language barriers, inadequate library facilities, and a different approach to education create the need for such texts. The present book aims to do just that and this accounts for its formidable size of 600 pages.

The Singh book, as a consequence, will be most valuable for intermediate courses in economic development in English-speaking countries where supplementary reading is kept at a minimum. In the United States adoption *in toto* would not be efficient. Some of the nontechnical chapters, however, could serve as useful supplements to the Bruton book.

This textbook is impressive in the sense that it comes from a person who has lived in less developed countries, has a good feel of the problems facing them, has carefully read and incorporated the literature, avoids generalizations, is more interested in substance (key issues) than form (the presentation of elegant models with limited validity is kept in bounds), and has a minimal propensity to impose on the student the slogans and the word mongering so popular in economic development literature.

The textbook has some fundamental limitations. It does not incorporate the literature since 1963 (writing was terminated in 1963 and publishing in 1966); it presents a view that is based on the writings about developed and developing nations by economists residing mainly in the United States and India; makes few suggestions about the gaps existing in our knowledge of the field; is not sufficiently theoretical for a graduate economic development course; and incorporates practically no "foreign-language" literature. Furthermore, the tremendous degree of mosaic diversity within the underdeveloped world is not brought out strongly enough. Like many other economists, Singh has a tendency to view the growth process in terms of the narrow agriculture-industry framework. The fact that some less developed continents, such as Latin America, have at least 50 per cent of value added in services, which is neglected in the above framework, also goes unnoticed in the book.

Overall, it should be mentioned that the book, with its special emphasis on India, will serve as a useful example of how basic economic training can become more "intercultural."

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Economic Growth and Development. By BRUCE R. MORRIS WITH C. WENDELL KING. New York and London: Pitman, 1967. Pp. xi, 429. \$7.50.

In contrast to some other areas of applied economics, there is not yet a surfeit of texts in economic development. This reviewer is familiar with a half dozen good intermediate texts which differ significantly in conception and coverage. However, since many instructors prefer to use books of readings which,

although they introduce the student to some of the important literature, lack the kind of unifying thread intrinsic to a good text, it is likely that there is a market for a new text with its own unique orientation. Bruce R. Morris has attempted to meet the demand.

The author tells us that three points of view underlie his approach to the economic development of poor countries: (1) that "the combination of development measures applicable to each country is unique," (2) that "a wide variety of more or less interrelated measures is necessary for development and that the all too scarce resources of the country must be allocated in a manner calculated to achieve the best results," and (3) that "the primary problem in economic development is the improvement of the human element, and it is this that should have priority in the investment program" (p. x). The dust jacket refers to these points as "three vital theses," but it is more accurate to say that they represent the author's beliefs about economic development which, as statements of belief, probably command rather wide acceptance. The second point amounts to little more than a statement that poor countries should allocate their resources efficiently, and is so central to the concern of professional economists that it should be unnecessary to state it as a basic proposition undergirding an economics text.

The organization of the book follows from the first point of view. Since the author believes that the combination of development measures applicable to each country is unique, he does not attempt to present any theory of economic development which he believes to be generally applicable, but instead tries to touch upon all the problems and possible solutions which might be relevant in understanding the economic position of any poor country. Although this does not prevent the author from taking rather definite positions on certain topics (e.g., inflation, population), his general view is that the investigator must take a close look at the individual country before he can give useful advice. Each chapter then becomes a catalog of the many things that might be relevant in a given country to the aspect of development covered in that chapter.

The first two chapters contain some quantitative data on poor countries, a description of their significant characteristics, and a brief survey of theories of economic growth from the Mercantilists through the Classical school, and up to and including the modern-day contributions of Leibenstein, Nurkse, Hirschman, Rostow, Lewis, Hagen, Hoselitz, Myrdal, and Rosenstein-Rodan. Chapters three through thirteen cover those aspects of economic life which the author believes are important to understanding the economic problems and prospects of poor countries. These aspects include social and cultural factors, agriculture, population, labor, industry, technology and entrepreneurship, sources of finance, international trade, institutional arrangements, and the control of economic decisions. Chapters fourteen and fifteen cover investment criteria and programming and some typical policy decisions.

The author eschews the use of "mathematics and complex diagrams" (p. ix). He believes the beginning student must first "acquire a working knowledge of what is happening in the underdeveloped countries." The only mathematical symbols used are included in a brief discussion of Harrod-Domar

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growth models (pp. 49-51). The paragraphs pointing out the implications of these models are particularly unsatisfactory. Only three diagrams are used, the first of which (p. 125) is adapted from an article by Mellor and Stevens (*Journal of Farm Economics*, August, 1956) and is not carefully drawn. Also the textual explanation of what this diagram is meant to convey is inadequate.

The editorial work on the volume is poor. The table of contents is a simple list of chapter headings, and there is no list of tables and charts. There are sentences ending with phrases such as "and the like" (p. 367), "and so on" (pp. 438, 476, 496), "four hours of time" (p. 445), and paragraphs where a good editing job could have substantially improved the clarity of the material. The index was carelessly done, and because a detailed table of contents is omitted, it is very difficult to locate many topics. For example, the following topics, all of which are covered in the text, cannot be located by referring to the index: "big push" (erroneously attributed to Hirschman rather than to Rosenstein-Rodan), "critical minimum effort," "multiple exchange rates," and "blueprint planning." A random check of the index uncovered several errors and omissions. There are also errors in the footnote references.

Space allows only a few substantive criticisms of the book.

1. The chapter on theories of economic growth lacks an adequate synthesis of the various ideas on growth and development which are surveyed. In addition, the treatment in parts of this chapter and elsewhere in the book is so synoptic that it is often difficult to get at the main linkages in the argument.

2. Familiarity with the work of F. H. Harbison and C. A. Myers might have led the author to temper his suggestion that poor countries "lay greater emphasis at early stages of development on establishing technical schools at various levels to train people in such skills as building trades and machine operators . . ." (p. 188). Harbison and Myers, in *Education, Manpower, & Economic Growth* (p. 56), have taken a divergent position: "Our observations . . . lead us to believe . . . that most of the Level I countries [most underdeveloped by their index] have not reached a stage of development where pre-employment vocational schools at the secondary level are really practical."

3. In discussing migratory labor in Africa, Morris writes that one of the disadvantages is that "agriculture is disrupted by variations in the labor force." Morris ignores the work of men such as W. J. Barber in *Economic Development and Cultural Change* (April, 1960) who has argued that the periodic migration of at least some African tribesmen represents a rational adaptation to the periodic labor requirements for male labor in a context of slash and burn agriculture. It seems more reasonable to say that migration may in some instances relieve the pressure upon peasant agriculturists to change agricultural methods which are no longer appropriate because of an increased population-land ratio.

To close on a more favorable note, the chapter on social and cultural factors contributed by C. Wendell King, a sociologist, is a welcome innovation. The treatment of the sociological dimensions of the change process is especially interesting. Finally, the book does impress upon the reader the complexity of

the problems facing poor countries, and the necessarily tentative nature of much of the analysis and of many of the policy recommendations in the field. It also will serve to acquaint the student with a fairly wide body of literature.

FRANCIS M. McLAUGHLIN

Boston College

Impact of Western Man: A Study of Europe's Role in the World Economy 1750-1960. By WILLIAM WOODRUFF. New York: St. Martin's Press, 1967. Pp. xvii, 375. \$8.50.

Throughout its history—at least until the mid-twentieth century—European civilization has been expansionist and evangelical. In the Middle Ages it spread south to the Italian and Iberian peninsulas, west to the British Isles, north to Scandinavia, and eastward across the German and Polish plains to Russia and even, for a time, to the Near East. In the fifteenth, sixteenth, and seventeenth centuries it spread across the oceans to the Americas and set up outposts on the shores of Africa, Asia, and the islands off East Asia. In the last two centuries the carriers of European, or Western, civilization extended and intensified their pressures on other cultures and civilizations. In keeping with their evangelical tradition Westerners tried to impose by persuasion or force—frequently with considerable success—their culture, their religion, their technology, and their economy on the alien peoples with whom they came in contact. Only the discords and fissures within Western civilization itself prevented complete domination of the world by the West.

This is a story of epic proportions. Few other than authors of textbooks have dared attempt to tell it. Even segments of the story have their pitfalls. Professor Woodruff tells us in his preface that he was aware of them, but even he has not been able to avoid them. It is to his credit, in any case, that he has made the attempt. Perhaps his trial will stimulate other, more successful attempts. His plan is straight forward enough. Following a brief introductory chapter, he traces the course of European expansion and imperialism from the voyages of Columbus and Vasco da Gama to the mid-twentieth century. Then follow chapters devoted to European emigration, foreign investment, the diffusion of technology, the "conquest of distance" (which overlaps in part the two previous chapters), and international commerce. Each of these chapters is accompanied by a tabular appendix which presents in summary form the principal statistical and some other factual information relevant to its subject. In a short "epilogue" he presents his ideas on the philosophical implications of the story.

The principal contribution of the book is that it brings together in one place a great deal of the factual information relevant to the subject of European expansion in the last two centuries. Unfortunately, since the author relied almost exclusively on secondary sources, some of the information is incorrect, contradictory, outdated, or otherwise misleading. For example, he exaggerates "the destruction of Spanish sea power . . . in 1588" (p. 19); he incorrectly states that "real wages rose relatively less in the United States in the period 1860-1913" than in the principal European countries (p. 67); he repeats the discredited myth of the security-conscious French investor (p. 117); he im-

plies that European technology was in a state of "relative retardation" with respect to the rest of the world as late as the beginning of the eighteenth century (pp. 165-66); and he appears to believe in the long-outmoded theory of geographical determinism (pp. 166-67). On page 226 one reads that "railways crossed the great mountain ranges of the Andes . . . and opened up the remotest part of every continent," but on page 231 we are told (correctly) that "in 1960 three quarters of Latin American territory had no railway communications at all." And what is one to make of the statement (p. 192) that in the U.S. iron industry "modern techniques followed closely, if tardily, upon European developments"?

The tabular appendices, on which much effort was apparently expended, and which might have constituted the most important contribution of the book, are quite disappointing. Much of the information is derived from easily accessible standard reference works. Table V/6, "Electrical Energy, 1960" for example, is reprinted directly from the United Nations *Statistical Yearbook, 1962*. (The relevance of this information to the "impact of Western man" is not entirely obvious.) The tables relating to foreign investment are especially misleading. Table IV/1, the key table, lists gross but not net investments; this results in showing Sweden, Canada, and the United States as creditor nations before World War I, when in fact they were net debtors (Canada still is). On the other hand, some important creditor nations, such as Belgium and Switzerland, are omitted altogether. Moreover, the estimates of investment (and of foreign commerce in Chapter VII) are garnered uncritically from a variety of sources with little attempt to systematize or reconcile their differences. The internal contradictions in the tables of foreign commerce are especially glaring. The appendix to Chapter VI, on the diffusion of technology, simply lists the dates and a few particulars of notable inventions, discoveries, and applications in the fields of agriculture, mining, industry, etc.

Professor Woodruff has not succeeded in reconciling in his own mind the precise outlines and limitations of his subject. The very title of the book reveals its ambivalent character, linking "Western man" with European influence; at many points it is difficult to know whether the inhabitants of the United States, the British dominions, and Latin America are regarded as agents or as objects of Western influence. Part scientific popularization, part textbook, and part scholarly treatise, the volume is not notably successful in any of these genres. There is also a confusion with respect to the geographical and cultural scope of the volume; although apparently devoted to the consequences of European expansion for the rest of the world, much of the text in fact deals with purely internal European development. A similar confusion exists with respect to its contents, partly a catalogue of facts and figures, partly a philosophical essay on the theme, "whither civilization?" The latter ambivalence carries over into the author's prose style as well. Three examples will suffice to illustrate (and will illustrate as well the author's—probably unconscious—ethnocentrism): "More recently techniques have been developed for the recovery and utilization of uranium, titanium, beryllium, zirconium, and vanadium, used in nuclear reactors, etc." (p. 181); "The great metal birds that wing their way across the sky, or the voices that splutter and

hiss through the ether, remain part of the white man's magic" (p. 250); "It is a world of growing enlightenment. The European often brought light where prior to his intrusion there was darkness. . . . It is easy to be dazzled by the marvels of modern communications, but at least the white man's conquest of distance has made the world more fully aware of its collective dilemma" (pp. 336-37).

RONDO E. CAMERON

University of Wisconsin

The Dynamics of Growth in New England's Economy, 1870-1964. By ROBERT W. EISENMENGER. New England Research Series No. 2. Middletown, Conn.: Wesleyan University Press, 1967. Pp. xv, 201. \$9.00.

The New England region is poorly endowed with raw materials; it is distant from most consumer markets in the United States; and it has an unaccommodating climate. How then does it experience continuing growth, and what factors may account for its per capita income being higher than the national average. The documentation of this puzzle, and its solution, constitute the subject of Dr. Eisenmenger's book.

One of the main factors offsetting the disadvantages of the region, according to the author, is lower wages (or, more highly-skilled employees at the same wage rates). Lower labor costs can lead to more labor-intensive production processes or to a more labor-intensive industry mix. Eisenmenger's calculations suggest that two-thirds of the labor intensity differential between the United States and New England is simply due to differing output composition. The additional important elements in the region's growth are the fondness of business executives for living in the area, and the presence of agglomeration economies accruing from the high degree of urbanization in the region.

Although the author weaves supporting evidence into his presentation, this reader does not feel that all aspects of the thesis have been rigorously proved. Consider the alleged disadvantage of being distant from consumer markets on which the author elaborates in Chapter II. As Eisenmenger recognizes in Chapter VII, New England manufacturers are well situated with respect to the northeastern population belt. Besides, how important *quantitatively* is the transportation disadvantage, and how, if at all, has it been affected by the postwar trucking revolution?

Neglect of the quantitative dimension of locational factors is also exhibited in Eisenmenger's discussion of the residential preferences of executives and their role in the region's growth. He says: "Most manufacturing firms in New England are managed by men who were born or raised in the region where their manufacturing facilities are located. Unless their firms are failing, these men do not consider relocating to improve their profit position" (p. 99). Regardless of whether the statement is factually correct, and no evidence for such a sweeping generalization is found in the book, readers would have liked to know whether these firms are not, typically, the very small ones, and, if so, whether their contribution to New England's growth is quantitatively significant.

One important locational factor which receives only perfunctory treatment by the author is agglomeration economies. Perhaps one could not expect a full treatment of this subject in a volume devoted to general consideration of the economy of the region. However, the four paragraphs devoted to this question fall far short of this reader's expectation in the light of the importance of the subject.

On the positive side, Eisenmenger has succeeded in presenting a generally interesting study of the New England economy. In doing so, he relied heavily on traditional price theory and incorporated a great deal of current and relevant data into his analysis. This approach has produced a book which will be helpful to students of regional economics.

MATITYAHU MARCUS

Rutgers-The State University

Statistical Methods; Econometrics; Social Accounting

Mathematics in the Social Sciences and Other Essays. By RICHARD STONE. Cambridge, Mass.: M.I.T. Press, 1966. Pp. xiii, 291. \$12.00.

This is an excellent collection of essays reflecting the usual high standard of work from Mr. Stone. Anyone seriously interested in the application of econometric methods, model building, and theoretical reasoning to problems of national planning will benefit from reading these essays.

The title of the collection is misleading. A better synopsis of the contents might be: Econometric and other Mathematical Methods for Economic Planning. Seventeen essays are presented. The first (and possibly easiest to read) is entitled "Mathematics in the Social Sciences"; the remainder have titles such as: "Three Models of Economic Growth"; "Models of the National Economy for Planning Purposes"; "Transitional Planning"; "A Dynamic Model of Demand"; "The Changing Pattern of Consumption"; "Multiple Classifications in Social Accounting"; and so forth.

Except for the first and possibly the next two essays the collection comes so close to being a book that in this reviewer's opinion it is a pity that Stone did not see fit to do the rewriting that would have added the extra coherence.

The first essay appeared in *Scientific American* and has the strengths and weakness of an article at that level of exposition. It is an excellent summary and reminder for those who already know a great deal. It only pays the usual economist's lip-service to other social sciences (which is probably all that can be expected in seventeen and a half pages).

The second essay on "The *A Priori* and the Empirical and Economics" makes an eloquent case for the importance of mixing both an empirical and theoretical approach to economic problems. It is excellent reading for the fledgling model builder.

Undoubtedly economists should be (and once were?) men of broad education, but given their lack of linguistic ability, footnotes translating the French and Greek quotations might help οἱ Πολλοί.

In his essay on "The Housekeeper and the Steersman" Stone touches on cy-

bernetics and the problems of control and feedback. I found the treatment somewhat slight. Hardly any discussion of simulation was given. Furthermore here and in the subsequent essays little if any consideration is given to the co-ordination of planning models with planning processes. Economic theorists, econometricians, administrators, data gatherers, and policy makers must all harmonize in an overall systems approach if the type of work envisioned and pioneered by Stone is to succeed. He is undoubtedly acutely aware of this, yet this message is at best only tangentially implicit in these essays.

"Misery and Bliss," an article on population savings and consumption, provides a fine example of Stone's ability to combine his powers as an applied economist and a theoretician in building models to contrast positive and normative theory. The Modigliani and Brumberg saving function is used and policy implications are derived from a simple differential equation model.

In the ninth essay, "A Model of the Educational System," a dynamic program is formulated. It is obvious that for long-range planning socio-economic factors such as education must be taken into account; however, as is pointed out, one of the most difficult problems to be tackled is how we wish to define the educational process. (For example, should an educational cost be imputed to being brought up in a stable and literate home?)

Several subsequent essays deal with regional accounting and the multiple classification of social accounts. An excellent exposition of the problems involved and a plea for a unified system of social accounting are both given.

The collection ends with "British Economic Balances in 1970," calculated from the model of British economic growth developed by the Department of Applied Economics at Cambridge.

The collection of essays serves a useful function: the elegance and apparent ease with which Stone is able to model economic or socio-economic processes serves to whet our appetites. It is to be hoped that this is followed by a more unified book on planning models and planning processes.

M. SHUBIK

Yale University

The Meaning and Measurement of the National Income and of Other Social Accounting Aggregates. By EDGAR Z. PALMER. Lincoln: University of Nebraska Press, 1966. Pp. xiii, 417. \$10.95.

The field of social accounting is not oversupplied with good, up-to-date texts that satisfactorily provide—among other things—review of past developments and present trends in the field; detailed discussion of technical points and controversies, including reference to the relevant literature; sharply focussed comparison of social accounting practices in different countries; and a generalized approach to the subject that puts its various subelements into perspective and facilitates the exposition and understanding of specific points. To anyone who attempts to teach a social accounting course that goes beyond the barest rudiments, there is considerable interest in the appearance of a book that, according to its preface, "attempts to condense [the] discussions" in the literature, to bring "into focus many of the controversial questions in the field," to emphasize "the development of the concepts and measurement of so-

cial accounting," to look critically at many of the concepts and practices, and to provide a "vision [of] a complete and interlocking system of social accounts. . . ." There is a real need for a book that could deliver this package. It is, therefore, disappointing to find that Professor Palmer's book does not adequately fill the need. Although there are several sections that should be of considerable interest both to the student and to the expert, the book as a whole has certain deficiencies that keep it from being the long-sought-for, modern, comprehensive textbook in this field.

One problem is that the exposition is not always up to date; and to a greater extent than one would normally expect from a 1966 publication date. One gets an impression of a book basically written or thought through at various earlier periods in the past, with discussion of subsequent developments not really well integrated into the body of the book. For example, at several points, past developments and controversies seem to be discussed to a great extent as they were lived through, that is, without the benefit of the insight and explanatory power that might be provided by putting them in the perspective of subsequent developments in the various issues. Similarly, little advantage is taken of the power and discipline of the use of a comprehensive and articulated sector accounting structure in facilitating the explanation, clarification, and comparison of concepts. Indeed, despite frequent references to the matter, the book does not, in general, adequately expose the reader to the approach, which plays such an important role in recent work in the field, of dealing with social accounting concepts within such a schematic framework. The title of the book, with its emphasis on specific aggregates rather than on structure, is indicative of its tone in this respect.

Even in the sections dealing with the various social accounting systems currently put out in the United States—income and product, input-output, flow-of-funds, balance of payments, the descriptions are not always up to date. The basic discussions are in terms of versions of the systems other than the current ones. While mention is made of some of the changes that have occurred since the version described, in no case is it done systematically or completely. To some extent, of course, this may be due to the time lag between submission of final copy and publication date, but this is not the whole explanation since the book does contain certain references to publications of even later date than some of the slighted developments. As an extreme example of this, in the case of the flow-of-funds accounts, the description is in terms of the 1954 version and the author mentions only a few minor aspects of the considerable changes made in 1959, and to much lesser extent in 1962 and other years, when the current system was established.

There are other instances of inadequate discussion of recent developments and literature. For example, although the author expresses great interest in closer structural integration among the existing published sets of accounts, he neither makes adequate reference to relevant discussions that have appeared in the literature, nor mentions the extent to which changes in the published sets of U.S. accounts have been in the direction of closer integration. In this connection, as in several others, there is no mention of the Conference of Eu-

ropean Statisticians, which has been a major forum for developments in social accounting in recent years.

Despite the broad coverage set forth in the preface and attempted in the body of the book, one gets the impression that the author's real interest in social accounting (as indicated also by the book's title) is in one particular aspect—the definition of income. A large part of the book is devoted to a discussion of various problems in the concept of income, both in national and private accounting. Some of his suggestions for changing the boundaries of income concepts (as well as similar suggestions on the boundaries of production and capital formation) are of interest, although the presentation and terminology sometimes make it difficult to be sure of the exact consequences of his suggestions for all aspects of the structure of accounts. The discussion is occasionally marred by a tendency to mix together points and quotations relating to the definition of income for a variety of purposes—e.g., taxes, corporate management, measurement of national productive activity, income distribution, consumption functions—often with no hint to the reader that what is relevant for one purpose may not necessarily be so for another.

Because of these and other problems in the presentation, the book is probably not completely suitable as the general textbook for specialized courses in social accounting or as a supplementary text on the subject in more general courses. Similarly, it is hard to see it, as the author hopes, as helpful to the average layman. On the other hand, specialists and advanced students in the field, who should not be thrown off by the omissions and lapses and the occasional unusual terminology, should find much to interest and stimulate them in various parts of the book—e.g., in the author's individual approach to many questions, in the attempt to present the historical development of various measures and concepts, and in the valuable insights on many particular points.

STANLEY J. SIGEL

Board of Governors of the Federal Reserve System

Economic Systems; Planning and Reform; Cooperation

Central Planning; Evaluation of Variants. By KRZYSZTOF PORWIT. Translated from the Polish by Józef Stadler and edited by Helen Infeld. New York and Oxford: Pergamon Press, 1967. Pp. vi, 200. \$3.50.

We learn from the preface that this book, published in Polish in 1964, had previously been circulated under a preliminary version and discussed at the Planning Commission, at the University of Warsaw, at the Polish Economic Association, and at the Academy of Sciences. Most of the prominent figures of the economic profession in Poland are said to have contributed by their advice to the final form of the text, while the author is a member of the research department of the planning commission. For all these features the book *a priori* attracts our attention.

Its purpose is to advocate a new methodology for the preparation of long-term plans. The author strictly confines himself to this important but narrowly defined subject. He does not suggest that, simultaneously to the new meth-

od proposed for planning, reforms should be introduced in the actual operation of the Polish economy. Neither does he devote any space to detailed informative facts on the present process of planning.

The main theme of the book is very well explained in the introduction; it runs about as follows. In making a long term plan, one implicitly or explicitly chooses among the various possible combinations of the available productive techniques. The present method of planning is not satisfactory from this point of view because it studies hardly more than one variant. The choice among various organizations of the productive sector should receive much more attention. This requires a complete rethinking of the planning methodology, all the more so because the development of modern calculation methods and equipment makes possible a much more rigorous approach than was possible before.

In principle the problem amounts to the solution of a large mathematical program. But the dimension of the latter and the wealth of information that it should incorporate are so gigantic that one cannot hope to solve the problem directly or even to pose it completely. To get around the difficulty, one should combine "complex calculations," a rather unfortunate expression to designate computations made at the central level on the basis of a simplified model of the whole economy, with a number of "sector calculations," each one dealing in more detail with the organization of one particular industry. Complex calculations will use relatively little information on the various industries, but they can embrace the whole set of mutual relationships in the economy. Sector calculations must take as externally given the information concerning the elements outside of the particular sector, but they make possible a more detailed analysis of the data within the sector.

The idea that information must be exchanged between the central planning commission and the industries or smaller units is certainly not new in the literature of Eastern Europe. But Porwit has a particular notion as to how it should be organized. The complex calculations should involve an aggregate linear program and permit the determination of dual prices for groups of commodities. Such dual prices would then enter as given parameters in the sector calculations.

Conveying these propositions is the main object of the book. In order to make them clear, the author thinks it necessary to explain how the choice of a plan amounts to the solution of a mathematical program and what are the significance and exact nature of dual prices. Moreover he considers his proposal as being so substantial as to imply a complete reorganization of planning procedures and therefore as to require intermediate measures during a transition period. This explains the organization of the book: chapter 1 carefully formalizes the problem of long-term planning as an activity analysis linear program; chapter 2 deals with dual prices—it considers particularly those referring to the constraints on investment and on foreign balances; chapters 3 and 4 examine respectively the complex and sector calculations; chapter 5 discusses the problem posed by the progressive introduction of the proposed method.

As an exposition of its main proposals, the book is excellent. Porwit knows

perfectly both the theoretical and practical aspects that are relevant to his purpose. He explains them with great pedagogical qualities and with a good sense of the exact validity of each point. This is true even of the slippery paragraphs 2.6 and 2.7, where he attacks two difficult questions: (i) the discussion between W. Nyemtchynov and L. Kantorovitch on the significance of sector calculations, and (ii) the relation between dual prices, the "law of value" and actual prices in a competitive economy. Such a competent, serious and well-balanced work should have an impact on the economic thinking in Eastern Europe, especially among the young generation. Since he essentially agrees with the author, the reviewer warmly welcomes the publication of this book.

Those among the Western scholars who will be interested in Porwit's theory of long term planning may, however, notice two weak points of his discussion.

In the first place he is much too brief on the difficult choice of the capital stock to be kept in existence at the end of the plan. This is said to be "externally given" in chapter 1; the aggregate model of chapter 3 relies on capital-output coefficients; but there is no serious discussion of the problem. The unaware reader will certainly not appreciate its difficulty and not realize that it seriously matters for any attempt at decentralization in the process of plan making.

In the second place, Porwit does not spell out precisely the exchange of information between the central program and those dealing with the sectors. How, if at all, are the results of the sector calculations to be taken into account in the further complex calculations? Why should the consideration of prices based on incorrect information result in efficiency in sector calculations? Does he view the process as repeating itself several times? If so, how will it proceed? The reviewer believes these nontrivial questions are important if we want to make real progress in the theory of planning.

However, if such theoretical points are raised, it is because the book finds its place in the library of modern economic science. These questions are intended to invite us to go more deeply into the problem tackled by Porwit. In its present form, his monograph is already perfectly suited to help develop the communications between Eastern and Western scholars. Let us hope that many of the same quality are produced in the years to come.

E. MALINVAUD

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Planning, Profit and Incentives in the USSR: Vol. I. The Liberman Discussion—A New Phase in Soviet Economic Thought. Edited by MYRON E. SHARPE. White Plains, N.Y.: International Arts and Sciences Press, 1966. Pp. xiii, 314. \$10.00.

This volume collects 31 translations of Soviet articles on the Liberman Proposals and other programs for economic reform in the USSR. The articles are arranged chronologically in three sections. The first two present the development of Professor Liberman's approach and nine responses, *pro* and *con*, to the formulation which has become known as "The Liberman Proposal." The third is a more heterogeneous assembly of 17 later pieces, dealing less

specifically with Liberman and more generally with the role of profits and prices in workable approaches to decentralization. Several of the articles in this section deal with a 1964 experiment in the clothing industry (since greatly expanded) under which the firm's product mix is determined not by orders received from above but by "direct contacts" with trade organizations (and thence with the market place).

The collection offers a good cross-section of one broad stratum of the economic controversy of 1962-65. Although it may be imprecise to describe this level as journalistic (14 of the contributions are taken from *Pravda*, *Izvestiia*, and other daily newspapers, and five others come from primarily political periodicals), the contributors very rarely penetrate to the efficiency implications of alternative administrative arrangements. In part, this reflects the effort of some of the reformers, including Liberman, to separate the implementation of central directives from their origination; they would rather not be suspected of favoring the chaos of the market over centralized, planned guidance of the socialist economy. The discussion, however, soon makes clear that this separation was not easily achieved, and that broader questions of allocative decision-making could not be avoided in the discussion of decentralized implementation. The host of Soviet contributors to this deeper issue are represented in this collection only by the late V. S. Nemchinov and by N. Fedorenko, both leading mathematical economists. Here too, however, the selections were intended for mass consumption.

The focus, then, is upon improving the implementation of central directives, primarily through rationalization of the incentive system. Although tending toward repetition, the articles represent an extensive critique of the Stalinist system, which, all participants agree, has outlived its usefulness. The problem, essentially, stems from basing managerial bonuses primarily on the fulfillment of physical output targets and the basing of those targets, often rather mechanically, upon previously achieved levels. The system generates an over-riding preoccupation with short-run quantitative performance and, more damaging, a strong tendency on the part of the firm to conceal its true capacity from their superior organs. Much of the material on this disfunctional behavior will be familiar from the writings of Berliner, Granick, and Nove, but these detailed observations from the horse's mouth provide authoritative, contemporary insight into the treacherous environment of the Soviet plant manager.

If often lacking in theoretical interest, the discussion is pleasantly free of ideology. One observes only a dogmatic subscription to pricing on the basis of socially necessary labor time, although the need for exceptions is granted and, more important, the definition of this fundamental Marxian variable remains a legitimate subject for debate. There is also a polemical defense of the use of the profit criterion against charges, of Western origin, of creeping capitalism.

So much for the general tone and content of the collection. Among the articles of particular interest are the early papers by Liberman, which provide a clue to the development of their author's thinking. The Liberman Proposals of 1962, to which most of the discussion has been addressed, have as their central elements: (a) retention of assigned output targets and of centralized allocation of critical inputs; (b) the transfer of much detailed operational deci-

sion making from the center to the firm; and (c) the introduction of "profitability," the ratio of profit to fixed and working capital (using Soviet definitions of costs, etc.) as the principal measure of managerial performance, source of managerial bonuses, and, therefore, as the principal objective of enterprise activity. This alteration in the role of the output target (combined with curtailment of administrative interference in plant management) would close the gap between the interests of the firm and of the State. Instead of concealing capacity in pursuit of a lighter output target, the firm would extend itself in pursuit of maximum profit. Profitability, then, is the key to decentralization: it assures rational exercise of the authority relinquished by the center to the firm. The apparent objective of the scheme is more efficient execution of centrally-set tasks, not the substitution of the market for the State Planning Commission in guiding resource allocation.

Liberman's earlier (1955) suggestion had the same objective and also relied upon profit as the basis of an improved incentive system; but here, the rate of profit was defined as a percentage of unit cost of production, the definition then standard in the Soviet Union. The idea was simple: detailed study of Ukrainian machinery plants had shown that the main source of reduction in unit cost was the spreading of overhead as output rose. If, therefore, unit cost as a per cent of price (or, what amounts to the same thing, profit as a per cent of unit cost) were made the main success indicator and determinant of managerial bonuses, the manager would be induced both to eliminate inefficiency in production and to seek higher production targets: profitability could be increased primarily by increasing output; output could be increased only by increased allotments of rationed inputs; such allotments would be granted only if higher output targets were accepted. By replacing the two dominant success indicators (physical output and cost reduction) by one, the incentive to seek lower output targets would be eliminated.

Why did Liberman abandon this approach? Several reasons suggest themselves: (a) not many industries may have been operating in the range of decreasing short-run average total cost; (b) to the extent that Liberman's cost data reflected economies of scale or changing production functions, strong pressure for increased investment would have been generated; and (c) profit rate targets for multi-product firms may not have proven as easy to generate as Liberman argued. This apparent failure notwithstanding, one may imagine Liberman persevering with the notion that in profit lay the "intersection," as later writers were to refer to it, of numerous variables then being separately planned *and* of the interest of the center and the firm.

Gatovskii's article, "The Role of Profit in a Socialist Economy," provides a concise statement of the position of what may be called the conservative reformers: the objective of reform is explicitly restricted to questions of implementation; profit should play a greater role (as success indicator, source of bonuses, and of capital accumulation) but it cannot serve as a guide to investment decisions (except in the choice between goods which are strong or perfect substitutes). In general, "... resources are distributed between branches so as to satisfy the totality of the growing requirements of society, in line, of course, with the available production potentialities."

But how the planners are to discover the latter—"The terms on which alternatives are offered"—Lange, quoting Wicksteed—is left unanswered (and unasked). The price system can play no role in the process since Gatovskii would adjust prices to eliminate disparities in profit rates, thereby also eliminating whatever weak and deceptive signals the latter may have represented. Gatovskii does allude to the possibility of a multiple price system which could provide useful information to the central planners, but this does not appear to be his intention. Like many of his colleagues, he is more concerned with the distributive than the allocative role of the price system. Allocation is a technical problem, to be treated in accordance with central policy.

The opposition denies that profitability can be trusted with the central role in the incentive system. As spread over the contributions of Zverev, Sukharevskii, and several others, a variety of arguments, of unequal merit, are made: (a) if, as intended, major allocations from firm profits are made to workers' bonuses and to decentralized investment, there is danger of inconsistency with central plans for the consumption and investment goods industries; (b) given the failure of Soviet prices to reflect the "importance" of different commodities, profit maximization will misguide what discretion the firm has over its output and input mixes (i.e., one pattern of misallocation will simply replace another); (c) under Liberman's scheme, each firm would be assigned a profitability norm which would become no less a bone of contention between the center and the firm than were the old output targets; and (d) in an economy as large and diverse as the Soviet, no single success indicator can be generally applicable.

In general, the critics rest their case with the identification of potential shortcomings. The rules of the game do not require consideration of the possibility that the proposed reforms represent an improvement over existing procedures. This may represent the "perfectionist bias" to which D. Granick has referred or, perhaps, an overriding distrust of any reduction in central authority over the firm. ("The chaos of the market place" may have more than ideological significance to potentially unemployed central planners.) In any event, the failure to compare outcomes renders this discussion less interesting than it might have been.

The articles by Nemchinov and Fedorenko offer the only cogent thought in the volume on the interaction between planning and implementation. The central point is stressed that decentralization by means of a rational price system facilitates rather than undermines central planning. Nemchinov draws interesting conclusions concerning the introduction of a capital charge, the limitation of central targetting to final rather than total product, and the substitution of State trade in intermediate products for the current centralized distribution system. While Nemchinov's general position will strike familiar notes with Western welfare economists, certain specifics raise doubts. Especially noticeable is the view of the capital charge and profits: profit norms are based on planned charges against fixed assets, the charges varying from branch to branch, and the charges are to be set in accordance with the branches' capital accumulation requirements. The capital charge, therefore, is a financing device (or income source) rather than a rationing device. On the practical level, Nemchinov's program presupposes rationalization of the price

system, an endeavor which seems to enjoy little appreciation and less support where it counts.

Fedorenko also argues for more effective central planning by substitution of "indirect levers" for administrative commands. He emphasizes the potential contributions of mathematical models, computers, and improved information flow to the coordination of economic activity. On the other hand, Fedorenko seems to share the view expressed by Belkin and Berman that not even the mightiest computers can save the centralist model from increasing inefficiency and waste. The major statement for the opposition is by G. Kosiachenko, who argues for the necessity of retaining (and improving) directive planning, increasing the role of profit in the economy, but abandoning all thought of decentralizing through a flexible price mechanism: "... any sort of automatic regulation of the enterprises' activities through price and profit fluctuations is out of the question under the conditions of a socialist economy, for that would mean a transition to spontaneous price formation." In part, his position seems to reflect a lack of appreciation of the possibility of harnessing "indirect levers" to planners' preferences; but he also scores valid points with his references to externalities. And so controversy proceeds: often repetitious and superficial, occasionally illuminated by institutional or theoretical insight; the "best"—which so preoccupies Western theorists—appears rarely, and then, most often implicitly, as the enemy of the better.

A concluding word on the translation. Clearly not being in command of economic terminology, the translators have all too often relied upon cognates and general dictionary definitions. The result is a liberal scattering of individual terms (e.g., "normative cost of processing") and whole sentences ("All material and technical resources are scattered among individual fund-holders . . . and individual detailed commodity positions.") which will tax the imagination of the nonspecialist reader.

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Langfristige Wirtschaftspolitik in Westeuropa. By KARLHEINZ KLEPS. Beitrage zur Wirtschaftspolitik, Vol. 5. Freiburg im Breisgau: Rombach, 1966. Pp. 524.

This book combines a survey of theories of economic planning with a report on past and present plans in the Common Market countries and the United Kingdom, Sweden, and Norway. French *planification* and the "social market economy" in Western Germany are analyzed in greater detail than the other systems. The last part of the book presents the special problems of economic planning for the European Common Market as a whole.

Kleps points out that there is a close relationship between basic concepts of economic theory prevailing at different periods and the economic policy measures taken by the industrialized countries of Western Europe and the United States. He calls the type of economic policy pursued by most European countries between the last decades of the 19th century and the great depression "Punktueeller Interventionismus" or phase I of government economic policy. A

more thorough discussion of this phase which was based on marginal utility theory and on Marshall's "Principles of Economics" would have helped young economists to get a better understanding of pre-depression theories of government economics. The next phase, called "Autonome Konjunktur und Beschaeftigungspolitik," or phase II, was stimulated by the problems of the great depression. This phase of economic policy and the economic concepts on which it was based are much better understood by contemporary economists than phase I. The book under review deals primarily with the policy which aims at promoting global growth, the so-called phase III. The author mentions the U.S. National Planning Association as the initiator of this policy which began to exert worldwide influence after 1946. Kleps does not concentrate on a comparison of growth rates, a topic more fully covered by Angus Maddison's *Economic Growth in the West*, but he describes the economic situation in which plans for each country were conceived and the special problems of several countries, e.g., regional problems in the Netherlands, the foreign trade situation in Norway, and the monetary and balance-of-payment situation in the United Kingdom. One of the strong points of the study lies in the presentation of many different approaches to planning. But this advantage is diminished by an overemphasis on the description of the instruments of planning. Kleps' book is weak in analyzing the regional problems of France and Italy that played a decisive role in the formulation of French and Italian policy. These problems are discussed more thoroughly in Barzanti's *The Underdeveloped Areas Within the Common Market*.

The author's economic evaluation of planning in general will satisfy those economists who are afraid of the trend towards *dirigisme* in all Common Market countries. Kleps does not agree with the views of conservative economists, e.g., J. Rueff, von Mises, the late W. Roepke and others, who are opposed to all economic policy measures except those designed to restore or sustain free competition. He envisages, though, the danger of increasing government intervention in all types of planning. While recognizing the merits of "Guidelines" or so-called *Indicative* planning as compared to centralized planning in a command economy, he points out that neither French *planification* nor the German "Social Market Economy" was able to cope with problems of stabilization. In contrast to those advocates of the French system who characterize *planification* as market research which supports rather than replaces the market mechanism, Kleps would fully agree with a statement by C. P. Kindleberger: "Information removes uncertainty but collusion removes it more thoroughly, either collusion with government or with other firms" (*National Economic Planning*, Universities-National Bureau Ser. No. 19, Princeton, 1967). In the last section of his book Kleps reviews several problems which face the Common Market Planning Board, especially questions of energy economics, transportation, and agriculture. The difficulty of finding a desirable average growth rate is aggravated by the fact that one country may aim at growth in agriculture while others have a faster growth of industry as their major goal. Even if the Planning Board were to accept E. F. Denison's conclusion ("Sources of Postwar Growth in Nine Western Countries," this *Review*, Proc., May 1967, 56, 325-32) that accelerated economic growth during the last fifteen years was

based to a large measure on the decrease of the labor force in agriculture in favor of increased employment in industry, governments would—for political reasons—still pursue a policy of subsidization of agriculture.

After discussing shortcomings of the "social market economy" in West Germany, especially the lack of coordination in energy policies and the insufficient allocations for education and health, Kleps suggests that one step towards greater economic stability could be found in a better coordination of financial policies of all public households (federal, states, and cities). He recommends that budgets should be formulated for several years. He finds that single-year budgets induce savings at the wrong places and are a cause of instability rather than instruments of sound fiscal policy. It seems that a number of the author's suggestions have been incorporated in the recent German "Economic Stabilization Law." Kleps and several other economists have voiced their dissatisfaction with monetary policies during 1966 which they hold responsible for the present "recession." It seems to this reviewer that a phase IV of economic policy may show a greater awareness of problems of stability in preference to policies to promote "growth at any price."

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Business Fluctuations

Indicators of Business Expansions and Contractions. By GEOFFREY H. MOORE AND JULIUS SHISKIN. NBER Occasional Papers No. 103. New York: National Bureau of Economic Research, Columbia University Press, distrib., 1967. Pp. 127. \$6.00.

An Appraisal of Short-Term Economic Forecasts. By VICTOR ZARNOWITZ. NBER Occasional Papers No. 104. New York: National Bureau of Economic Research, Columbia University Press, distrib., 1967. Pp. xii, 144. \$5.00.

These books are the first in a series of National Bureau publications which will deal with topics in the area of forecasting. The study by Moore and Shiskin aims to improve the statistical tools available to forecasters. It (1) updates the list of NBER cyclical indicators and classifies them by timing and economic process, (2) develops an explicit scoring system for selecting indicators, and (3) compares the new list of indicators with previous lists.

The complete list of indicators does not differ drastically from the 1960 list, with five series dropped and thirteen others added; but the short list of particularly significant series has been substantially altered. In using indicators for analysis, forecasters have always implicitly assumed that the economy's structure is sufficiently stable so that the indicators will present an accurate representation of the future in addition to providing historical information. It would, therefore, have been interesting to have had a more detailed *analysis* of the reasons why series, which previously had been considered useful, were eliminated from the lists.

The scoring system is based on the significance, adequacy, conformity, timing, smoothness, and currency of the data. Each criterion is defined with weights assigned according to a number of characteristics. The arbitrary nature of the weights is recognized, and independent observers might have assigned different importance to each item.

However, it is important to determine whether the scoring system measures each series' usefulness for current analysis. First, only *historical* data are examined for their timing and conformity relative to NBER reference cycles. The way these data would have appeared on a contemporaneous basis or the number of false reported movements are never examined. Second, the relative accuracy of the early data and the extent of the errors are neglected. For instance, on one criterion, statistical adequacy, the scoring system favors series which report weekly movements and which indicate the size of subsequent revisions. The retail sales series reports both; the nonagricultural employment series, neither. Partially for these reasons, the statistical adequacy is .77 for retail sales and only .61 for the employment series. Other research has, however, indicated that the average error in the first available retail sales figures is 100 per cent of the mean monthly change, while the nonagriculture employment series is much more accurate. While the overall score did favor the employment series, surely, the criterion, statistical adequacy, in an *ideal* scoring system, would have included *accuracy* as one characteristic.

The scoring system, thus, is not entirely related to the uses for which the indicators are intended. Nevertheless, another set of indicators has been provided for current analysis. Hopefully, the analytical quality of these series will be demonstrated.

Zarnowitz's study evaluates eight sets of *ex ante* forecasts for the period 1957-63. All the predictions were generated by judgmental techniques; most are annual forecasts, but some provide estimates for quarterly intervals, and the predictions of one group were prepared quarterly. Zarnowitz has developed a large number of measures of the errors between the forecasts and the actual data. The discrepancies between the actual and predicted levels as well as between the actual and forecast changes of GNP, its major components, and industrial production were calculated. Zarnowitz correctly recognized that the difference between the predicted and actual changes is more meaningful than the discrepancies between the levels. This point might have been emphasized and less attention might then have been devoted to the level predictions, for forecasters should regard their task as predicting changes and not the data revisions which occur between the beginning and end of a forecast period.

The forecasts of the change in GNP displayed mean absolute errors which were more than 40 per cent of the mean absolute change. However, the GNP forecasts were still better than the predictions of some components, especially in the investment area, thus indicating the presence of offsetting errors. The types of discrepancies, underestimates, overestimates, and turning-point errors were all examined both for the annual forecast and the shorter span and intra-period forecasts. The root mean square errors of the forecasts were compared with similar measures generated by the "forecasts" of three naive models.

It is impossible to summarize all the results, but generally the GNP forecasts

outperformed the naive standards, while some of the sectoral forecasts were substantially poorer. Forecasters generally underestimated changes, and failed to recognize many turns, with both results in accord with previous findings. There was also no systematic uniformity between the size of the errors and the length of the span over which the forecasts were prepared.

This book should not be viewed merely as a historical record of the performance of particular forecasters. Rather the study should be considered as providing some valuable information for determining how forecasting techniques can be improved. First, the results of these judgmental forecasts can be compared with the projections of econometric models. This would enable us to determine which approach is better. Second, although Zarnowitz de-emphasizes this point, there is substantial variation between the records of the various forecasters, even though all used judgmental procedures. If it were possible to replicate the forecasting techniques of the *best* forecasters, it might be possible to improve forecasting procedures in general. Zarnowitz indicates that (1) the use of anticipation data improves the forecasts and (2) expert knowledge of particular sectors improves the projections of activity in these areas. Neither result is surprising, but both have implications for the development of forecasting procedures and the establishment of groups which make predictions. These findings plus Zarnowitz's competent handling of the statistical material make this a study which all economists who are interested in forecasting should read.

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The American Economy to 1975—an Interindustry Forecast. By CLOPPER ALMON, JR. New York: Harper & Row, 1966. Pp. xi, 153. \$9.50.

Professor Almon has documented an important milestone in the methodology of long-range economic forecasting. His book describes the first full-scale demonstration of how long-run employment and interindustry demand projections can be integrated with input-output analysis to produce mutually consistent forecasts of the American economy.

The main contribution of this work is "to integrate these parts into a unified, comprehensive, usable forecast of the whole economy" (p. ix). The final forecasts (not published) consist of the constant dollar sales of each of ninety industries to each of 144 markets for each year from 1963 to 1975, including intermediate demands, capital equipment investment, construction and ultimate use. These forecasts are summarized in a Master Table (pp. 153-70) which shows the fraction of the sales of each industry which will go to the various markets in 1975 and the growth rates of the various markets. Two additional rows on the Master Table show the distribution and growth rates of employment.

This comprehensive set of invisible industry sales forecasts is developed in two parts. In the first part, judgmental forecasts of government expenditures to 1980 and judgmental forecasts of exports and competitive imports to 1975 are produced. These forecasts assume no increase in defense and space spending beyond 1964 levels, with defense procurement actually declining

about 1 per cent per year, and exports and imports continuing a rapid but balanced expansion.

The second part involves a series of computerized iterations performed on the inputs to single-equation "structural forecasts" of each industry in order to determine a level of disposable income and consumption that is both consistent with the deduced investment, construction spending and inventory accumulation of each industry, given its sales, and at the same time produces a level of total product matching an independent projection of full-employment output. One cannot but admire such a grand conception and marvel at its successful completion.

Nevertheless, any potential user of these results must tread carefully if he is to avoid the pitfalls of credulity created by such a computer-maintained consistency and the quicksand of choosing to ignore the implications of Almon's clearly stated assumptions.

Consider, for example, the determinants of the level of the independently projected full-employment output to which all final industry projections are matched. These levels are calculated for each forecast year as a product of projected labor force productivity times a fully employed labor force. However, the industry labor productivity projections are all simple exponential extrapolations of historical trends from 1947 to 1963. "All the trends in automation, education, and shifting job structure in each industry are simply assumed to continue into the future. In particular, we have not investigated the effect of capital investment by an industry on its rate of change of productivity" (p. 128). Furthermore, full employment of the labor force (unemployment of about 4 per cent in 1970 and 1975) is to be obtained, it is assumed, by proper government policy.

The labor force projections, in turn, are developed by multiplying the projected age-eligible population by their projected labor-force participation rates. The population projection used in both the forecasts of labor force and consumption spending is "a moderately high series" (p. 120) selected from among several provided by the U.S. Census Bureau. The labor-force participation forecasts are BLS trends based on the assumption that future changes in participation rates will proceed at about 50 per cent of the rate of change from 1948 to 1964.

There is, of course, nothing particularly "consistent" about these assumptions (except, perhaps their optimism). The consistency of which Almon speaks concerns only the industry interactions *after* these technological and labor-force assumptions have determined the overall level of production and employment, given the judgmental forecasts of government expenditures, exports and competitive imports, and specification of the numerous single-equation structural models.

Amid the complexities and multitudinous assumptions of this important long-run forecasting document one searches for a relatively simple conclusion—a critical test of the quality of this particular formulation of the Almon theory of consistent long-run econometric forecasting. One such simple prediction derived from the specific quantitative theory presented in *The American Economy to 1975* is the projected rate of growth of disposable per capita income se-

lected by the iterative computer procedures as satisfying both the input-output consistency requirements and the full-employment final output projections. The specific Almon theory boldly predicts that disposable per capita income "is projected to grow 2.7 per cent per year instead of the 1.7 per cent per year which it grew between 1950 and 1963, or the 0.7 per cent per year it averaged during the 1956-63 period" (p. 47). "In the decade ahead the yearly growth of (disposable per capita) income will run more than double that of the last" (p. 25).

For the quality of the forecast conception, and its lucid, literate and frank presentation, we are deeply indebted to Almon. For the quality of the forecast proper, until time brings forth its own corroboration, we can only form our own private Bayesian judgment of credibility.

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Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

The Term Structure of Interest Rates—Expectations and Behavior Patterns
By BURTON GORDON MALKIEL. Princeton: Princeton University Press,
1966. Pp. xxi, 271. \$8.50.

In this volume, Malkiel develops an expectations theory of term structure which includes some elements of the hedging pressure and segmentation theories. In the first five chapters, he reviews the previous work in this field, reformulates the expectations theory so that it focuses on bond prices, and extends the theory to incorporate transactions costs. Malkiel then presents some evidence that the behavior assumptions underlying the expectational theories do not make sufficient allowance for institutional practices. The synthesis he develops is presented in a partial-equilibrium model which takes account of these institutional factors and also allows for some flexibility in supply.

Malkiel begins his analysis with several theorems on bond prices to show that for a given uniform change in yields the absolute change in bond prices increases, but at a diminishing rate. This suggests that prices of long term bonds of different maturities may behave more alike than will prices of short term instruments with different maturities. He next introduces the hypothesis of "a normal range of interest rates" which he uses to determine the first and second derivatives of the yield curve. He simplifies the analysis by assuming a linear utility function so that he works with the mathematical expectation instead of the expected utility. When interest rates are near the upper bound of the normal range, investors will move into long term securities and the yield curve will be negatively sloped. Conversely when interest rates are at the low end of the range, the likelihood of capital losses exceeds that of capital gains and investors go short and the yield curve is positively sloped. In either case, the price changes for most longer term bonds are roughly similar so that whether the slope of the yield curve is positive or negative it will tend to flatten out. The third step in his analysis is to introduce expectations explicit-

ly by assigning a subjective probability to the direction of interest rate changes. For example, at a time when interest rates are near the high side of the range, the market may assume that there is a high probability that interest rates will fall. The introduction of explicit expectations in this model affects the first derivative of the yield curve but does not generally change the second derivative.

This is the basic model. Malkiel shows how it can easily be modified to allow for the fact that short rates may fluctuate more than long rates, to remove the assumption of an invariant normal range of interest rates, and to include a Hicksian liquidity premium.

This expectations theory differs in some respects from the traditional theory. In the neo-classical Fisher-Hicks-Lutz theory, investors choose between short and long term securities on the basis of their expectations of future short term rates for an indefinitely long period. Malkiel assumes that investors think of a normal range of interest rates, plan in terms of a short horizon, and compare alternatives for this short period. In his model investors compare short term forecasts of long term interest rates unlike the investors of the traditional model who compare long term forecasts of short term interest rates. The two models lead to the same results if we assume that expected holding-period yields are equalized for any one holding period. In this case it can be shown that the market is implicitly estimating a set of forward long term rates as well as a whole series of forward short term rates.

In Chapter 4, Malkiel presents results of several empirical tests attempting to predict the differential between long and short rates. The basic idea underlying these tests is that when interest rates are high relative to their normal range, the short rate will tend to be high relative to the long rate. To avoid serial correlation and other biases, the author develops several formulations of the basic regression equation and tests these with three different interest-rate time series. The results all seem to indicate that short rates are more volatile than long rates and that the spread is a function of the gap between the current long rate relative to its normal range. These findings are interesting and suggestive and are consistent with the expectation theory he outlined. Although the author does not define explicitly the substantive content of an expectations theory, he nevertheless asserts that his results constitute independent evidence supporting the expectations theory; but since he does not identify the forecasting requirements on forward rates that distinguish an expectations theory, it is not clear how these tests discriminate among different expectation theories, or between expectation and non-expectation theories.

One of the most interesting chapters in the volume is Chapter 5, where Malkiel analyzes transactions costs and shows that their effects are likely to be felt mostly in the very early maturities. For this reason transactions costs may generate a "humped" yield curve from a curve that would otherwise descend monotonically. He also suggests that transactions costs could account for Kessel's finding that the bias (the amount by which the forward rate overstates future spot rates) varies over the cycle. In Chapter 6 Malkiel presents aggregate, cross-sectional data on institutional holdings and interview data to test the behavior assumptions of the expectations theory and the segmentation the-

ory. He finds that while most bond investors are not indifferent among maturities offering the same holding period yield, they do nevertheless substitute among maturities over a wide range of the yield curve. He also finds evidence that institutional investors have maturity preferences, diverse (non-uniform) expectations and that professional speculative activity may be circumscribed.

Malkiel concludes that neither of the two theories is satisfactory, and therefore attempts to synthesize the expectations and institutional theories in Chapter 7. He modifies the expectations model by introducing diverse expectations, transactions costs and maturity preferences and shows that these factors allow relative changes in supply to affect the term structure. But as expectations become more uniform, the term structure becomes less sensitive to changes in relative supplies. In the Appendix he presents a careful analysis of the circumstances in which a humped yield curve is likely to emerge.

He concludes his study with an analysis of "operation twist." Studies by Okun, Scott, Wood, and Modigliani and Sutch do not leave much scope for changes in relative supplies. But he is not entirely convinced by these studies because the effect of Treasury and Federal actions on the debt was not entirely consistent with the objectives of "operation twist." In addition he indicates that some evidence in favor of "twist" could be derived from his own empirical work and that of Wallace. He concludes that whatever changes in the yield structure did occur very likely resulted from changes in regulation Q and the effect of "twist" on the formation of expectations. This is a disappointing finding in light of all the intellectual effort that has been devoted to term structure theory.

This volume provides a systematic treatment of the subject, covering both the analytical and institutional aspects, and should prove to be especially helpful to students of monetary economics.

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Household Capital Formation and Financing 1897-1962. By F. THOMAS JUSTER. National Bureau of Economic Research Series No. 83. New York: Columbia University Press, for National Bureau of Economic Research, 1966. Pp. xiv, 146. \$6.00.

This volume represents another addition to the National Bureau's continuing research program in consumer credit, a field in which it has pioneered for many years. In this monograph Mr. Juster offers a concise quantitative analysis of household capital formation (expenditures on household durable goods and household construction) and of certain global aspects of the means of financing it. Beyond the mainly quantitative approach, the author provides a limited amount of theoretical analysis, including an examination of certain important conceptual problems.

Initially, he elects and defends treating purchases of household durables as investment. He then examines trends in household capital formation and relates them to trends in business capital formation. In contrast to the persistent decline in the ratio of gross business investment to GNP, reflecting essentially

a relative decline in investment in business structures, Juster finds that gross expenditures on major household durables and household construction constitute a rising ratio of GNP. Indeed, whereas in the earlier part of Juster's survey period, the business sector had a strongly dominant share in gross capital formation, toward the end of the survey period the household sector had become slightly more important than the business sector. Moreover, the secular decline in the ratio of gross capital formation to GNP (when the former is confined to the business sector) disappears when the ratio is recomputed to include household capital formation and government civilian construction. If expenditures for research and development and for education are included in the numerator, which Juster considers appropriate for analysis of growth, there is a clearly rising over-all ratio. Juster also takes note of the apparent understatement of both business and household capital formation, mainly in the construction components, because of deficiencies in indices of construction costs. As a result, previous estimates of the ratio of capital formation to GNP in constant dollars may have a considerable downward bias.

In consumer credit the expected basic trends are confirmed: growth in installment credit outstanding has substantially exceeded that in the stock of durable goods and there has been a large rise in the ratio of household debt to household assets. The ratio of household debt to business debt has risen sharply, with recent changes in the absolute amounts of each form of debt reaching near-equality. Juster finds that "a major cause of the expansion in [household] credit use has been a persistent secular relaxation in credit terms" and suggests that "the secular relaxation in credit terms is best interpreted as a secular decline in the effective cost of borrowing."

A cyclical analysis follows. Over time Juster finds declining relative cyclical amplitude in business capital formation. He also finds absolute cyclical amplitude in household capital formation shifting from a fraction of that in the business sector to an amount exceeding it, with non-structures of major importance in both areas. At the same time, absolute cyclical amplitudes in short-term and total credit are found to be similar for both business and household sectors in recent years. Adding timing relationships, Juster finds—as others have before—evidence of a causal link running from consumer credit to consumer outlays to business cycles. Thus he is led to the conclusion that an "obvious implication of these findings is that analysis of business cycle movements should be at least as much concerned with the behavior of households as with the behavior of business firms."

This work presumably is not intended to make its mark in terms of presentation of mainly new or original materials: its *raison d'être*—and a good one indeed—is its presentation of a convenient, up-to-date summary and interpretation of the basic data in the areas covered, plus a restatement and vigorous defense of recent thinking with respect to underlying concepts, to which Juster himself has made a number of contributions. Juster does this well and so reinforces the case for certain changes in conventional thinking, particularly with respect to the concept of capital formation, while making it easier for workers in the field to push on further. His findings and analysis offer a challenge to

business cycle theorists. They will also be useful to makers of economic and financial policy.

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Regulation of Interest Rates on Bank Deposits. By ALBERT H. COX, JR. Michigan Business Studies Vol. xvii, No. 4. Ann Arbor: Bureau of Business Research, University of Michigan, 1966. Pp. xii, 207. \$6.00.

Since the advent of high interest rates in recent years, discussions on the constraining effects of legislated ceilings on bank deposit rates have become topical. The present book adds to a greatly needed discourse on the role and effects of the regulations governing the maximum rates allowable on bank deposits.

The author commences by tracing the arguments which supported adoption of the regulations on bank deposit rates and finds the dominant argument to be that excessive competition among banks threatens the solvency of the banking system. This proposition of "destructive competition" raises two questions: (1) does economic analysis suggest that destructive competition is inevitable, and (2) has there been any empirical evidence indicating that destructive competition existed in the past when deposit rates were unregulated.

The inevitability of destructive competition is dismissed by the author as he claims (without the benefit of supporting evidence) that the market structure of the banking sector is an oligopoly-oligopsony structure and hence not one in which destructive competition will prevail. Somewhat more complete is the author's inquiry into the question as to whether rate competition on deposits has ever been a major cause of bankruptcies. His sincere efforts to obtain data which would cast some light on the question are apparent. Unfortunately, the question remains unresolved, primarily because of insufficient data on individual banks during the period prior to the regulation imposing the rate ceilings. Nevertheless, the author's position that intense rate competition was not an important cause of bankruptcies during the period under investigation is not implausible in the light of available data.

Pointing out the close substitutability of bank deposits for deposits of other financial institutions, the author further argues that the consequences of the ceiling on deposit rates have been to restrict the level of commercial bank deposits relative to those of other financial institutions. Happily, the data do support the widely-held belief that the composition of public holdings of various types of securities is responsive to the relative rates of return. With bank deposit rates at the ceiling, increases in rates paid on deposits of other institutions led to declines in bank deposits relative to total deposits. However, the author makes too much of this as he then argues that the final result will be to affect unfavorably the allocation of credit within the financial sector. Before this conclusion is warranted, it must first be established that marginal shifts in deposits among financial institutions will in fact lead to differences in the aggregate composition of assets held by all financial institutions and thereby appreciably affect the ultimate flow of funds to issuers of primary debt.

Discussions of deposit rate regulations raise some disturbing theoretical

questions which, unfortunately, are not brought up in the book. Notwithstanding the author's assertion that the banking sector has an oligopoly-oligopsony market structure, if deposit rates are not regulated what is the equilibrium rate of interest on demand deposits? And perhaps more importantly, what happens to the price level? These are not unrelated questions: a merchant willing to sell his product at a particular price when no interest is paid on demand deposits will surely be willing to take a lower price if he receives a stream of interest payments in addition. Interest-bearing money is simply worth more than non-interest-bearing money and the price level will reflect the higher value of the former. The higher the interest rate paid on demand deposits, the lower the price level will be. While there has as yet been no evidence uncovered which would suggest that such adjustments as outlined above have taken place, analysis suggests that they would take place. The gains from the elimination of the rate ceiling must be balanced against the potential losses due to the deflation that may result.

Moreover, if it is conceded that interest rates on demand deposits will rise above zero when the regulations are removed, there can be little doubt that the redistribution of income from banks to the public (via the interest payments) will decrease the rate of return in the banking industry. Further research is required to determine whether this redistribution ultimately holds good or ill for society.

Underlying the policy questions as to whether interest rates on bank deposits should be regulated is a host of theoretical questions yet unresolved. As usual, the definitive answer must await the results of additional research. The present book adds very little to our understanding of the theoretical issues involved but makes a contribution by providing empirical evidence that should prove valuable in future discussions.

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Analytical Methods in Banking. Edited by KALMAN J. COHEN AND FREDERICK S. HAMMER. Homewood, Ill.: Richard D. Irwin, 1966. Pp. xv, 599. \$12.50.

Operations research in banking, by comparison with industry, is still in a preliminary developmental stage. Cohen and Hammer have been in the forefront of many of the pioneering efforts in banking. This book presents a series of articles which the editors presumably feel are the cream of the operations research crop in banking.

After a somewhat rosy-viewed introductory chapter by the editors, the book divides itself into five major sections. The three sections on trust department and bond department operations and "Other Banking Problems" contain a substantial amount of material of relevance primarily to the large diversified metropolitan bank. Although these sections include some reprints of outstanding articles, particularly those by Markowitz, Sharpe, Fisher, and Kessel, it would be stretching it to suppose that medium or small size banks could derive much practical support from research of this nature. Other notable contributions include Crane's simulation model of corporate demand deposits and

Benston's study of economies of scale and marginal costs in banking, the latter being exclusively concerned with small banks.

The bread-and-butter banking problems of bank asset management and lending and credit policy are covered in the first two sections. Here the most ambitious efforts include the well-known Charnes-Chambers linear programming model for bank portfolios, the credit scoring and risk measurement studies of Myers and Forgy, and Paul Smith; and Hester's canonical correlation study of the commercial bank loan offer function. A glaring omission in the articles on bank asset management is Porter's *Yale Essays* study of bank portfolio selection. Another omission is related to the use of statistical marginal cost and marginal revenue estimates for asset and liability management in banking. Benston's study, while certainly one of the best of its kind, is dependent on cost accounting data. A substantial component of costs cannot be allocated by cost accounting methods; hence, Benston's marginal cost estimates are of rather limited validity.

Charnes and Chambers demonstrate clearly that, so far as the formalities are concerned, it is possible to develop a linear program for bank asset management that can accommodate both multiperiod horizons and, if necessary, chance constraints. Unfortunately, the actual constraints used by Charnes and Chambers are extremely artificial, being based on that strange amalgam, the Federal Reserve's "Form For Analyzing Bank Capital." Only a fraction of the real effort required in applying linear programming to an actual bank has been undertaken by Charnes and Chambers. This effort would consist in eliciting from bankers themselves meaningful formulations of the constraints under which they operate. As it stands, Charnes-Chambers is merely an exercise or prospectus. The same artificiality surrounds the IBM-McKinsey bank-management simulation, a description of which is also included in this volume. Here, at least, it is admitted that the IBM game is intended merely as a training device. Hester demonstrates that canonical correlation, a somewhat neglected statistical tool, can help describe aspects of economic behavior, particularly the terms on which loans are granted. The normative relevance of Hester's technique is much less evident. Once again, a sophisticated methodology is expounded, but the hard task of successful practical application remains to be performed.

At times, the book has overtones of a revolutionist's handbook. This is particularly true in those portions devoted to the trust investment function. The Markowitz and Sharpe articles suggest not only that the one-period portfolio selection problem has been solved in principle, but that efficient computational techniques exist for the approximate execution of E-V analyses. The security evaluation model of Ahlers (SEM) creates the impression that a mechanical forecasting method based on exponential smoothing of earnings per share, an estimate of the pay-out ratio, and a measure of the variance in earnings can produce better forecasts of rates of return on securities than the usual techniques used by security analysts to select undervalued securities. Moreover, Ahlers' technique is not really compatible with the insights of the security analyst. Better results are obtained by SEM alone or by orthodox security analysis alone than by any combination of the two. By strong implication, the se-

curity analyst, viewed as a predictor of undervalued securities, is technologically obsolete. If that is not enough, Clarkson's study suggests that the decision-making process of the trust investment officer in buying and selling securities can be very effectively simulated, leaving him free to take very long (permanent?) vacations from his erstwhile duties. The trust department section ends with a crude method for rating the management of investment funds by Jack Treynor, which only serves to remind the reader of the severe judgments against investment fund performance that other studies have presented. In a nut shell, one might conclude that existing trust investment management is ripe for euthanasia by operations research.

This raises a very fundamental and important point with regard to the function of operations research and the prospects for a general operations research revolution in the management of large corporations. The main obstacle is fundamentally one of cost. Under current conditions, operations research is an extremely expensive method of making many types of decisions. The expected payoffs from developing sophisticated analytical techniques must be very substantial before it becomes an effective substitute for ordinary seat-of-the-pants management methods. While the relative cost of operations research is likely to decline as successful applications gain widespread and standardized dissemination, there are few such techniques now available in banking.

The most unequivocally successful operations research applications in banking are of a much less ambitious nature than those related to bank asset management, or trust investment. The volume contains three outstanding examples. Michener and Peterson provide a well-reasoned and effective strategy for deciding on the optimal pursuit duration for collection of defaulted loans; Cohen and Hammer, building on earlier work, provide a technique for determining the optimal coupon schedules for municipal bonds; Byerly demonstrates how Monte Carlo methods can improve scheduling of tellers. In each case the problem is of limited scope, and is well defined and constrained.

One area in which an operations research breakthrough would be worth the cost is corporate cash management and bank lending strategy. The model of corporate demand deposits developed by Crane is definitely a step in the right direction, one in which much more work needs to be done. Crane rightly includes nonprice variables, such as mechanical services and credit services provided to corporations as factors affecting their demand for deposits. However, Crane does not fully develop a hypothesis as to the significance of such items as lines of credit, compensating balances, and the prime rate, and other terms imposed in connection with the corporate customer relationship.

In summary, Cohen and Hammer have provided a very useful service in gathering together a wide variety of material relevant to bank management. Their introductory comments and appraisals of each article are thoughtful and constructive. The most valuable contribution of this volume is at the academic rather than the practical bank operating level. For the first time a text is available to support a course in bank management, having serious intellectual content. On the practical level, while the book is short on examples of demonstrably successful OR applications in banking, one could use the materials collected here as a sales promotion kit to interest workaday bankers in

experimenting more fully and aggressively with quantitative methods of many sorts, not simply those outlined in this volume.

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Federal Reserve Monetary Policy 1917-1933. By ELMUS R. WICKER. New York: Random House, 1966. Pp. xiv, 221. \$5.95.

An oft-told tale is recounted in Professor Wicker's volume. It is a tale which never loses interest and never fails to intrigue. The main developments of this period are well known. This most recent study fills in many a significant detail of great interest to students of central banking.

Wicker's study is based on various primary sources: The Goldenweiser, Harrison and McAdoo papers, the Hamlin diaries, the Leffingwell letter books, the minutes of the Federal Reserve Board and the minutes and related papers of the Open-Market Investment Committee and the Open-Market Policy Conference. Lester Chandler's biography, based on Governor Strong's papers, provided important information as did the official publications of the Reserve System and the testimony taken before various Congressional Committees. The author restricted himself in the main to a fairly narrow, although important, focus: the attitudes of the Treasury and Federal Reserve officials on various questions as reflected in their writings and their votes on specific issues.

Probably no central banking system in its early years had to function within a more turbulent environment than did the Reserve System. One has only to recall the pre-World War I gold inflow, the then unprecedented problems of war finance, the deflation of 1920-21 which engulfed practically the whole world, the tariff policy of the American government which imposed a heavy burden on foreign debtors, the insistence of the American government that war debts be collected, the demands of the Allied powers for reparations, the machinations of the security affiliates, the epidemic of bank failures in the 'twenties, the overvaluation of the pound and the undervaluation of the franc, the speculative mania of the 'twenties and the overall decline in credit quality, the unwillingness to use fiscal means to combat the Great Depression (tax rates were raised in 1932), the breakdown of the foreign exchange patterns in the early 'thirties and domestic and international liquidity crises. At the end it was President Hoover who proposed a guarantee of bank deposits. If this had been implemented, say, in the middle of 1931, the United States might have been spared the ordeal of plunging into the secondary phases of the depression.

Wicker believes that the growing use of economists "will improve monetary behavior." One hopes that this may be the case. The interpretation of the passing scene is, however, often more an art than a science. And an art it doubtless will remain despite the profusion of data which serve as grist for the economists' mills.

Economists are prone to fall prey to the one-criterion fallacy of central bank policy-price level stabilization, the money supply, full employment, or whatever. In their enthusiasm they are apt to neglect other factors and forces and to become victims of the one-criterion approach in much the same way as

were the rigid gold standard adherents of an earlier period. A central banker needs flexibility of approach, the ability to see the whole forest despite the attractiveness of individual trees.

Above all a central banker must not overlook changes in the quality of credit. One has only to recall the speculative mania in farm lands in World War I, the inventory speculation of 1919, the decline in the quality of bonds and mortgages in the 'twenties, the use by Austrian and German banks of short-term funds to finance long-term projects and the deterioration of acceptance credits, to see the relationship between these and the ensuing developments of the 'thirties. Fortunately, academicians now are placing more emphasis on this extremely important topic.

In recent years it has been customary among economists to adjust their spectacles, to sharpen their pencils and to grade the Reserve System on its performance. If one were to lapse into this pedantic exercise, he would conclude that the Reserve System emerged from World War I with a record which excelled that of the Treasury. It emphasized the need for savings, it opposed the low interest rate policy of the Treasury, it urged that war paper be kept out of commercial bank portfolios. Similarly in World War II, the Reserve System emerged with a better record than the Treasury.

In World War I Federal Reserve credit took the form of loans to member banks instead of open-market operations, which means that in the postwar period the Reserve Banks could bring pressure on member banks to repay their loans and commercial banks in turn could bring pressure on their customers. Some of the excess credit was squeezed out of the banking system. The postwar decline in prices was looked upon as a normal reaction to war and postwar inflation. The policy followed in World War I restored part of the purchasing power of money at the cost of depression, that of World War II has led to a continuous erosion of purchasing power.

In the 'twenties, the Reserve System tried to recreate the pre-1914 world in a totally inappropriate economic environment. Trade barriers, the growing burden of unproductive debt, the restoration of exchange rates at unrealistic levels, the effect of the gold exchange standard as a credit multiplier, the growing arrogance of economic nationalism posed an unrelenting threat. The means adopted in the hope of restoring the one-world concept were financial, the lowering of interest rates in New York to promote the flow of funds to London and to induce the French to take the gold they wished from New York. These policies established interest rates which, President Schacht of the Reichsbank pointed out, could not be maintained. As interest rates rose under the influence of the speculative mania, pressure was placed on some European currencies and the situation was worse than if the United States had, from the outset, geared credit policy to domestic developments. Wicker's emphasis doubtless precluded his own interpretations of these developments which, if they could have been included, would have imparted life to an interesting record.

Similarly the Great Depression is replete with episodes which carry meaning for the present. To be sure, the United States has not repeated the tariff mistakes of that period and is able to rely, as it was not then, on the income

sustaining effects of the built-in stabilizers. However, heavy reliance upon a growing volume of Eurodollars as a means of financing credit needs and equally heavy reliance upon sharp increases in negotiable time certificates of deposit as a means of enlarging bank funds may be laying the basis for future international and domestic liquidity crises.

Have the monetary lessons of that period made an impress? Are we avoiding mistakes in policy, not necessarily the same mistakes but others, which will incur the criticisms of future economists? The decline in credit quality neglected in the 'twenties is being neglected again. Lending terms have been liberalized, loan maturities extended, down payments reduced, protection is thinned, repayments are predicated upon a concatenation of favorable circumstances, bank lending officers come to the fore who have no knowledge of or experience in recessions. Bank credit is being used to finance all types of credit needs. The growth in foreign debt exceeds the ability of many foreign nations to repay. The Administration and the Congress show reluctance, to say the least, to balance the federal budget at high levels of employment. All gold exports are offset by Federal Reserve policy. Other nations are expected to underwrite the balance of payments deficit of the United States. Controls are invoked to obtain temporary surcease. The interest rate curve is twisted and then untwisted. The illiquidity of banks and business firms increases. Consumer indebtedness soars. Consumer prices rise and cost push exerts its virulent influence. So many are the resemblances to the 'twenties that it would appear that not only have we learned no lesson but we seem to be repeating the mistakes of that decade with renewed vigor and determination. Whether performance is better or worse than that of the 'twenties and 'thirties will be left to the judgment of some future historian of the Federal Reserve System who will examine the records with the same diligence as did Professor Wicker.

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Public Finance; Fiscal Policy

Fiscal Policy for Economic Growth in Latin America. By the Joint Tax Program of the Organization of American States, Inter-American Development Bank, and Economic Commission for Latin America. Baltimore: The Johns Hopkins Press, 1965. Pp. x, 462. \$12.00.

This volume, containing the papers and proceedings of a conference held in Santiago, Chile, in December of 1962, includes papers by V. L. Urquidi, R. Desai, N. Kaldor, A. Harberger and J. H. Adler on the more general aspects of development finance; and by R. Goode, D. Jarach, A. Prest, J. Naharro and H. Wald on the reform of particular taxes. Finally, there is a paper by C. Cosciani on tax coordination in a Latin American common market, and a summary report of the views arrived at by the conference. Each of the main papers is followed by lively discussion, and though geared to the needs of Latin American countries, most of the material is applicable to a much broader setting. In all, the volume is much superior to what usually emerges from confer-

ences of this sort and makes excellent reading for a course in development finance.

The conference was organized in the context of the commitments undertaken by Latin American countries in the Charter of Punta del Este. Including policies to secure a growth rate of 2.5 per cent and a more equitable distribution of income, these commitments (if such they are) present a formidable challenge to tax reform in South America. There is general agreement in the Report that to meet it, a substantially higher ratio of tax revenue to GNP must be reached. Also, there is general agreement that it is well within the countries' economic and administrative capacity to do so. There is somewhat less general agreement on the proper scope and potential of tax policy as a redistributive tool; but the gist of the discussion is that the prevailing distribution of income is intolerably skewed and that much more needs to be done, especially in securing a more adequate tax contribution from property income.

The present system of indirect taxation should be simplified by replacing the multiplicity of specific taxes with a more general tax, be it of the sales or (there being some controversy on this) of the value added tax variety. This general tax should then be supplemented by a set of progressive excises in items of luxury consumption, both home produced and imported. No reference is made to an expenditure tax.

Reform of the individual income tax should aim at a unitary system, going beyond the global complementary tax now used by many South American countries as a supplement to a basic proportional tax with source withholding. The base should be defined broadly including capital gains, with constructive realization at death or transfer. (If this has been politically impossible to apply in developed countries, it seems a bit ironic to recommend it in the LDC setting!) Rates should be realistic and, according to most (but not all) participants, not above 50 per cent. Agricultural income should be included and be subject to income tax as distinct from land taxation. If capital income is to be taxed more heavily than work income, this should be accomplished not via a surcharge under the income tax, but a supplementary flat rate tax on net wealth. While requiring additional administration, the net wealth tax may also be used to strengthen the enforcement of income tax.

Passing over the discussion of transfer and corporation taxes, we proceed to what may well be the most important set of recommendations, relating to land taxation. It is agreed that the contribution of land revenue falls far short of what it should be, and that more effective land taxation is essential not only as a matter of revenue but also as a matter of economic policy. Present land use is highly inefficient in many cases, and this tendency is made worse by under-taxation. The discussion examines various devices to make land taxation more effective. These include better valuation surveys and simplified assessment methods, as well as a self-assessment scheme which entitles the government to purchase at the declared price. Also consideration is given to assessing potential or average rather than actual crops, to using progressive rates, and other devices to improve incentives for more efficient utilization.

In all, the volume shows that the needed directions of tax reform are fairly clear-cut and that there is substantial agreement among economists and tax

experts as to what should be done. Nor are the administrative obstacles of a prohibitive sort. If progress has been slow, the explanation I fear lies in the political obstacles which block a genuine acceptance of the objectives set forth in Punta del Este.

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Fiscal Harmonization in Common Markets: Vol. I: Theory; Vol. II: Practice. Edited by CARL S. SHOUP. New York and London: Columbia University Press, 1967. Pp. xx, 468; xxi, 674. \$30.00 the set.

This thorough and meticulous study was initiated in 1962 and guided by the skilled hand of Professor Shoup. The contributors, who met frequently at Columbia University, attempt to develop a theory of fiscal harmonization and to apply it to particular problems and regions.

D. Dosser in the first chapter blends ingeniously positive and normative principles of the theory of international trade with those of the theory of public finance, while keeping the discussion close to the important current issues. The chapter also serves as a base from which the forays of later chapters spring.

In the second chapter H. Shibata constructs a theory of free trade areas with emphasis on measures to prevent trade deflection. Unlike Dosser, who assumes fixed exchange rates between members of a union, he permits exchange rates to vary.

In Chapter III W. N. McNie explores some of the implications of transport costs for a discussion of the respective merits of the origin and destination principles of indirect taxation.

Chapters IV and V, by Dr. Norbert Anel, deal with the harmonization of public expenditure. Cost-reducing government expenditures are used to develop a theory of expenditure unions. The effects of social security expenditures on the mobility of labor are also examined. Without committing any of the widespread fallacies about the need for harmonization in this field, Anel argues that harmonization forms an important element in the functioning of a common market.

Volume II contains chapters on problems of particular common markets and free trade areas, what they have done and what they plan to do. The European Common Market dominates the discussion, but the Central American Common Market, the Latin American Free Trade Area, East Africa and Soviet Economies are also considered in detail. Dr. Clara Sullivan discusses indirect taxation in three chapters and Dr. Peggy Musgrave direct taxation in two. Dr. G. K. Shaw explores stabilization policies and relates them to tax and public expenditure harmonization. Professor Shibata follows with a chapter on tax harmonization in EFTA. Marion H. Gillim reviews these problems for the Central American Common Market and the Latin American Free Trade Area. J. F. Due and P. Robson discuss the East African Common Market and finally R. Bird ends the volume with an analysis of harmonization in Soviet countries.

It is possible to select only a few points from this large volume of valuable

material. It is curious to say—as Dosser does—that competition (in which the success of one implies the failure of others) is a good synonym for “expansion of individual choice and opportunity” and to equate it to “freedom of firms to produce where they will, for consumers to have a wider range of material (and cultural) enjoyment, for people to live and work where they will.” Competition, as a means, may or may not have these *results*, but so may cooperation and many forms of restriction. Dosser dismisses definitions of integration which are couched in terms of vague aspirations, but the identification of means and ends does not reduce such vagueness. Dosser’s discussion should be contrasted with Bird’s interesting discussion of integration and harmonization in COMECON.

The analysis of public joint-benefit expenditure whets one’s appetite for more, both in Dosser’s and in Andel’s chapter. One would have expected a deeper analysis of the appropriate formula for cost-sharing. Neither the question whether GNP or National Income, nor the problem of the valuation of goods and services (market prices or factor cost? purchasing power parity or national prices converted at equilibrium exchange rates?) is discussed.

R. Bird’s chapter in Volume I on regional policies in a common market raises questions which are now increasingly discussed not only in a regional but also in an international context. Much of existing theory is built on the assumption of either complete immobility or completely free mobility of all factors. But very different conclusions emerge if partial mobility of some factors is assumed. In particular, many of the implications of *laissez-faire* and *laissez-passer* have to be radically modified.

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Accelerated Depreciation in the United States, 1954-60. By NORMAN B. TURE. National Bureau of Economic Research Fiscal Study No. 9. New York and London: Columbia University Press, distributor for National Bureau of Economic Research, 1967. Pp. xix, 238. \$7.50.

This book is the first, other than two conference volumes, in the National Bureau’s project on the relation between tax policies and economic growth directed by the author of this study. Its purpose is to shed light on one of the most popular postwar fiscal devices adopted by governments throughout the world—accelerated depreciation. He deals with the changes made by the Internal Revenue Code of 1954 permitting two new methods of depreciation—the double-declining-balance and the sum-of-year-digits—and not on the developments of the 60’s—guide-line lives and the investment credit, although these are to be studied later.

Ture asks the important question: to what extent did these provisions stimulate capital formation? In his search for answers he focuses primarily on the extent to which these provisions have been used by business firms, since non-use would preclude their having any investment stimulus, on the revenue losses resulting from these new methods, and, finally, on relating the order of magnitude of the change in profitability and flow of funds to the demand for additional capital goods.

The major part of his study is concerned with the first question. He bases his analysis of use on Treasury Department data. For unincorporated enterprises, data are available only for 1959. There are some data for all of the years for corporations based on special samples, and a highly detailed study "Life of Depreciable Assets" made for 1959 with broad coverage. He analyzes these data to determine the extent of use of accelerated depreciation by type of business organization, industry, service life of assets, type of facility, and size of firm, with many cross tabulations presented (more than half of the book represents tabular presentations).

The mass of detail defies simple description in a review, but some findings are of especial interest. There was clearly some kind of learning function associated with the accelerated-depreciation provisions. The percentage of corporations using declining-balance rose from 8 per cent in 1954 to 24 per cent in 1960; those using SOYD rose from 5 per cent to 6 per cent. Because some may use both methods, the proportion using straight-line alone in 1960 stands at a startlingly high 70-75 per cent. If we look at assets acquired by corporations from 1953-1959 on hand in 1959, only 45 per cent were under accelerated depreciation. Evidence is more limited for unincorporated enterprises, but they appear to have used accelerated depreciation much less. Despite the fact that the proportion of yearly acquisitions of assets placed under accelerated depreciation was probably rising (although direct information is not available), these percentages are so low as to demand explanation.

Why did so few companies avail themselves of what in most circumstances would increase their present value? The question is whether firms failed to use it for rational or irrational reasons, or sheer ignorance. There are obvious reasons why not all assets acquired would be covered—second-hand facilities and assets with service lives of less than 3 years were ineligible—although this surely could account for but little of the total. Utilities might have had their troubles with regulatory commissions, but 42 per cent of their post-1953 acquisitions were on accelerated depreciation, second only to manufacturing (57 per cent) among major industry groupings. There is enormous variation by size of firm. Use varied from 15 per cent of post-1953 acquisitions for corporations with assets of less than \$1 million, to 17 per cent for those with assets of from \$1 to \$25 million, to 68 per cent for the largest size class. There is still much to be explained, and perhaps the Hall-Stainbach interview studies forthcoming in this series will further illuminate these puzzles.

Ture then proceeds to estimate the corporate depreciation added in 1959 by the accelerated methods—about half of straight-line depreciation on post-1953 additions—in manufacturing and public-utility corporations with assets \$25 million and over. This amount converted into percentage tax reduction, however, tells a different story. The smallest size corporation received 7.5 per cent reduction, while the largest received only 6.1 per cent. Nor is manufacturing the major gainer (5 per cent) in this sense, being superseded by service (17 per cent), agriculture (8 per cent), and utilities (6 per cent).

Finally, Ture uses his estimate of the change in 1959 tax liabilities (\$1.3 billion) as a shift in the supply of funds schedule and Terborgh's estimate of percentage change in yield as a shift in the demand for assets schedule to ar-

rive at an estimate of the effect on corporate outlays in 1959 of not less than \$1.3 billion and as much as \$2.7 billion. I think his conclusion here is incautious. The simple supply-demand analysis can certainly involve double counting (in a perfect capital market), the percentage change in yields may be irrelevant, if ignorant, the mechanical application of various elasticities does not inform us of their legitimacy, and the time response of demand is unclear. It is hard to relate it to the Jorgensen-Hall estimates (AER, June 1967) of the effects of 1954 accelerated depreciation. But by the same token, the questions raised by Ture's study on the limited use of accelerated depreciation requires modification in its effect on the Jorgensen-Hall computation of capital cost. It is not their computation that is relevant, but that of business firms as they learn about these provisions or remain ignorant.

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International Economics

Theoretical Issues in International Economics. By M. O. CLEMENT, RICHARD L. PFISTER, AND KENNETH J. ROTHWELL. Boston: Houghton Mifflin, 1967. Pp. xiii, 449. \$6.50; paper, \$5.25.

This is an ambitious book. Its stated purpose is to review a number of contemporary issues of international economic theory "which in particular have received widespread and intensive attention in the last two decades and in which signal contributions have been made" (Preface, p. viii). Each of the chapters is designed as a "survey article," to develop and compare ideas and concepts that have emerged from specific theoretical discussions in the literature. In all, nine topics are treated—every one an old, familiar friend. In Part I, under the heading of Pure Trade Theory, four chapters examine "Trade and Relative Factor Prices" (the effects of trade on factor returns), "Trade and Relative Factor Supplies" (the effects of factor supplies on trade), "The Terms of Trade," and "The Theory of Customs Unions." In Part II, under the heading of International Monetary Theory, five chapters examine "The Adjustment Mechanism in International Payments," "Exchange Rates: Fixed or Flexible?," "Elasticities and Absorption in Devaluation Analysis," "Persistent Disequilibrium: Dollar Shortage and Dollar Glut," and "The Adequacy of International Reserves." Each chapter is accompanied by an extensive bibliography of the literature through mid-1964.

On the whole, I think the volume is a triumph. Professors Clement, Pfister, and Rothwell have digested scores of articles and monographs representing the output of an entire generation of international economists—a gargantuan task in itself. Moreover, in their reviews they have respected both the complexity of contemporary issues and the subtlety of theoretical discussions. And even though they were individually responsible for drafting separate chapters, they were collectively successful in minimizing stylistic differences, substantive inconsistencies, and avoidable redundancies. Consequently, they have produced a volume of extraordinary usefulness—nine autonomous survey articles, all of

high quality and some (especially Chapters 1, 3, 6, and 7) that are absolutely first-rate. Students at the graduate level and teachers will find the book an invaluable reference aid, and it should certainly be read by anyone claiming a professional interest in international economic theory.

To be sure, the volume is not without flaws. The authors may be criticized on a number of grounds—though principally for errors of omission rather than commission. For instance, their list of topics can hardly be described as exhaustive: the number of theoretical issues in international economics is considerably greater than nine, and some of those that are excluded have received at least as much intensive attention in the last two decades as some of those that are included. In particular, one regrets the absence of any survey of the branch of international economic theory that is concerned with commercial policy and welfare. Surely, as a minimum, a chapter would have been warranted reviewing the profession's changing attitude toward the broad question of free trade vs. protection. Likewise, one regrets the very cursory treatment in Chapter 4 of such vital subjects as the general theory of the second best (one paragraph) and customs unions in less developed areas (one sentence). Surely these should have received more consideration than is elsewhere accorded such outmoded issues as, for example, elasticity pessimism, which appears in no less than three different chapters, or the postwar dollar shortage, which occupies much of Chapter 8.

Chapter 2 is seriously deficient in failing to consider adequately the dynamic aspects of international trade theory. This chapter does constitute an impressive restatement of the Heckscher-Ohlin theorem, but it neglects the fundamental fact that the factor-proportions model is basically static in nature. Even when, as in the latter part of the chapter, account is taken of variations in factor supplies and technology, the model is incapable of explaining satisfactorily changing patterns of real-world trade in manufactures. This has been confirmed by several empirical studies. More dynamic approaches to explaining trade are needed, and tentative alternatives have been proposed by a number of economists—among them, Ford, Linder, and Posner—emphasizing economies of scale, technological innovations, and modifications of the entire "productive environment." These theoretical developments are important, yet they are ignored in Chapter 2, which consequently gives a rather misleading impression of what factors are significant in determining industrial trade patterns.

Similarly, Chapter 8 is seriously deficient in failing to take proper notice of financial factors that are significant in determining the United States balance-of-payments position. The chapter purports to survey the various long-term explanations of the dollar's persistent disequilibrium since World War II—of first its shortage and then its glut. From its start, however, the chapter limits itself on grounds of "convenience" (p. 356) to an analysis of the *trade* position of the United States. This is rather like trying to explain the operations of a commercial bank by concentrating exclusively on its function as consumer and seller of goods and services; the narrow focus is convenient but not very meaningful. The United States is much more than a trader in the world community: it is in addition the central bank of issue for the international mone-

tary system. The problem of the American balance of payments cannot be analyzed meaningfully without taking into account, as many economists have done but Chapter 8 does not, the role of the dollar as reserve and vehicle currency and the role of this country as financial intermediary. Little wonder that the chapter is unable to conclude with a "truly useful hypothesis of the dollar problem" (p. 391). Its coverage is simply too inadequate.

Perhaps the weakest chapter, from an analytical point of view, is Chapter 9, which in discussing the problem of international liquidity fails to distinguish clearly between the "demand" for reserves and the "desire" for reserves. As Fritz Machlup pointed out as early as 1962, the two are by no means identical. Demand implies an offer of something in exchange for the object demanded, whereas desire is a psychological concept that implies nothing about a willingness to offer anything in exchange. Yet the chapter speaks, for example, of the "underdeveloped countries' virtually insatiable appetite for international exchange" (p. 429) as if this were the same as their demand for reserves. Nothing could be further from the truth: the LDC's want reserves, but they are unwilling or unable to generate the external surpluses required to earn them. On this issue the chapter is confused. Remarkably, though, in a volume of this scope and ambition, confusions like this are exceedingly rare. The authors are to be commended.

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Trade, Aid and Development: The Rich and Poor Nations. By JOHN PINCUS. New York and London: McGraw-Hill for the Council on Foreign Relations, 1967. Pp. xv, 400. \$10.00.

This book is a survey of a variety of topics on the trade problems of developing countries and their relationship to the trade policies of developed countries. It includes theoretical issues of trade and development strategy, statistical data on trade and aid, the historical development and role of international economic institutions such as IBRD, GATT, etc., the motivations of aid-givers and aid-receivers as exemplified by the UNCTAD conference, and many policy issues and proposals such as commodity price-fixing agreements and tariff preferences. The most useful aspect of the book, although it is not the major portion of it, is the provision of statistical data. Trade both in primary commodities and in manufactured goods, relative growth rates, the receipt and donation of aid grants, the value of grants versus loans at different assumed rates of discount, and projections of foreign exchange "gaps" and aid "requirements" are provided. The weakness of the book is its inadequate and frequently cavalier treatment of analytical issues on which important questions of economic policy depend. For this latter reason, it is not a good book for students, although parts of it may prove useful for the profession at large.

From time to time, Pincus professes general agnosticism regarding what development policy should be (p. 73, for example) because of the lack of homogeneity among developing countries. Yet, he frequently makes very strong generalized statements on development strategy which are not built up from any well-defined set of assumptions or underlying model that describes the

economic environment in which he is operating. His ideology, which follows that of Myrdal and Prebisch, is represented by the following quotations:

Second, underdeveloped countries are not in practice faced with a choice between free trade and protection. If they want to industrialize they are going to have to protect most industries, with the exception of those for which transport costs, taste differences, or a favorable domestic resource supply situation provide the equivalent of tariff protection. The only relevant issues are which industries to protect, how much protection to give, and at what cost (p. 147).

. . . But no country that faced international competition from industrialized countries have ever become competitive in world markets for manufactures until it developed its own industry behind a protective barrier. The call for free trade in manufactures arises after the country has become competitive (p. 203).

There are two kinds of presumptions built into these statements. One is a question of historical fact that experiences such as late 19th century Japan, which was forced by military power to agree to free trade with Europe and the United States, and Hong Kong since World War II, are somehow no longer relevant. The other problem is analytical and has many aspects. In a short review, it is difficult to establish an economic environment about which meaningful statements can be made. Let us assume we have an economy within which agricultural and mineral wealth is very limited vis-à-vis population pressure. The development of substantial manufacturing activity in a few specialized lines is to the comparative advantage of such countries. Japan, Korea, and India would all fall into this category. Is it possible to protect most industry—actual and potential—simultaneously by the general use of tariff and non-tariff restrictions on imports?

The idea of protection is to pull resources into activities they would otherwise not flow into at the expense of other activities. The advantages that a single six-digit industry would have derived from protection are largely lost when other six-digit industries receive the same "protection" and increase their demands for the economy's scarce investment resources. Moreover, there will be an offsetting change in the relationship of the exchange rate to the internal price level. This results in an upward "real" valuation of the currency which offsets to some degree, although not completely, the effects of a general tariff in limiting foreign competition in manufactured imports. So the real protection being given to each domestic manufacturing industry by widespread tariff and non-tariff restrictions is far less than the apparent protection associated with high nominal tariff rates or price differentials between domestic goods and equivalent imported goods at the official exchange rate. Moreover, this limited "protection" is dispersed widely over a wide variety of industries, each serving the local market on a scale which is frequently too small to absorb modern technology without having to meet tests of cost effectiveness.

However, the policy of widespread nominal protection covering a very large proportion of potential imports does discriminate sharply in one important respect: those domestic manufacturing activities which are comparatively most efficient and which are potential export activities, are heavily penalized. Even

though they are given approximately the same protection on their domestic sales as their less efficient brethren, they are unprotected—frequently drastically so—on their actual or (more importantly) potential exports. Exporters face a very unfavorable conjunction of exchange rates and internal price levels which arise out of the import restrictions. Costs of production in terms of the local currency are too high vis-a-vis export prices translated into domestic prices at the existing exchange rate. Taxing or restricting imports entering the economy is equivalent to taxing exports that leave it. Pincus is apparently unaware of this fact even though much of his book is concerned with the development of industrial exports from LDC's.

One can believe, as does Pincus, that for some countries the greatest opportunities for efficient new investment lie in import substitution in the industrial sector (p. 169). It may well be that agriculture has limited development possibilities in or of itself (p. 158). Does it then follow that because one makes these assessments, perhaps correctly, that governments should heavily tax industrial export and agricultural activities in order to subsidize industrial import substitution? If industrial import substitution is indeed more advantageous, this is the route that would be followed under a neutral fiscal and tax policy where the government carefully ensures that investment funds—either public or private—flow to those uses where the rate of return is highest. It should not be necessary to make the rate of return in industrial import-competing activities look artificially high while simultaneously making rates of return to farmers and industrial exporters look unnaturally low.

A natural consequence of the inappropriate assessment of internal LDC economic policy is to emphasize external constraints thrown up by rich nations against imports from LDC's and to emphasize the heightened importance of aid transfers in this context of foreign exchange scarcity. Pincus summarizes different authors' projections of aid "requirements" based on the "two gap" analysis which separates an *ex ante* savings-investment gap from an *ex ante* foreign exchange gap in viewing aid transfers. He criticizes the two gap formulation by stating, among other things, that: (1) it is of limited usefulness because the two gaps are necessarily equalized *ex post*; (2) the foreign exchange gap implies inflationary pressure whereas the savings-investment gap does not; and (3) "if there is an import surplus without inflation, some domestic producers are losing money and will have to cut back if the surplus persists" (pp. 300-301). Although the first criticism appears true but irrelevant, the second not true in the context of ordinarily accepted two gap models, and the third simply very peculiar, what is particularly upsetting is that the full model to which these comments refer is not spelled out. A systematic listing of assumptions and equations describing the internal logic of the two-gap model and its assumed production technology are all missing, so that the reader is simply exposed to a series of *ad hoc* and rather puzzling comments with no basis for judging their verity. This is rather representative of Pincus' analytical approach in many areas.

Pincus has an extensive discussion on all the advantages and disadvantages of tariff preferences for LDC's in the markets of wealthy countries. He comes out in favor of preferences, although he would make them temporary by hav-

ing general "most favored nation" tariffs fall to the preferred level with the passage of time. His book was completed before the successful completion of the Kennedy Round of the GATT negotiations so that there is now considerably less impact to be obtained from his suggestion. He would modify the "principal supplier" clause in GATT to give LDC's more bargaining power. He favors commodity price-fixing agreements as a means of disguised aid transfers and considers the stabilization of producers' incomes to be of relatively minor economic importance. Generally, he goes along with most proposals for *de facto* aid transfers in one form or another without being much concerned with allocative efficiency.

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Effects of U.K. Direct Investment Overseas. By W. B. REDDAWAY, WITH J. O. N. PERKINS, S. J. POTTER, AND C. T. TAYLOR. Occasional Paper No. 12, Department of Applied Economics, University of Cambridge. New York: Cambridge University Press, 1967. Pp. 196. \$4.75.

After years of speculation in Europe and the United States about the efficiency implications and the balance-of-payments effects of foreign private direct investment, some first-class empirical work on the subject is beginning to appear. This is one such effort, aimed at measuring the effects of foreign private direct investment by British investors upon the British economy. The Cambridge group asked practically all such major investors (1) to account for the transactions undertaken through and with their foreign subsidiaries and affiliates and (2) to offer a best guess on the transactions that *would have occurred* with the United Kingdom if the British investor had not undertaken his overseas investments. Companies incorporating over 70 per cent of the net assets of all United Kingdom foreign direct investment responded to the request.

How to apply the results? In a breathtaking *tour de force*, the Cambridge group made two assumptions: that the aggregate output of goods and services of the United Kingdom, the aggregate level of consumption, and the distribution of income in the United Kingdom are largely determined by government policy; and that the government's policy goals are more or less invariant, whether or not the foreign investment takes place. Accordingly, insofar as foreign investment takes place, the investment's main effect is to divert output from home investment to exports, and not much else.

Given the premises, the initial balance-of-payment impact of the foreign investment consists primarily of: (1) The outward flow of capital; and (2) a certain amount of exports by the United Kingdom, in excess of the U.K. exports which would have taken place if the investor had not been a U.K. resident. In subsequent periods, the effects of the investment become more complex. These include: (3) An inflow of profits to the U.K., net after taxes paid to the foreign host government; (4) an outflow of interest payments from the U.K., representing the marginal cost to the U.K. economy of borrowing reserves or drawing down other overseas assets, in order to secure the capital for that part of the investment which is not provided by way of the export of real

assets through the current account; (5) an inflow of receipts to the U.K. from the export of goods and services in excess of what would have occurred if the investor had not been a U.K. resident; and, finally, (6) a reduction in the outflow of payments from the U.K. due to the saving on imports which were aborted by reason of the decline in home investment that was the consequence of the net outflow of goods associated with the foreign investment.

The Reddaway report does its best to quantify each of these slightly tortuous concepts, using data from the questionnaires purporting to represent the "average" U.K. foreign investment. According to its calculations, the initial favorable effect of a U.K. foreign direct investment of £100 is about £9, while the annual effect thereafter is about £4.

Insofar as the Reddaway report has policy implications, the implications are clear. A country like the United Kingdom could achieve a considerable easing of balance-of-payment strain for a number of years by blocking the continued outflow of direct investment funds. Of course, the wisdom of a cut-back would depend on the opportunity cost; on whether the added home investment and the reduction in the precariousness of the U.K. situation could be used during those years to generate balance-of-payment consequences more favorable than those achieved by the foreign investment.

The virtues of the Cambridge report seem to me to be in the skillful handling of complex facts; the dangers seem to lie in the risk that some unwary government official may actually rely upon its results for policy purposes.

One major difficulty with the Reddaway report is the model of the British economy that underlies the results. It is a most implausible model, in my opinion. In some respects, the model includes some rather startlingly pure manifestations of a competitive economy. Goods and services are priced at their marginal cost; resources are readily shifted, from selling insurance policies for export to making widgets for the home market; there are no supply bottlenecks, and no internal or external economies of scale. At the same time, however, the model assumes a British economy that is presided over by a government whose objectives are given and unchangeable, to be retained through thick and thin whatever the private sector may do. These two groups of characteristics, on first reading, make strange bedfellows. But it is obvious how they have come to be used together. They represent pure forms of the modern economists' aspirations: a society in which all the good fruits of pure competition and all the creative products of a wise and benevolent activist government are somehow blended in a perfect union.

My own image of the British economy is rather different. It is an economy in which the public sector learns by doing, by moving from one uncertain situation to the next and adjusting for past errors as it goes. There are occasional major irreversible decisions which push the economy in new directions. But there is neither a well-articulated plan nor any reasonable expectation of one. As for the private sector, it makes its decisions under conditions of uncertainty, in an industrial structure that is strongly oligopolistic, uncertain of the critical facts relevant to its decisions and unable readily to obtain them. These are characteristics common to many Western economies; for most policy purposes they have to be taken as given. For economies such as these, the Redda-

way model is fairly close to irrelevant. The approach needed is probably one based on sequential analysis, a messy start-stop-ponder-and-choose procedure.

This brings us close to the heart of the matter. It is one thing to devise a hypothetical model of the economy in order to test a series of hypotheses, *ceteris paribus*. It is something else to project the behavior of an economy, as a preliminary step for prescribing policy in order to alter its behavior. Sometimes the two exercises have a good deal in common; sometimes they do not. My guess is that this is one case in which they do not.

But there are some more specific points in the Cambridge study worth exploring. One of these is the assumption that the scarce resource preempted by foreign direct investment is goods and services; and the related assumption that these goods and services, if released, would be available for home investment. My own inclination would be to investigate a model for the United Kingdom economy in which the critical resource-allocating effect of foreign direct investment was to deploy *management* for overseas operations rather than for home enterprise. National savings and investment in the aggregate, according to my preferred version, would be somewhat elastic, being expanded or contracted as necessary to match management's perception of its reach. The effect of this change in assumptions would not be trivial (it could, for example, obviate the need for the critical assumption, made by the Reddaway report, that the United Kingdom has to borrow foreign exchange reserves or liquidate foreign exchange assets to finance its foreign direct investment). The reader may well be tempted to ask just how such a model could be constructed. This is a tough question; but while we are groping for the answer, we ought to resist the overwhelming temptation to assume that the easily measurable magnitudes are the relevant ones.

Another element in the study which I would be inclined to handle differently from the Reddaway report is that of royalties and management fees. The Cambridge team, after some consideration of alternatives, decided to assume that such fees represented the export of a service equal in value and in resource use to the fees received. In reality, management fees are usually assessed by some internal formula of the parent on a basis that has nothing to do with open market prices—if indeed the concept of an “open market price” has any meaning in the context. As for royalties and licensing fees, they ordinarily signal little more than the assignment to a subsidiary of monopoly rights in a foreign patent or trademark; while *bona fide* technical assistance sometimes goes along, that often takes the form of shop manuals, foreigners' visits, and other media of zero marginal cost to the British “exporter.” The Cambridge group's handling of this item, therefore, has an aura of arid unreality.

There is one added difficulty in the Reddaway study that deserves a word or two of comment. This has to do with the elements of vulnerability that are involved in asking investors what might have happened *if they had not made a given investment*. How does an investor make his decision in the first place? According to various studies, he perceives either an opportunity or a threat in some distant market. The signals, threat or promise, are not crystal clear. But

if he invests, he does so with fingers crossed, and with the expectation that in considerable measure he will learn by doing. And so he usually does. The costs of operation differ in lesser or greater degree from the investor's expectation; the market proves amenable or resistant in unexpected ways; the product line proves to need adjusting. But adjustment and adaptation occur as the years pass.

Enter the researcher with the question: What would you have done differently, if you had not invested? The question is asked at a time when the investor has already been exposed to a learning-and-response process in the foreign market which was one of the consequences of the investment itself. With the benefit of that process indelibly imprinted in his mind, will his answer contain bias? Very likely. Can we predict the direction of the bias? Almost certainly.

In the present case, my guess is that he would frame the alternative course of action on the assumption—the erroneous assumption—that his knowledge of the local market and his perception of risk in the local market would be much like that already acquired through the investment route. Hence, he would tend to assume that, in the absence of the investment, he would have sold by way of export any product which his current knowledge suggests is exportable. His estimate would not reflect the characteristic inertia that derives from ignorance and from high perceived risk and which, in my view, is one of the largest blocks to export.

I am tempted to conclude this review with the usual laudatory phrases for a job well done. But Mr. Reddaway hardly needs much comment of that sort from this writer. This is by all odds the best analysis of its kind that has seen the light of day. One only wishes that it could be recognized for what it is—an ingenious exercise whose conclusions are determined by its premises and whose premises may have little relation to the conditions that confront the policymakers.

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America in the Market Place—Trade, Tariffs and Balance of Payments. By PAUL H. DOUGLAS. New York: Holt, Rinehart and Winston, 1966. Pp. ix, 369. \$7.95.

Senator Douglas provides a highly readable exposition of his views on international trade, tariffs, and the balance of payments. The book is addressed chiefly to the policy-maker and the noneconomist but some of its chapters, like those on the historical development of tariffs and trade, merit the attention of economics students also.

In most questions, Douglas is on the side of the angels. He presents a convincing case for free trade and effectively dismisses the arguments of protectionists. The mercantilists meet with a similar fate and there are trenchant references to present day mercantilists. Nevertheless, in the policy discussion Douglas appears to accept the popular view which considers the other countries' tariff concessions as a gain and our own as a sacrifice. He submits that

we are pledged to get as much as we give and complains that the tariff concessions offered by the Common Market in the Kennedy round of tariff negotiations are not sufficiently generous (p. 146).

In the historical part of the book, Douglas is sharply critical of protectionist tendencies in the United States in the late nineteenth and the early twentieth century and endorses the moves made towards free trade since Cordell Hull's Trade Agreements Act of 1934. But in the discussion of recent tariff negotiations, the United States comes off a little too well. It is claimed that we have had only minor departures from a liberal trade policy and that the specific protective measures used by the the United States "have been relatively slight in comparison with the export bounties and covert forms of discrimination practiced by many other nations" (p. 299). Elsewhere, American generosity and European self-interest are contrasted. Thus, Douglas submits that, in separating the discussion of agricultural goods from that of industrial merchandise in the Kennedy round of negotiations, "once again the generous desire of the Americans to co-operate had been turned by the Europeans to their own advantage" (p. 145).

The views voiced in the discussion of aid to less developed countries are also open to dispute. According to Douglas, "on the whole, the United States emerges from these comparisons as by far the most generous of the industrial nations. It has also distributed its assistance over a much wider spectrum of countries and, having no colonial empire of its own, has made liberal grants and investments in countries over which it has had no political control" (p. 196). This exposition disregards the political and military objectives of U.S. aid and the concentration of a good part of it in Vietnam, Korea, and Taiwan. Also, one can hardly claim that our burden of foreign assistance much exceeds that of any other Western nations. According to the latest OECD compilation, in 1965 U.S. aid to developing countries amounted to 0.64 per cent of national income, while the corresponding ratios were 1.06 in France, 0.84 in Belgium, and 0.59 for the OECD countries as a whole. True, our aid terms are generally more favorable than those of other countries, but much of U.S. food aid is overpriced and tied aid may entail a larger excess cost in this country than elsewhere.

Senator Douglas has considerable interest in international monetary arrangements. He favors a modified version of the Triffin Plan under which new reserves would be distributed to the members of a reorganized IMF in proportion to their quotas. These new collective reserve units would grow at a rate that permits total reserves to increase at about 3 per cent a year. Nevertheless, selective increases would also be permitted.

Increased international reserves would permit countries to "ride out" larger balance-of-payments fluctuations than they have in the past without necessitating variations in exchange rates. Douglas is a staunch opponent of flexible rates. He maintains that Britain narrowly escaped devaluation in 1964 and again in 1965 and that she "was saved directly and indirectly by the United States" (p. 270). Some would interpret the historical evidence in a different fashion and blame the U.S. government for having exerted pressure on Britain not to devalue.

This reader would also disagree with Douglas on the desirability of establishing a trading area consisting of the United States, EFTA, Canada, Australia, New Zealand, and Japan in the event that the Common Market decided against participation. This proposal would create a division in the Atlantic alliance and would tend to increase the dominant position of the United States in political and economic relationships with the allies which is opposed by many Europeans. A continuation of tariff negotiations under the most-favored-nation clause might be a better means for furthering our political and economic objectives.

These comments should not detract from the value of the book which presents an eloquent and reasoned plea for freer trade, for helping less developed countries, and for improving the international monetary mechanism. It should be read by Douglas' former colleagues in Congress, as well as in the Executive Branch, and it should command attention among people interested in our international relations.

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Disarmament and World Economic Interdependence. Edited by EMILE BENOIT. New York: Columbia University Press, 1967. Pp. 260. \$8.00.

A precondition for partial disarmament is the recognition of our international interdependence and the convergence of our economic and political systems. Activities that promote the international exchange of people especially scientists and educators, or stimulate trade in goods and services, or that foster economic growth, may contribute to the establishment of peace. The gradual process of international accommodation and depolarization may then lead to partial disarmament and arms controls. Economic problems associated with the transition from defense production to civilian output are manageable. This is the theme of the latest book edited by Emile Benoit, professor of international business at Columbia University and expert on the economic consequences of disarmament. The book contains 17 papers prepared for the Conference on Economic Aspects of World Disarmament and Interdependence held in August, 1965 at Oslo, Norway.

The leading article by Benoit and Harold Lubell of AID provides, for the first time, a comprehensive standardized tabulation of the economic resources devoted to national security by 36 nations. They estimate that these nations account for 97 per cent of the world's military spending of \$133 billion. The statistics are available for 14 of the NATO countries, 8 Communist nations of the Warsaw Pact, and 14 others including China, Israel, and the U.A.R. Data are given in local currencies, then in dollars at official exchange rates and finally at purchasing power parity exchange rates. The defense expenditures for each country are then subdivided into functional categories for military personnel, procurement, research and development, construction, and operations. This enables the authors to analyze separately the economic impact of disarmament on the nations and the extent to which the civilian sectors are burdened. The arms spending is also expressed as a percentage of the govern-

ment budget and of G.D.P. This article should be a significant reference source for economists, sociologists, and political scientists interested in the impact of military spending.

The book is enhanced by several excellent essays on specific countries. Morris Bornstein's article attempts to isolate the economic factors in the Soviet attitude towards arms control. For the U.S.S.R. military spending conflicts with other priorities: investment for growth, improvement in consumption, and foreign economic assistance. Arms control, says Bornstein, could be an attractive answer to their economic problems, and thus the western powers should offer some powerful economic incentives for Soviet cooperation.

Arthur J. Brown of the University of Leeds assesses the complex and uncertain influence of partial disarmament on the balance of payments of the United Kingdom. Olav Bjerkholt uses an interindustry model for Norway to weigh the possible economic result of complete disarmament in his country. Ludek Urban writes as a Communist in his essay stating that socialist countries do not fear the adjustment to arms control. They do not have to worry about maintaining aggregate demand, so their adaptation would be a physical one solved by proper planning.

"Arms Control and the Vulnerability of Communist China's Economy to External Pressures and Inducements" is the title of Alexander Eckstein's monograph. It is a factual statistical study of China's foreign trade and has very little direct relationship to the theme of this book. It would be well to have had it published where the "China watchers" would be more apt to see it.

Like so many other reports on conferences, a few of the articles seem to be included more for diplomatic reasons than for scholarly content. One article would have been suitable for presentation before the chamber of commerce of Southern California to reassure them that they are not part of the military-industrial complex. The author prefers to call the defense companies "problem-solving" companies.

The last 73 pages contain six essays stressing the preconditions of disarmament and are grouped under the broad title of Competition, Cooperation and Convergence in an Interdependent World. Economists from six different countries wrote these articles—some obviously struggling (and in vain) to make their essays fit the theme. The best of these, however, was on "The Convergence of Economic Systems in East and West" written by J. Tinbergen and colleagues.

In times like these it may seem absurd to write about disarmament and to hold international conferences on interdependence, but the world scene changes quickly. The nuclear test-ban treaty was signed less than a year after the Cuban missile crisis. Following the Middle East and Vietnam settlements, perhaps this book will become a reference work for those leaders responsible for easing the adjustment from war to partial disarmament.

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International Economic Institutions. By M. A. G. VAN MEERHAEGHE. New York: John Wiley and Sons, 1966. Pp. xx, 404. \$8.00.

This book aims at a worthy purpose—to combine description of the origins, provisions, and operations of contemporary international economic institutions with an analysis of their effectiveness. Unfortunately, Professor van Meerhaeghe's reach exceeds his grasp in several dimensions.

Part I is an attempted summary and critique of the theory of international trade, international monetary theory, and the theory of international economic policy, condensed into 86 pages. The stated aim of this summary is to "enhance the utility of the next two parts [dealing respectively with world and European organizations], by explaining some terms or concepts in international economics." But as the author (who is professor of economics at the State University of Ghent) points out, he does not pretend to give a complete explanation of the theory of international economic relations. The uses of Part I therefore remain obscure, far too condensed for the beginner, and of no value to the professional. For example, his exposition and critique of Ricardian comparative cost theory, the Heckscher-Ohlin theory, and Samuelson's extension of the factor-price equalization theorem occupy a total of about one thousand words, without the aid of algebra or geometry. The effect is kaleidoscopic (for example, the chapter on international economic policies makes no reference to either foreign private investment or foreign aid, and is, in effect, limited to a cataloguing of various measures affecting balance-of-payments and employment via commercial policy and exchange rate variation); and the reader hopes for more useful material from the rest of the book.

This hope is realized only in part. Part II is largely a description of provisions and operations of the International Monetary Fund and associated international monetary arrangements, the International Bank for Reconstruction and Development and affiliates, the General Agreement on Tariffs and Trade (with a brief reference to the United Nations Conference on Trade and Development), and the international commodity agreements for wheat, sugar, coffee, and tin. Part III gives similar summaries for Benelux, the Organization for Economic Cooperation and Development, the European Coal and Steel Community, the European Economic Community, and the European Free Trade Association.

The book is most useful at the purely descriptive level, covering in orderly fashion the functions of a dozen international economic organizations. Some of this material is unsatisfactory, however. First, there is too much reference to provisions of particular articles or paragraphs of agreements by number, so that unless the reader has a particularly retentive memory he is obliged to leaf back every few pages for the original citation to see what Article 24 of GATT, for example, is about.

Second, and more important, some world and European organizations are passed over with brief mentions (economic functions of the United Nations, and its specialized agencies, UNCTAD, Euratom).

Third, extra-European regional institutions are entirely passed over (Cen-

tral American Common Market and Latin American Free Trade Area receive four lines each, while the regional development banks, Comecon, and the Central African Customs Union are completely ignored).

Fourth, the descriptions are often confusing, partly because of excessive compression and partly, I suspect, because of poor translation. The author's acknowledgement states "In order to achieve translation of this work in the shortest possible time it was necessary to engage the services of a relatively large number of translators. . . . A. Meuleman gave most valuable assistance in achieving uniformity of spelling and terminology, a considerable task in view of the multiplicity of translators."

When it comes to appraisal of the advantages and disadvantages of each institution, the volume is less useful. Professor van Meerhaeghe does not state his value premises, so they must be largely inferred by the reader. He sees little value to OECD and UNCTAD, both institutions being needless in his view. IMF and IBRD receive his approval in general—he discusses the Triffin and Bernstein plans, but views the "so-called lack of international liquidities," as "in the nature of a working hypothesis rather than an imminent danger," requiring no far-reaching changes in the present international monetary system. He sees commodity agreements as potentially moderately useful devices for price stabilization, and says that more agreements and better-managed ones are in order. For promoting the underdeveloped countries' interests in maintaining commodity earnings, he proposes expansion of the IMF compensatory finance facility.

EEC receives his general support, although preferences for EEC associates and the common agriculture policy draw his fire. He also cautions on the basis of Benelux experience against undue haste in integration efforts. EFTA is discussed rather briefly; as the author points out, it owes its origin to the establishment of EEC, and is a medley from both the geographic and economic standpoint.

The effort at appraisal of institutions would have benefited from a concluding chapter, relating the functions of the various organizations to each other, therefore offering a more comprehensive framework for analyzing the value of the various institutions, in terms of the growth of the world economy, or some other specified welfare criteria.

This volume is in no way a substitute for the source material or the monographic literature on the individual institutions (e.g., Curzon's *Multilateral Commercial Diplomacy*, Frank's *European Common Market*, Camps' *Britain and the European Community*, Aubrey's *Atlantic Economic Cooperation*). It offers a moderately useful handbook for the reference shelf. It also points to a need which it does not fulfill—a literate summary and analysis of the functions of all international economic organizations in relation to specified or alternative goals of international economic policy.

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International Economic Problems. By JAMES C. INGRAM. New York: John Wiley & Sons, 1966. Pp. x, 180. \$4.95; paper, \$1.95.

Professor Ingram has written a good introductory, though comprehensive, book of the major problems confronting the world economy. Addressed primarily to elementary students and laymen, the 180-page text provides, first a theoretical background of basic principles of international trade and monetary organization, and then focuses in the last three chapters on three international trade topics which are both timely and important: trade between advanced (A) and underdeveloped (U) countries, the European Economic Community (EEC), and the international monetary system.

The author stresses the importance of a student understanding the comparative advantage theory in order that he may be on "surer" grounds when approaching current and complex issues of trade and development. Some parts of this exposition might seem to be oversimplified, i.e., "we have a clear basis for ranking products in order of factor intensity." But, then, the purpose of the exposition is to provide the student with the bare essentials, and the state of controversy in fields like the terms of trade, infant industry argument, and economies of scale is incorporated. It is pointed out that categorical statements about the secular decline in the terms of trade, or of the "potential" comparative advantage of the infant industries, or of the magnitude of the economies of scale, are hard to make. At the same time, the importance is recognized of the U countries' stand at the United Nations Conference on Trade and Development (UNCTAD): that A countries should open up their economies to U countries' exports and reduce the protection accorded to those of A countries' industries in which U countries have a comparative advantage, i.e., light manufactures. Perhaps because of the relatively recent development of the concept of "effective protection"—that is, protection granted on the value added—economic implications of effective protection (which tend to disfavor U countries' manufactured exports) are not treated as fully as one would wish. Nevertheless, coverage of the U countries' trade problems is, in general, comprehensive and interesting.

In discussing the economic benefits that the EEC has derived from their regional grouping, the author rightly points out the lack of factual evidence to settle conclusively the economies of scale argument. But, on the resource allocation effect, no mention is made of the studies by Verdoorn, Scitovsky, Johnson, and Welmesfelder, all of which have consistently shown the scant degree of the benefit of this effect. Statements like "trade creation appears to have outweighed trade diversion" and "most observers think that the connection is close" (between the formation of the EEC and good fortune) are attributed generally to the rapid increase in income and trade. But the fact that income of these countries grew rapidly even before the Common Market is not spelled out. With respect to the future course of the EEC and its impact on the rest of the world, however, Ingram does emphasize uncertainty, pointing out the undercurrent "inward-looking" attitude and agricultural policy. In this connection, the importance of the Trade Expansion Act (Kennedy round negotiation) is discussed.

Discussion of the instability and dilemma of the existing payment system and its analysis in terms of "expenditure switching" and "expenditure reducing" as means to improve the trade balance of a country should prove exciting to the student. In connection with the analysis of U.S. balance of payments deficits, the improved "competitive position" as reflected by a sharp rise in current accounts of the U.S. economy during 1958-64 *vis-a-vis* Europe is explained in terms of the comparatively slow rise in wages and prices in the United States. But when the author goes one step further and says that this reflects the inherent strength of the American economy, one wonders; the student might get the unwarranted impression that "competitiveness" necessarily reflects the basic comparative advantage of the economy and that these terms can be measured so easily. Furthermore, had Ingram provided figures for the merchandise and service accounts for the preceding years (in addition to the comparison of 1964 with 1958), students would have discovered easily that the 1964 surplus was untypically high. Discussion of selective measures imposed by the United States, i.e. "interest equalization tax" and "buy American," should provide the perceptive student an instance of the inconsistency of the professed claims of free trade and the national interest of countries. The shortcomings of these measures, however, could have been delved into further.

In the next edition of the book, one would like to see one problem in particular discussed, viz., the prospects of regional groupings among the U countries (some steps toward this in Latin America and their limited success so far are very briefly mentioned). Currently, there are several other such views, e.g., the Regional Harmonization of Development Plans proposed by the ECAFE secretariat. Should trade liberalization precede regional cooperation, or should it be just incidental to the basic coordination of national development plans among U countries? Questions like these should make an interesting discussion.

This book should serve as a good supplement to an introductory text. Those belonging to other disciplines such as political science and international relations should find this absorbing single-volume account of contemporary economic problems extremely interesting.

SEIJI NAYA

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Business Finance; Investment and Security Markets; Insurance

The Capital Expenditure Decision. By G. DAVID QUIRIN. Homewood, Ill.: Richard D. Irwin, 1967. Pp. x, 258. \$7.50.

This slender volume provides a systematic review of the principal aspects of capital expenditure analysis. Quirin's approach is to describe the basic methodology of discounted cash flow approaches to capital budgeting, to review the relevant literature for areas of conflict or unresolved questions in theory, and to attempt formulation of original solutions to these problems.

The result is a fairly balanced text which should provide a novice with a reasonable understanding of the uses and limitations of capital expenditure

theory. At the same time there is sufficient new material, including a number of useful and imaginative illustrations, to attract the attention of those already well versed in the area. As this latter group might imagine, however, Quirin's solutions to heretofore irresolvable problems are generally heuristic, and the theoretical gaps which previously existed still remain with us. Particularly when we drop, as Quirin does in his final two chapters, the convenient assumption that the economic world is one of certainty is it clear that there is yet no definitive solution to the question of appropriate corporate strategy regarding capital structure, or to that of optimal capital budgeting policy.

The first two chapters provide a brief introduction to the importance, in both the private and public sectors, of a rational scheme of resource allocation. Embracing only 23 pages of text, they might easily have been combined. The third chapter provides, in only 30 pages, an examination of the relative merits of seven different methods of evaluating proposed expenditures. Most of the attention centers on discounted cash flow criteria, of which Quirin's preference is for the ratio of discounted gross benefits to discounted gross costs. If a recurring flaw is to be cited, it is the brevity of the discourse on important topics. For example, he terminates (p. 34) the discussion of Gordon's payoff period article,¹ when the relationship between the payback period and the internal rate of return might well have been developed fully.

This chapter suffers from unnecessary quibbling with previous authors. For example (p. 40), he erroneously attributes to Bierman and Smidt² the notion that ranking of projects by net present value is independent of the level of the discount rate, and then goes on to disprove it. Again, in examining the problem of multiple rates of return (pp. 54-5) Quirin provides references to five authors apparently selected solely for their failure to resolve the problem of evaluating multiple rate projects to his satisfaction. Quirin himself simply avoids the question by concluding that a project should be accepted if net present value is positive at the firm's cost of capital, whatever the rate of return might be. In the theoretical world of wealth optimization and unlimited capital, the issue indeed disappears, but it remains with us when capital rationing exists. Later (p. 180), when discussing budgeting with capital rationing, Quirin suggests that a schedule which ranks projects by rate of return must indicate not only when a multiple rate project will be accepted, but also when it must be removed from the accepted list as available capital is allocated to projects offering progressively lower yields. For all the critical comment on earlier writers one expects a more satisfying solution.

An interesting section on narrow vs. wide margins (p. 63) includes the suggestion that benefit and cost streams should be discounted separately, rather than netted in each year, as is the customary practice. The suggestion is particularly relevant in the case of mutually exclusive projects, among which a gross benefit-cost ratio provides greater discrimination as a ranking measure than does the internal rate of return. Quirin then extends the notion to the sit-

¹ Myron J. Gordon, "The Payoff Period and the Rate of Profit," *Journal of Business*, October 1955.

² H. Bierman, Jr. and S. Smidt, *The Capital Budgeting Decision*. New York, 1960.

uation in which a vendor finances equipment purchases for customers. Although we agree that financing and purchasing of fixed assets should generally be considered separately as Quirin suggests, it is not always possible to meet this ideal. In particular, when the most attractive financing proposal is offered by a vendor whose equipment, considered alone, fares only second-best on a benefit-cost ratio scale some combined methodology must be employed.

A second, more detailed, chapter on capital expenditure policy under certainty is followed by two chapters on the cost of capital. It is in the second of these that the principal original material appears. Here Quirin effectively distinguishes the short-run cost of capital from that which obtains in both the intermediate and long run. Concentric isocost ellipses, bounded by attainable levels of debt and equity, are mapped on axes representing total amount of debt and equity. Quirin then develops an expansion path for short-run average capital cost by plotting points of tangency between the curves and parallel lines representing various levels of total capital. In the intermediate run, as financing options are increased, the slopes of the marginal and average cost curves decline. In the long run, after the market adjusts to the effects of new financing, both average and marginal cost curves are horizontal, irrespective of capital structure. Thus Quirin affirms the long-run validity of the Modigliani-Miller thesis, although we may doubt that the required market adjustment will ever be perceptible in a world of imperfect information and continual change.

When considering capital budgeting under risk (Chapter 11), Quirin develops a useful application of the Markowitz portfolio selection model. Proposed projects are sequentially combined with existing operations and with all previously accepted projects to determine whether acceptance of the project in question will add to net present value. This requires prior specification of variances, covariances, and the market's risk preference schedule. Rejected projects which show promise are retested after each new acceptance because of the possibility that a high negative correlation may reduce total variance. Quirin fails to point out that after the last project has been evaluated and a package accepted it is possible that net present value might be further increased by withdrawing a project previously accepted. This would, of course, require that its removal lower total variance and the required capitalization rate sufficiently to offset the decrement to the expected earnings stream.

Quirin's intended audience includes those interested in capital budgeting for both industry and government, and two chapters are devoted specifically to the public sector. These are the weakest chapters in the text, being argumentative and critical rather than analytical. Little new is offered except for the author's personal observations concerning the illogical ways which governments use to allocate their citizens' dollars, and existing theory is accorded only hurried, superficial discussion.

Quirin's text should find wide application in both graduate business programs and management development seminars. It contains relatively few problems, a minimum of present value tables, and little reference to refinements such as accelerated depreciation, continuous compounding, con-

tinuous receipts, and so forth. Its brevity, while sometimes frustrating to one familiar with the subject, makes it a convenient capsule course on an important subject of economic inquiry.

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Industrial Organization; Government and Business; Industry Studies

The Demand for Natural Gas in the United States. By PIETRO BALESTRA. Contributions to Economic Analysis No. 46. Amsterdam: North-Holland Publishing Co., 1967. Pp. 153. \$7.00.

This book presents an econometric investigation of natural gas demand in the United States residential and commercial market. The author works up from an overly simple static model to a more elaborate dynamic one. In the latter, which is clearly more appropriate, he divides natural gas demand into new and existing parts. He points out that the high transfer costs of shifting between alternative fuels make price effects almost irrelevant to consumers already locked into existing equipment. These effects carry weight only in the investment decision when a new heating system is purchased or an old one replaced. Judicious use of assumptions leads to a model embodying this phenomenon in which the variables are all observable, and the parameters of this model are estimated.

The data used are by state for 1950-62. Estimation for the static model is for each state separately; estimation for the more satisfactory dynamic model uses first cross-section data for individual years and then pooled cross-section data. Evidence is found for a structural break at about 1956, and Balestra suggests that this is due to the characteristics of gas as an essentially new fuel, penetrating new markets in the early fifties. The final estimates are for 1957-62.

It is in the securing of these final estimates that the book reaches its econometric high point. Very careful attention is given to the problems of pooling cross-section and time series data, and best asymptotically normal (BAN) estimates are secured using a disturbance model which is superior to existing efforts in this area. The usefulness of this procedure is clearly not restricted to studies of the demand for natural gas and it deserves the close attention of general econometricians.¹

Thus the final model used is a good one and the estimating techniques better than that. Nevertheless, the book seems unsatisfactory in several respects.

First, it is rather padded. Far too much time is spent on the estimates and properties of obviously unsatisfactory models; further, elementary material or

¹ Balestra and Marc Nerlove, who cooperated in developing this procedure, have published it separately as P. Balestra and M. Nerlove, "Pooling Cross-Section and Time Series Data in the Estimation of a Dynamic Model: The Demand for Natural Gas," *Econometrica*, July 1966, 34, 585-612.

material readily available elsewhere is sometimes spelled out at unnecessary length.

More important than this, however, is the lack of much discussion as to the economic implications of the results. Considering the importance of the characteristics of natural gas demand for regulatory policy, one would have liked to have seen some treatment of the policy issues. Instead, the final estimates are presented almost as an afterthought to the treatment of estimation procedures.

Such failure to discuss economic issues shows up elsewhere as well. The short-run demand for gas is the demand for the services of an existing stock of capital equipment. Balestra finds it "difficult to conceive of a theoretical situation in which a change in income is accompanied by a short-run change in gas consumption" (p. 23). Yet apparently he has no such difficulty with the short-run effects of price. This discrepancy is left unexplained, and when the long-run dynamic model is developed, the rate of equipment utilization is assumed constant.

Further, there is a closely related question which Balestra leaves undiscussed. In their work on electricity demand, Fisher and Kaysen² found an interesting pattern of short-run effects. In particular, they found that for much of the nation, the pattern of short-run income elasticities over states was closely related to the degree of urbanization. They tentatively explained this in terms of the amount of time spent in the home, thus giving a theoretical basis for the existence of short-run income effects. Balestra notes the results but not the interpretation and finds that urbanization has no important effects on natural gas demand. That may well be true, but it is not clear why there should be a difference in this regard between gas and electricity-using consumers. Balestra does not discuss this.

In fact, Balestra fails to test the urbanization hypothesis in the proper form. He enters degree of urbanization as a separate variable in his cross-section dynamic analysis and finds its effects statistically insignificant. Yet the Fisher-Kaysen hypothesis is exclusively applicable to the short run and to only roughly two-thirds of the states. More important, it is an hypothesis as to income elasticities which is not properly tested by the addition of urbanization as a separate variable. Again, urbanization and short-run income effects may or may not be important, but Balestra too easily dismisses the subject.

Similarly unsupported statements turn up also in some other places. Balestra states (p. 25) that an unweighted average of the prices of various fuels is a better measure of opportunity cost than an average weighted by quantities consumed. This is not obvious to me if the availability and relative prices of alternative fuels vary over observations.

Finally, Balestra assumes (p. 50) that the relative prices of appliances using different fuels can be omitted from the demand regressions. He justifies this in terms of discounting initial cost over a long period and by casual empiricism as to current practices of price setting in the appliance and fuel mar-

² F. M. Fisher in association with C. Kaysen, *A Study in Econometrics: The Demand for Electricity in the United States*, Amsterdam 1962.

kets. The arguments are not wholly convincing and so important a point should not have been passed over in such a fashion.

In short, I should have liked to see a book a good deal shorter in many places and rather expanded in some. The present book seems to me to contribute more to econometric methodology than it does to our knowledge of the gas industry. Nevertheless, the general quality is high and contributions of the latter kind are by no means lacking. Balestra has advanced our quantitative knowledge of demand behavior in the energy industries; one hopes for more such studies.

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Defense Management. Edited by STEPHEN ENKE. Englewood Cliffs, N.J.: Prentice-Hall, 1967. Pp. xiii, 385. \$12.95.

The editor's purpose in assembling the twenty essays that comprise this volume was to describe the use of cost-benefit (or systems) analysis during the last six years in the Department of Defense. It is possible to draw upon production theory to distinguish a cost-benefit problem from an operations research problem in the defense management field in the following (oversimplified) manner. Typically, operations research begins with a given "input mix" and seeks to define the point on the "production function" over it, when the production surface depicts the maximum "product" return attainable from input mixes. Cost-benefit analysis, on the other hand, (1) assumes the production function in relevant input mix ranges has already been defined, usually along one or more isoquants, and (2) permits input mixes to be varied, ordinarily to find a minimum-cost input mix on a given isoquant.

Part I of the book contains five essays concerned with the McNamara-Hitch innovations in defense planning and program budgeting. William A. Niskanen's discussion of the Five Year Force Structure and Financial Program and the program budget is excellent, and nowhere more so than in his expression of some misgivings that the Five Year Program may introduce some anti-innovatory rigidity into Pentagon planning, especially under the extreme centralization of decision making that characterizes the McNamara era. Robert N. Grosse and Arnold Proschan discuss the program budgeting procedure in greater detail but do not go far beyond the exposition of the process. Norman V. Breckner and Joseph W. Noah discuss the costing of systems in very general terms, providing little in the way of realistic illustration, and touching such important problems as time discounting and sensitivity analysis in brief and unsatisfying ways. The following two chapters by Roland N. McKean and Armen A. Alchian survey in repetitious fashion some of the problems and limitations that inhere in the new budgeting and planning procedures. McKean makes two important points: (1) the major importance of the new methods is that they force the planner to consider trade-offs in effectiveness, but (2) any other reasonable system would also call attention to trade-offs, so that choices among such procedures must be based upon the merits of the trade-offs they feature. From this part, however, it is fair to con-

clude that the Novick-Mosher-Smithies-Hitch-McKean program budget analysis of the nineteen fifties has been marvellously effective in practice, although it is no panacea and may contain some potential for rigidity.

Part II contains nine essays which can be grouped under the heading "Cost-Effectiveness Applications" only by some stretching of the meaning of the term. Rather straightforward accounts are given of the earlier, more mundane work at RAND by Richard B. Rainey, Jr., John J. McCall, and Harrison S. Campbell, dealing with force-mobility vs. prepositioning, maintenance of equipment, and spare parts management respectively. While they are competently done, the essays move at a level of summary and nontechnical treatment that is introductory in nature, and the topics are by now a bit overexposed. In a similar, if less cut-and-dried, vein, the essays by R. H. McMahan, Jr. and D. H. Taylor (Central War Alternatives), Herman Kahn and Anthony J. Wiener (New Perspectives on Civil Defense), and Frederick T. Moore (Incentive Contracts) are competent but lackluster, and the Kahn-Wiener piece leaves the definite impression that the authors are rather bored with the whole subject. Jora R. Minasian did the best he could with the rather unimportant problem of land utilization for defense (the Department of Defense holds only about 3.5 per cent of the land held by all Federal agencies) and highlights some rather obvious problems. I found Harry J. Gilman's essay on military manpower utilization a fine example of rather straightforward application of minimum-cost reasoning to defense problems; despite the fact that I would question his too-easy assumption that military draft manpower should be valued at civilian labor market prices, the essay recommends itself as reading in courses where such illustrations of real-world economic reasoning are valuable.

Unquestionably, one of the two highlights of the book is the brilliant essay by James R. Schlesinger on the changing environment of systems analysis. Schlesinger represents the second generation of defense analysts, who must subordinate the simpler analytical problems of deterring spasm nuclear attack to the subtler issues of flexible response and limited nonnuclear war. To a great extent the nature of such environments and scenarios makes the quarter-century old determinate techniques of game theory, programming, and operations research appear dated and overly restrictive upon the imagination. An important strain of the Hitch-McKean witness was that stressing the importance of economic theory as metaphor rather than model; as a guide for steering the expert's intuition along useful paths; as a chisel to shape in frankly inexplicable ways the incommensurable and the intangible into the potentially useful and concrete.

Schlesinger argues, for example, that the basic characteristic of uncertain phenomena is not that they are unforeseen but that they are unforeseeable. An implication of this is the fatuity of hopes for developing rigorous frameworks to cope with uncertainty. Given this assumption, therefore, one may turn his attention to less intellectually satisfying but more fruitful methods of hedging against the unknown future. Schlesinger argues most interestingly and convincingly that one hedge is to postpone decisions until they must be made, and cites as evidence the Kennedy administration's choice of *Polaris* and *Minute-*

man systems as one which froze our deterrent posture unnecessarily into a small-yield missile type. On the basis of information that existed at the time the decision was made, delay was possible and would have permitted a better system to be developed, and therefore the decision made was an inefficient one within the context of these new and less formal techniques.

Part III is another miscellany grouped under the heading "Special Defense Problems," and contains essays on research and development by T. K. Glenan, Jr., on the impacts of defense spending upon the domestic economy (Murray L. Weidenbaum) and upon the balance of payments (Rolf Piekarz), on the planning difficulties of alliances as displayed by NATO (A. W. Marshall), and on military assistance programs (Charles Wolf, Jr.). Once again the level of competence of the work is good if little true novelty is forthcoming, and the essays contain excellent sources of basic data on the several problems as well.

Also in this part is the second highpoint of the volume: an essay by Martin J. Bailey on the potential of decentralizing decision making in defense management, especially through the devices of internal and shadow prices. Bailey recognizes the difficulties of extending these methods to governmental processes lacking the profit criterion, but also feels that in the defense subeconomy under strong presidential leadership the social welfare function can be unambiguously defined. Bailey draws an interesting comparison between the Eisenhower administration's dispersion of such preference-definition among the several services and the Kennedy administration's more Crusoe-like procedure. The essay concludes with a brief illustration of the methods such a plan might adopt in the determination of air- and sea-lift facilities.

One of the disappointing aspects of the work inheres in the introductory nature of most of its presentations: it fails to come to grips with the grander and deeper issues of economic decision making in the defense area. Notably, the need for a major extension of standard welfare economics concepts from the market mechanism's regime to the realm of governmental decision making is peculiarly acute. In the Breckner-Noah, Minasian, and Gilman essays the important question recurs concerning the manner in which one values funds, land, and manpower in defense uses when they have not in fact been bid away from the market by price methods. The authors either take the market's price as the proper valuation without further discussion or discuss alternatives inadequately. And again, the extremely limiting problem of determining criterion variables to implement the effectiveness part of the analysis is not discussed at length in the essays, implicitly lending support to Admiral Rickover's allegation that the accent in cost-effectiveness is indeed on costs.

It was surprising, too, to find that nowhere in the book was the TFX decision drawn upon to illustrate the discussion or to point up the limitations of extensive cost-effectiveness studies when the Secretary of Defense feels intuitively that a low bidder's cost estimates are "less realistic" than a high bidder's. Further, the important impacts the whole affair has had upon head-to-head bargaining and prototype contracting might have argued for some such reference.

Nonetheless, the book is an excellent survey in nontechnical terms of the

McNamara-Hitch revolution in programming and budgeting, cost-effectiveness, and some of the broader economic impacts of the defense budget. In addition it gives some internal evidence of the shift of systems analysis away from the rigorous and determinate model approach to subtler and more imaginative methods of meeting current problems. Its major drawbacks are that it says little that is new, it is (probably inevitably, given the differential in the rates at which defense problems arise and the publishing process moves) a bit dated in its discussion of explicit problems, and it is much over-priced at \$13.

ROBERT E. KUENNE

General Economic Systems Project

The Steel Industry of India. By WILLIAM A. JOHNSON. Cambridge, Mass.: Harvard University Press, 1966. Pp. xv, 340. \$7.50.

This is a useful study of India's experience in expanding her steel industry during the past 10 years. The book's chief merit lies in its careful empirical testing of theoretical criteria (e.g., the social rate of return) to decide whether or not (1) the Indian Government has been wise to give such heavy emphasis to iron and steel, (2) the new mills have been put at sensible locations and have adopted the most appropriate production processes, and (3) these big public projects have achieved reasonable operating efficiency. To the first two queries he answers "yes"; to the latter he answers an ambiguous "no."

The most interesting part of the book is Johnson's measurement of the social returns from the industry's expansion during the second and third Plans. He uses the capital costs of a representative new government mill, the operating costs of the largest private mill (Tata's), and the average c.i.f. price of imported steel to calculate a normative internal rate of return (13 per cent for a .77 mn. ton plant, 21 per cent for a 1.5 mn. ton plant). The values were calculated with shadow prices for foreign exchange (the 1966 devaluation made them market prices) plus a shadow price for coal but not for labor. He notes that these values bias the result downwards, i.e., well-executed projects ought to have been able to do better. In practice, of course, the new government mills (plus the expanded private companies) have done worse—worse in terms of their social returns and far worse in terms of their private returns (the government mills have all experienced heavy losses). Johnson puts most of the blame on the government's control policies for steel, especially its misplaced desire to keep steel prices low to help development and its unwillingness to let public and private producers make high enough profits. Johnson believes that if the government had dared to rely more heavily on prices to curb demand and to discourage over-ordering the saving in demand would have eliminated the need for one of the three new public-sector plants. His feeling that Fourth Plan steel targets are too high is finding short-run support today as Indian steel demand turns slack. Johnson also blames poor management for part of the low returns in the government plants; but his comments on the managerial problems of these plants do not distinguish clearly enough among general problems of government ownership in a democracy, specific problems of organization and control, and the teething problems of new plants anywhere.

On another topic of current relevance in India—the pressure to sacrifice

sound locations to political considerations—Johnson concludes that the four new government plants have all been reasonably located even though each is in a different state. All are in the northeast, where the iron ore and the metallurgical coal are found. However, Johnson has such a hard time coming out loud and clear against a South Indian location (for which there has been considerable pressure) that one doubts such a mill would sacrifice much economics to politics.

The book has nine chapters, eight appendices, 45 tables, a 17-page bibliography, and a good index. The compression of a thorough and well-documented dissertation into a well-organized, well-focused book is not easy, and the author and his editors deserve only middling grades for organization and selectivity. The discussion of iron and steel technology and its relation to the factor proportions problem is disappointing in an area that might have been of great interest. An excess of the subjunctive mood robs many statements of their bite. In the end, not much is said about some of the larger issues of industrial growth at which the author bravely aimed. But a book need not be a paragon of readability and wisdom to be a highly useful contribution. Mr. Johnson has written a book anyone interested in Indian steel definitely should read.

G. B. BALDWIN

*International Bank for Reconstruction
and Development*

Land Economics; Agricultural Economics; Economic Geography; Housing

Northern California's Water Industry. By JOE S. BAIN, RICHARD E. CAVES, AND JULIUS MARGOLIS. Baltimore: The Johns Hopkins Press, for Resources for the Future, 1966. Pp. xvii, 766. \$15.00.

It is not possible to do justice to this volume in the space of this review. The book consists of 766 pages, much of which is highly technical in nature. The work obviously represents a research undertaking of considerable magnitude. It would have been interesting to know more about the actual research undertaking; only the most fragmentary information is available in the preface regarding the amount of research resources, the personnel involved, and the amount of time devoted to the research on which the book is based.

The book consists of twenty chapters and four appendices. There are four parts to the book—Part I—Structure; Part II—Conduct; Part III—Performance; and Part IV—Summary and Policy Proposals. The fruits of the book are not harvested easily. Not only is the book long and technical, it is also rather poorly organized and written. Nevertheless, the returns from careful study will exceed the cost if one wishes to become informed on the topic under discussion.

The authors use the conventional tools of industry analysis to study the water industry of Northern California. To this reviewer's knowledge, this is the first effort to use this particular set of analytical tools to study water problems. On page 5, the authors state:

An industry study thus tests a set of hypotheses taking the following general form: a given constellation of elements of market structure generate a certain range of patterns of market conduct which, in turn, give rise to certain qualities and defects of market performance. It is not an exercise in unstructured description. Nor does it "test" the axiomatically true—or at least irrefutable—hypothesis that the behavior of human beings can somehow be explained by the environment in which they live. Rather, the industry study tests refutable hypotheses drawn from economic theory.

In this reviewer's opinion, the work is an impressive one. No doubt it will come to be considered something of a landmark in resource economics. It is important to give the basis for the above statement before proceeding so that a rationale can be established for the remainder of this review.

1. The volume is impressive because of what it contains in empirical information about the waters of Northern California and their development, allocation, and use.
2. The volume is impressive because of the always competent and, at times, imaginative use of economic analysis in treating the empirical information bearing on the subject in question.
3. The volume is somewhat less impressive in treating hypotheses resulting from market structure, conduct and performance theory. The reader is never quite sure just what hypotheses are being tested. The main value of the particular theory used would appear to be as a point of departure in organizing the research.
4. The book is not revolutionary with respect to economic evaluation of natural resources. There are many interesting and useful applications developed as particular problems are analyzed. Yet there appears to be no significant contribution to economic evaluation methodology as such.
5. The book is quite impressive in its integrated treatment of economic and political motivation. Some exceedingly interesting material in this connection can be found in Chapter 8 entitled "Conduct of Water Agencies: Organization Motives and Normative Criteria."
6. The policy recommendations are neither surprising nor especially original. Most have been advanced at one time or another by other economists or students of water resource development. However, the policy recommendations are impressive in this particular case because of the documentation.

What, then, are the main conclusions of this ambitious research? The reader is warned that the following treatment is too brief to do justice to the contents of the book. With respect to development, the authors conclude that the practices followed have resulted in excessive development. It appears the California Department of Water Resources has erred more in this respect than have the Federal agencies. The authors conclude that violations of allocative functions have, at times, in the sequence of development assumed significant proportions. With respect to the current situation in California, the following quote is significant:

The Feather River project . . . under present plans will undertake a clearly excessive aggregate interbasin transfer. Not only is the project excessive in aggregate scale; it also includes segments that could be eliminated, which now drag down its overall economic rating. In general, the proposed transfers to the North Bay region, to the Mojave Desert, and to the Palm Springs area for urban use, lack any visible economic justification in toto; the proposed transfer to Kern County for irrigation is, at best, overly large, and any transfer from the Eel to feed these dubious delivery areas is economically unsupportable. It is conceivable that a scaled-down project designed mainly to carry Feather River water to the South Coastal Basin might have economic justification.

The authors trace the cause of the above misallocations to the institutional framework, and develop policy recommendations to correct these evils. Among their recommendations:

1. Two independent commissions, one at the national and one at the state level, with extended powers over the authorization of water projects.
2. Changes in the law to encourage the marketability of water rights.
3. Vesting in the State Water Quality Control Board regulatory powers which would permit them to establish "strict standards for the abatement of all types of pollution" and ". . . a schedule of penalties or taxes which would be payable to the state by various classes of water users for unabated or nonabateable pollution" (p. 668).

The authors are not naive with respect to the political feasibility of their proposals. They say, with respect to the first recommendation above,

This is not the most politically saleable idea ever advanced. But we believe that it has substantial merit, and could be worth a substantial sales effort (p. 664).

One is led to wonder how the authors can analyze the economic and political situation as carefully as they have, and then with apparent comfort offer recommendations such as those listed above. It should be obvious that the implementation of the above recommendations will have to contend with powerful forces. Undoubtedly these forces could be identified from the research under review. Yet the recommendations are simply stated, as though there were some ready client eager to snap them up and put them into practice. As mentioned above, the same type of conclusions and recommendations have been put forth by others for some time, without apparent effect on public decision making. Why? Perhaps Resources for the Future, Inc. should finance an effort to answer this relatively simple question. Otherwise, the financing of such elaborate studies may help educate a small group of professional economists and provide research training for aspiring professional economists without having any visible effect on the allocation of our riches for water resource development.

There are additional features of the book which should be mentioned, both of a plus and minor nature. Appendix A, entitled "A 'Pure' Theory of the Demand-Price Relationship for Irrigation Water," appears to be a distinct con-

tribution to the literature. It should make agricultural economists blush to realize so little has been done in this field, although not all of the relevant literature on the subject has been cited. The book is marred by confusing writing and shallow thought in places. This is surprising when one considers the fact that the economists are obviously competent, and that careful analysis is carried forth in other parts of the book. For example:

Any discrepancy between marginal cost and marginal value tends to signal an excess, or deficiency, in the quantity of resources employed in the activity in question. The *elasticity of demand* indicates the size of the misallocation associated with a given gap between marginal cost and marginal value (p. 162).

What about the elasticity of supply? What about the reallocation of resources associated with a redistribution of income? Again and again the authors refer to price as a determinant of demand. Yet these minor items should not detract the reader from the substantive and rewarding parts of the book.

EMERY N. CASTLE

Oregon State University

The Economics of Outdoor Recreation. By MARION CLAWSON AND JACK L. KNETSCH. Baltimore: The Johns Hopkins Press, 1966. Pp. xx, 328. \$8.50.

This book is about the social and economic aspects of outdoor recreation. It presents some new material and brings together in a convenient form many ideas, analyses, and viewpoints that are widely scattered. Most important, Clawson and Knetsch apply the analytical tools of economists to the problems of outdoor recreation. They fit their material into a broad analytical framework that consists of a supply-demand or market approach. They generally summarize the arguments concerning policy issues rather than advocate specific policies. Clawson and Knetsch direct their book primarily toward park and recreation workers and students in the field of outdoor recreation rather than toward professional economists. But economists concerned with outdoor recreation will find much of interest in the economic analysis and in the discussion of policy issues. The authors acknowledge receiving comments on all or parts of the book from fifty-two individuals who are presumably experts on some of the problems discussed. This is enough to give any reviewer an inferiority feeling.

The book has five parts. Part I includes a discussion of various terms and concepts used throughout the book. Part II is concerned with the demand for outdoor recreation. It is the longest and to me the most interesting part of the book. The authors discuss basic determinants of demand for outdoor recreation, attempt to estimate the demand facing particular sites, and review methods of estimating future demand. Part III is a discussion of various aspects of the supply side of outdoor recreation. It includes quality considerations, statistics concerning existing recreation facilities and their use, and the relation of available recreation resources to the location of population. Part IV brings demand and supply together to discuss various problems and policy issues concerning outdoor recreation. It covers such topics as the valuation of

resources for recreation, the local economic impact of spending for outdoor recreation, the role of public and private agencies, investment criteria, and pricing policies for outdoor recreation facilities. The final part of the book is a look at the future; it includes recommendations for needed research and discussions of major policy issues of the future.

The attempt of Clawson and Knetsch to develop demand curves for outdoor recreation represents a major advance over the employment of *use* as a measure of demand. But their application of demand analysis encounters some difficulties. The first occurs in defining the units for measuring quantity. They define the thing to be measured as the *whole recreation experience* (anticipation, travel to, on-site experience, travel from, and recollection) associated with a visit to a specific recreation area; the unit of measurement is the *visit* by a family or a party. Their demand curve thus shows the relationship between the cost of a visit to a specific recreation area and the number of visits to that area during some time period. Variations in the cost per visit result primarily from the different distances traveled in getting to the recreation site. To estimate this demand curve, the authors use data collected by others in surveys of visitors to particular recreational areas. They convert total visits by residents of various distance zones around a recreation area into attendance rates for the populations of the zones. From this price-attendance rate relationship they move to the demand for specific recreation resources which is the relationship between visits and admission charges at the recreation site.

Their derivation of the demand for specific recreation resources assumes that the observations of visits and costs for the whole recreation experience are points along a single demand curve. Clearly this assumption is not valid—each observation represents only a point on the demand curve for each distance zone. The authors are aware of this inadequacy of the data but feel that their technique gives reasonable though low estimates of “true” demand. I suspect, however, that a probabilistic gravity model similar to the one developed by David L. Huff¹ is more promising, especially for estimating the demand for new recreation areas. The estimation of the parameters of the gravity model would, of course, require surveys of households. We could learn much more about demand curves if public recreation areas would experiment with admission charges.

Several other topics and discussions in the book particularly interested me. I can mention only a few of them. The authors rightly reject the argument that free public parks help redistribute income in favor of the poor. Low income groups cannot travel to distant outdoor recreation facilities so cannot benefit from low park admissions. The discussion of the local impact of outdoor recreation suggests that tourism may not be a panacea for depressed areas. A recreation or tourist business can be a hazardous undertaking, the activity is highly seasonal, and pay scales for most employees of such businesses are among the lowest of major industries.

The attempt to remain neutral on policy issues and to present pro and con arguments leads to discussions that are not as incisive and clear as they could

¹ David L. Huff, *Determination of Intra-Urban Retail Trade Areas*, University of California at Los Angeles, 1962.

be. Sometimes I could not determine whether the authors were presenting their views or someone else's, or whether they accepted certain arguments as valid. I would have been more critical of the arguments for public rather than private provision of outdoor recreation facilities. There is no evidence to show that the political decision-making process has less serious imperfections than a private outdoor recreation market nor that judgments concerning the future are better if publicly made. On another critical note, the last two chapters repeat much material that appears earlier—one short chapter summarizing future policy issues and needed research would have been better.

In summary, this book performs an important function by employing an analytical approach in the discussion of problems of outdoor recreation. Earlier discussions usually lacked such an approach. By bringing together widely scattered materials and presenting them in an integrated and consistent manner, the book also serves a useful function. It should provide both a starting point and a stimulus for further research on this topic.

RICHARD L. PFISTER

Indiana University

Essays in Urban Land Economics—in Honor of Leo Grebler. Los Angeles: University of California Real Estate Research Program, 1966. Pp. 351. \$6.50.

This is a collection of essays in honor of Leo Grebler. It is a good collection. The sixteen essays are divided into five parts covering housing economics; housing finance; urban land economics; the housing industry; and the city. The book is a very interesting mixture of surveys of the economics of problems and policies, and some new material on specific facets of the housing market, with the former group predominating. As with any group of essays written by a number of people, the thread of continuity is not sharp, but divergence is much less than is usually the case, and is not particularly disturbing, as the essays are interesting and useful.

The papers provide a good deal of insight into the fields covered. Factual surveys on housing finance and on FHA-FNMA policies and effects are presented by Robert M. Fisher and Robert C. Weaver respectively. Ernest M. Fisher traces the effects of rent control using New York data. The analysis points out results that could have been (and were) expected on the basis of good economic analysis.

A marked characteristic of the essays is the use of good economic analysis. In exploring such diverse subjects as Chester Rapkin's treatment of price discrimination against Negroes, and land value estimates by Paul F. Wendt and William Goldner, it is clear that economics has much to contribute.

M. Carter McFarland traces the history of residential rehabilitation work, examining the effects of public policy on private decisions to increase rehabilitation efforts, or to continue the downward trend which, as it turns out, may be highly profitable. This is another example of sound economics having some-

thing to say about why problems exist and how we might better be able to attack them.

An essay on place prosperity dealing with some welfare considerations involved in public decisions redistributing economic activity, by Louis Winnick, is a real delight. It points out, in a very sharp but fair manner, the implications of various policies such as various urban renewal schemes and the Appalachia program for locational decisions. The true gains and losses involved in the shifts and their equity considerations, as students of these actions realize, are not all accounted for in the public statements of those responsible for these programs. A mayor of a city often has more reason to rejoice at the opening of a new industrial plant than has a federal administrator, but, as Winnick observes, you can barely tell this at the ceremony. Public officials and public policies have indeed outpaced the economists in this area, but essays such as this will help. To the rapidly growing number of people interested in this whole question, the benefits of this essay alone are greater than the cost of the entire book.

An essay by Fred E. Case deals with some of the characteristics of the enduring city, in which he cites many of the failings of current American cities, and some of the things which he and many would like to see cities become. He is more concerned with these objectives than with the reasons why they have not been achieved in the past. Some of these are supplied by Frank G. Mittlebach in a most interesting essay on the entrepreneurial influences in shaping the American city. Using illustrative examples, he points out many reasons for people making the types of decisions that ultimately shape cities. It is in this essay, more than in any of the others, that the pervasive disassociation of costs from beneficiaries in urban development (which does so much to shape the urban environment) is explored. One is a bit puzzled to find this type of economic consequence so lightly treated in a volume of this nature.

Leland S. Burns traces the threads of economic development theory and the role of housing investments in development in the first essay. After noting that housing often ranks low in dollar payoff as compared to investments—for example, in industrialization—he proceeds to supply a convincing argument that indeed there may be a higher payoff than that accounted for in GNP. Here is the familiar problem that economists are always running into, that many things are indeed valuable even though not measured in the same metric as National Income. Nonmarket economics, or such questions of market failure, are coming more and more to the attention of economists, and rightly so. Burns similarly rejects the extreme position that housing investment ought to be viewed as an end in itself, and takes particular exception, rightly, to estimates so often turned out that so many millions of units of housing are required “to meet unsatisfied needs of developing countries.” He observes that housing is critically short in underdeveloped countries, and in relative shortage everywhere else. The pity is that such statements of requirements, though nonsense, make such good copy and receive the attention they do.

The one difficulty found is that the book is not so much a collection of essays on urban economics as on housing economics. Beyond the possible mis-

nomer here, this may lie at the root of some of our difficulties in urban economics; that we may be preoccupied by housing policies, not always taking into account a broader economic interest. Only a couple of the essays attempt to relate housing matters to the formation of urban regions.

JACK L. KNETSCH

The George Washington University

The Role of Agriculture in Economic Development: A Case Study of Pakistan. By MAHMOOD HASAN KHAN. Wageningen, Netherlands: Centre for Agricultural Publications and Documentation, 1966. Pp. 161.

The analytical framework suggested by Khan for the process of economic development is a modification of the Rostovian model. First, he holds that Rostow's schema makes insufficient allowance for the variety of ways in which differing economies can fulfill the preconditions for take-off. Second, he assumes that the take-off in modern times will practically always be consciously induced by government action. Third, he accepts as essential to the Rostow thesis that the take-off is characterized by a rapid increase in capital formation (it is shown that 10 per cent of income being invested is insufficient where population growth is rapid) and by the establishment of one or more leading sectors. Fourth, he adds two essential conditions for take-off to those outlined by Rostow: (a) Agricultural productivity must show substantial improvement to provide food and raw materials for the expanding economy and (b) underemployment in the agricultural sector must be absorbed so that by the beginning of the stage of self-sustained growth surplus agricultural labor is exhausted.

Khan shows that the transformation of agriculture as manifested in rising productivity is absolutely essential to sustained industrial development. Increasing agricultural productivity enables the farmers to supply the food requirements for the growing urban and industrial population (obviating inflation). It allows the agricultural sector to release the labor force needed for industrial expansion and it expands the rural market for industrial products. Therefore, the method of transforming traditional agriculture is of crucial importance and he summarizes generally accepted methods under three headings: (i) acceptance by farmers of new or improved farm inputs; (ii) adoption of improved farm cultural practices; and (iii) introduction and assimilation of managerial innovations.

The discussion outlined above comprises the first two chapters of the book and gives the theoretical framework upon which the consideration of Pakistan is based. Aside from the modification of the Rostovian model, the value of these chapters is in the careful and concise reorganization of accepted ideas, rather than in new departures.

Three chapters follow on the nature of the Pakistan economy and the progress of its development with emphasis on the agricultural sector. These chapters give an amazingly balanced and concise summary of the traditional agricultural economy, its problems and its progress toward transformation. These

chapters are descriptive and their value lies in their careful summary of the literature with a few additions from unpublished documents.

The final chapter is an assessment of the prospects for the economic development of Pakistan with emphasis on its agricultural aspects. The official Perspective Plan for 1965-1985 calls for a doubling of per capita income and the provision of full employment by 1985. Khan asserts that this objective is impossible under the provisions of the plan for two basic reasons.

1. He estimates that population growth will average about 3.3 per cent per year instead of 2.7 per cent as assumed in the plan. Not only will the faster population growth reduce the projected growth of per capita income, but it will absorb much capital and will greatly increase the number of new jobs which must be created in order to overcome chronic unemployment.

2. The plan projects a growth of 5.6 per cent per year in the agricultural sector, but Khan estimates that this would require a growth of agricultural labor productivity of 4.7 per cent per year as compared to 1.5 per cent achieved recently. The plan does not show how this phenomenal transformation can be achieved and Khan reasons that it is impossible. Therefore, he concludes that substantial progress, but slower than that planned, should be expected.

WILLIS D. WEATHERFORD

Berea College

The Egyptian Agrarian Reform, 1952-1962. By GABRIEL S. SAAB. New York and London: Oxford University Press, 1967. Pp. xvi, 236. \$8.00.

The Agrarian Reform Law was the first major legislative act of the revolutionary Egyptian government under Nasser after King Farouk was overthrown in 1952. Now for the first time a scholarly, objective book in English is available evaluating the results of ten years of reform. Dr. Gabriel Saab was farming in the Nile Delta at the time of the revolution and later served in various departments of the UAR government as well as with the F.A.O. Later he was Director of the Institute of Rural Economics in Beirut. In order to study the Agrarian Reform Laws, Saab visited the major agricultural "mant-iqat" centers for an on-the-spot comparison between plans and implementation. He no longer lives in the UAR and has been free to write this frank, perceptive appraisal of the successes and failures of the monumental effort of reform.

The Egyptian "fellaheen" were ready for revolutionary changes in 1952. Sixty-five per cent of the 6 million feddans (one feddan = 1.038) of cultivated land was owned by six per cent of the landlords. The remaining thirty-five per cent of the land was fragmented into plots that averaged only 2.2 feddans and were owned by 2,769,000 peasants. Rents paid by tenants were extraordinarily high; wages to farm laborers on the large estates were at subsistence levels and sharecroppers were often required to turn over 100 per cent of their cotton crop and 50 per cent of their wheat to the landlords. Yields per feddan were low; water was wasted; drainage was poor; animal power predominated; health was miserable and unrest was widespread.

The major features of the Agrarian Reform Laws are described by the author in specific helpful chapters. (1) Land ownership was limited to 200 feddans per person or 300 feddans per family. In 1961 the maximum was reduced to 100. (2) Some of the land in excess of the limit was sold privately to buyers but at depressed prices. (3) The government expropriated or acquired a total of 955,000 feddans representing 15.5 per cent of the cultivated area (p. 28). The price paid was equal to 70 times the annual land tax—an amount equal to approximately 40 per cent of the market price in 1952. Payment was made in 30 years, 3 per cent non-transferable government bonds which were later repudiated. (4) Some of the land was resold to carefully selected experienced farmers in plots up to 5 feddans. (5) Cooperatives were established in the villages to plan the triennial rotation of crops, to supply seed, fertilizer, and insecticides, to provide agricultural credit and to market the crops. Membership was compulsory for those who received distributed land but voluntary for others. (6) Improvements were made in rural housing, sanitation, health and education. (7) New desert lands were reclaimed in the Sahara, west of the Delta in the Liberation Province and in the oasis areas of the New Valley.

Dr. Saab lists some solid achievements of the first ten years. The land has been redistributed to an additional million Egyptian peasants and the political and economic power of the large landlords has been reduced. There has been a slight improvement in the living conditions of the rural families—perhaps by 10 to 15 per cent in a decade. This has resulted primarily from a reduction in costs, like rent and seed, rather than from increases in income. There has been about a 45 per cent increase in the volume of agricultural output from 1953 to 1962. At least 40,000 feddans in the Liberation Province (El Tahrir), 35,000 feddans in the Egyptian American Rural Improvement Society (EARIS) and 100,000 feddans in the New Valley have been brought under cultivation for the first time.

However, in spite of this vertical and horizontal expansion of agriculture, most Egyptian observers, including this reviewer, are well aware of the inadequacies of the program. Saab points out that the initial enthusiasm of the leaders has disappeared; the bureaucracy is all-prevailing; decision making is painfully slow; land litigation is complex; priorities are uncertain; rising income is used for consumption rather than for vital saving; the agricultural infra-structure is deteriorating; and all the mammoth effort seems to help so few. Egypt has discovered that it is not easy to make a farmer out of an agricultural laborer. Dividing the land is not sufficient. Agrarian reform is far more complex than land reform. Other factors must also accompany social change in the areas of education, health and social organization.

"In conclusion," writes Saab, "the land distribution programme could best be described in 1961 as a major delaying action to win time by redistributing wealth. . . . It has fulfilled most of its initial targets . . . without any grave disruption of agricultural production, and the political power of the feudal landowners has been broken. The state (has) placed formidable obstacles in the way of a self-sustained process of economic, social and culture growth through its reluctance to envisage the emergence of any spontaneous construc-

tive initiative in the redistributed estates. . . . It has thus impressed on the whole land-distribution programme static characteristics gravely endangering its successful long-term evolution" (p. 140).

With this valid conclusion, it is not likely that other nations will copy the Egyptian Agrarian Reform; but no scholar interested in this topic or in the UAR should fail to read Saab's descriptive and factual book. Theoretical economists will miss any references to the actual planning process; to input/output ratios; to zero marginal productivity; to balance of payments difficulties or under-employment. But if one wishes to know how it was done in Egypt this is a very clear description.

ALBERT L. GRAY, JR.

Baldwin-Wallace College

Labor Economics

Employment, Race, and Poverty. Edited by ARTHUR M. ROSS AND HERBERT HILL. New York: Harcourt, Brace and World, 1967. Pp. ix, 598. \$3.95.

This is one of four volumes of articles resulting from the Research Program on Unemployment and the American Economy. This program was financed by the Ford Foundation "administered by the Institute of Industrial Relations" on the Berkeley campus, and its co-directors were Arthur M. Ross and R. A. Gordon. Herbert Hill, labor secretary for the NAACP, joined Ross as co-editor of this volume containing twenty separate contributions.

Since so much of the literature dealing with employment patterns of American Negroes is chock-full of the dreary and dismal record of white America, it is pleasing to be able to report that this volume contains evidence that things are changing, at long last, for the better. Co-editor Ross, with the aid of a curious statistical technique that uses absolute differences to measure proportionality, does not wholly concur with this estimate; but the evidence in the volume pushes the reviewer to that conclusion. Without attempting to rate these items in order of importance, the following listing of welcome changes is the basis for a generally optimistic view.

1. Ross notes that "One development in the 1959-1964 period is particularly striking . . . Negroes gained 134,000 skilled trades jobs although the total increase was only 425,000 and 41,000 sales jobs although the total increase was only 62,000. This represents a sharp break from the past" (p. 42).

2. Samuel Krislov, writing on federal government employment, informs us that "Clearly, the Equal Employment Program has seen truly significant changes in the four years of its enforcement. Not only have Negroes garnered 20 per cent of the new positions in the Federal service in this period, but they have significantly altered their pattern of employment" (p. 348).

3. Direct action techniques have scored some successes in previously all white employment situations. For instance, August Meier reports that ". . . the agreement CORE made in the summer of 1964 with the Bank of America in California was a major victory that provided for the hiring of 8,000 Negroes in a twelve month period" (p. 197).

4. Membership exclusion practices by trade unions remain the most recalcitrant single problem area in the fight for fair employment, but even here we find that these vicious practices are now being subjected to a wide array of weapons. Paul Norgren reports that Fair Employment Practices (FEP) Commissions in New York and Philadelphia have been successfully implementing fair employment practices and recently, in the sixties, have finally moved with some success against trade union violators. Most important for the question of union exclusion practices is the change of heart manifest on the NLRB. In an exhaustive and well-done article, lawyer Sanford Rosen explains how the NLRB has come to the position of defining exclusionist activity as an "unfair labor practice" and how this can be useful in moving against such conduct. Moreover, even though Title VII of the 1964 Civil Rights Act contains provisions for eliminating racist membership policies, Rosen makes an impressive case that the NLRB can do the job better.

5. Though George Strauss qualifies his article on management attitudes toward the fair employment question by restricting most of his evidence to the San Francisco area he does report that "Government and Negro action group pressures have pushed many employers past color-blindness to affirmative action" (p. 287). Consistent with this is his statement that "Placement officers from the better colleges report that the demand for Negro graduates far exceeds the supply" (p. 284).

6. And two writers, Norgren and Rowan, make it clear that the fears expressed by all concerned that integration in Southern plants would meet with great resistance have been consistently and grossly exaggerated. Rowan, after reviewing evidence in three plants in Birmingham, notes that "Relations between white and Negro employees have been good in the plants, and transition under the new integrated promotion system has been orderly, with scarcely an incident" (p. 334). Norgren seconds this by saying that "the conclusion seems warranted that racial equality in employment can be promoted through FEP legislation in the South . . ." (p. 566).

In contradistinction to the estimate made above that the winds of change are blowing beneficently, Charles Killingsworth concludes his diagnosis as follows: "The point is that the trends of the past quarter century in Negro migration, birth rates, death rates, occupational shifts and school attendance are now yielding the largest increases in Negro population in the regions and among the groups where Negro disadvantages are greatest" (p. 72). There is supporting evidence elsewhere in this volume, evidence that deals with the intractable ghetto problems, the minimum of returns received so far from our retraining efforts, and the debilitating effects of long-term harsh treatment.

This dichotomy, between improvement on the one hand and the degradation of the ghetto on the other, points up the major failing of research done on Negro employment patterns: the failure to note the growing class cleavage amongst Negroes. It should be understood that the items listed above as contributing to an optimistic view do make things better for that growing body of educated and skilled Negroes, but are simply no help to the school dropout, the unskilled, the functional illiterate.

In the preface to this useful volume of essays, many of them well written, the co-directors of the program explain that the reason for bringing together these essays is "... to furnish better guidelines for policies ..." (p. v). The best place to start is with the recognition that ghetto problems and fair employment problems, while compounded in difficulty and related by the race question, are essentially different.

MALCOLM H. LIGGETT

San Francisco State College

Labor Economics: Wages, Employment and Trade Unionism. By ALLAN M. CARTER AND F. RAY MARSHALL. Homewood, Ill.: Richard D. Irwin, 1967. Pp. xii, 584. \$8.50.

This is a textbook aimed at the introductory labor economics course, presuming a prior knowledge of the principles of economics. The book is divided into three principal sections: (1) Comparative Labor Movements (about 185 pages); (2) Wages, Employment, and Collective Bargaining (about 185 pages); and (3) Public Policy (about 190 pages). The book's most unusual features are an extensive discussion of labor movements in other countries and a somewhat greater emphasis on the economic theory of labor markets than is found in the average text.

In general, the book is competently written, and it probably surpasses the standard of the average level of undergraduate teaching in labor economics. The chapters on labor market theory are followed by appendices with more detailed graphical presentations of production functions, bargaining models, and so on. The exposition is clear, and there is no reason why undergraduates who have passed the usual principles course should have difficulty in understanding the material presented. The material on comparative labor movements helps to place American labor economics in the perspective of labor movements in other countries, and this should be especially valuable for the general student who takes only one course in the field. Also commendable is a two-chapter section devoted to the problems of minority groups in the labor market (Chapters 21 and 22).

Despite the fact that this book can only be described as competently conceived and written, I have an important reservation about its usefulness. That is, while a substantial part of the book is devoted to an exposition of the theory of the labor market, there isn't enough really serious effort devoted to helping students see the relevance of theory to understanding labor market behavior. The relevance of this criticism probably is not shared by a large number of the teachers of labor economics in the United States, and the authors might be unwise in considering the criticism, even if they should think it valid. Nevertheless, my opinion is that textbooks of all kinds and at all levels ought increasingly to reflect the great strides that economists are making in using economic theory to understand economic events. This means that a large part of the empirical material covered should be treated from the point of view of its relevance to economic theory; it also means presenting more empirical materi-

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al than is usual and selecting the material presented in the light of theoretical considerations.

My criticism extends even to the early chapter on theories of the labor movement. It seems to me pedantic to outline a large number of alternative and sometimes conflicting theories of the labor movement without devoting some space to considering their relevance and usefulness in the light of available evidence. Merely to present the theories becomes too much like Biblical exegesis for my taste. It may lead students to think that economics has little to offer but abstract theorizing.

If the authors had focused on testing economic theory, they would not have been so ready to make the statement that "Labor unions historically have been the major force in reducing the length of the standard work week" (p. 286). While the statement may be true, recent studies of labor supply imply that hours of work have responded to rising income levels. These alternative explanations ought to be contrasted with each other in the text, and an attempt should be made to resolve the conflict.

I question the relevance of presenting an appendix outlining a bargaining model which does not also present data of behavior which the model helps to understand (Chapter 13). While it may be true that the results of collective bargaining need the theory of bilateral monopoly to explain them (Chapter 12), the student is forced to accept this statement on the basis of the authority of the authors.

In Chapter 14, the authors discuss the impact of unions on fringe benefits. Again, while it may be true that unions have had a substantial impact on the composition of the typical wage package, the authors do not present an alternative hypothesis, that employers in a competitive market would have an incentive to offer the kinds of wage packages that their employees desire. If it is true, as the authors state, that the growth of the importance of the federal income tax has had something to do with the increasing demand for fringe benefits, then they ought to consider at least the possibility that fringe benefits would have grown in importance in the absence of unions.

The general issue that bothers me about this text (and it is common to practically all labor economics texts) is that despite the presentation of theory, data, and institutional material, the treatment is compartmentalized to a great extent (perhaps less in this book than in many others), and the hand that writes one section seems to be unaware of the activities of the hand that writes the other. I don't think this is the optimal way to train citizens, who ought to be developing an intellectual framework for dealing with issues on which they would like to form intelligent opinions, or economists, who ought to be gaining an understanding of an orderly and efficient means of drawing information about the world from data.

Nevertheless, given the currently prevailing standards of teaching in this field, the book ranks above average in conception and execution; teachers looking for a textbook to assign their students should give this one serious consideration.

BELTON M. FLEISHER

The Ohio State University

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Population; Welfare Programs; Consumer Economics

Studies in the Economics of Income Maintenance. Edited by OTTO ECKSTEIN.
Washington, D.C.: The Brookings Institution, 1967. Pp. xvi, 254. \$6.00.

Economists have been slow to apply sophisticated techniques of analysis to social insurance and welfare problems. Difficulties of definition and quantification of important variables have presented obstacles to such an approach.

In two of the four studies in this book, multiple regression analysis is extensively used. The studies were originally prepared as doctoral dissertations at Harvard under Otto Eckstein, who has supplied a brief introduction to the volume. They analyze particular aspects of Federal Old-Age, Survivors and Disability Insurance (Henry Aaron); of unemployment compensation in Massachusetts (Charles Warden, Jr.); of federally supported State welfare programs (Lora S. Collins); and of unemployment caused by the closing of the Mack Truck Plant in Plainfield, New Jersey, in 1961 (John W. Dorsey). In addition, comparisons are made of social security expenditures in 22 countries (Henry Aaron).

For the most part, the results of these studies are suggestive rather than definitive. Numerous relationships are explored and tentatively assessed, but generally, additional testing of hypotheses is necessary before they can be accepted with confidence.

Among the interesting findings in these studies are the following:

1. A cross-section analysis of 1957 social insurance and welfare expenditures in 22 countries indicates that the length of time a country has had a social insurance system is the most important single determinant of the percentage of its national income devoted to social security expenditures. The older its old-age or health insurance program, the larger is the social security percentage of its national income. Also, very wealthy countries generally spend proportionately less national income on social security.

2. Countries that make heavy use of general revenues to finance social insurance outlays tend to have a smaller proportion of national income devoted to social security than countries relying solely or largely on earmarked taxes, usually on payrolls. This finding may raise doubts about the wisdom of widespread use of general revenue financing for the social insurances in this country.

3. States with low per capita income tend to have a high percentage of their population of various public assistance programs and low average assistance payments to clients. Interstate variation in income level can account for more than half of the interstate variation in the proportion of the population on public assistance.

4. Analysis of 27 industries showed that, between 1947 and 1958, one-third of the unemployment benefits in Massachusetts were caused by cyclical swings, one-third by seasonal variations, about one-eighth by secular declines in employment, and the remainder divided between irregular (frictional) factors and unidentifiable elements. Nine-tenths of the benefits in building construction were attributable to seasonal fluctuations. Presumably experience

rating in unemployment compensation, which rests on individual employer responsibility, is inappropriate for unemployment stemming from cyclical or secular demand factors.

Although State Employment Service personnel thought that separation pay (mean of \$1,338) and \$50-a-week unemployment compensation caused the well-paid Mack workers to refuse jobs much more readily than they would without such financial security, multiple regression analysis did not support that claim. Dorsey's conclusion (p. 233) that "In general, unemployment benefits covered recurrent or ordinary expenditures for most families" seems to be refuted by his statistics (Table 16) showing unemployment benefits amounting to only 81 per cent of recurrent weekly expenditures.

Although many of the statistical results are fairly commonplace and make dull reading (particularly the 75 pages on public assistance expenditures), some results are quite interesting and should stimulate further investigations. Warden's initial reflections on experience rating in unemployment compensation in the light of his industry benefits analysis are one example.

The handling of the incidence of social insurance taxes by two of the authors illustrates the dangers of uncritical acceptance of conventional views. Aaron says (p. 20) that "There is virtual unanimity that payroll taxes levied on the employer are largely shifted, . . ." That is not true of widely varying State tax rates (from zero to 4 per cent) under experience rating in unemployment compensation. Because Warden neglects the incidence of State unemployment taxes, his conclusions about experience rating encouraging a shift of labor costs from habitually unstable industries to stable industries are certainly subject to question. Surprisingly, he does not attempt to reconcile his conclusions about the inappropriateness of experience rating for cyclical and secular-decline unemployment with his notions about the shifting and bearing of the full labor costs under experience rating.

One hopes that other doctoral theses will carry this type of analysis of social insurance and welfare expenditures further and help to clear up some of the mysteries, inconsistencies, and questionable conclusions that plague this subject area.

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The Changing Faces of Economic Insecurity. By JOHN G. TURNBULL, with MALCOLM S. COHEN AND MARY PEPPE. Minneapolis: University of Minnesota Press, 1966. Pp. ix, 157. \$5.00

Whether economic insecurity is greater or less than it was fifty years ago, whether the relative importance of the different causes of insecurity have changed and whether society is making more or less adequate provision for alleviating such insecurity as occurs are intriguing questions. By economic insecurity Professor Turnbull means income deficiency which he sees as being primarily due to "income curtailment" or "increased expenses." In fact, under the latter head he deals only with medical expenses, and only briefly with these. Although early in the book he mentions substandard wages as a

factor causing economic insecurity he presents no historical comparisons in this area.

His book concentrates on loss or interruption of income and deals with four of the causes, namely what he terms premature death (meaning thereby "death that leaves financial obligations unmet"), old age ("living too long in economic terms"), unemployment, and illness and accidents (for the latter, occupational only). In chapters devoted to each of these risks he compares the frequency of occurrence in 1960 and 1910, and the effectiveness of what he terms "alleviative programs" aiming to replace through organized measures some fraction of lost income. Of these he deals only with the social insurances because, presumably, of his belief that income maintenance should be based on right rather than need. Even so, the omission of the Veterans' Administration programs seems difficult to justify, while disregard of the public assistance programs involves an underestimate of the effectiveness of public measures in the areas of both income maintenance and medical expenses.

He finds that "there has been a decrease in the frequency of economic insecurity caused by premature death, old age [sic], economic unemployment and illness . . . Old age has increased as an economic insecurity problem . . . One may conclude that the decrease in insecurity from premature death and illness has more than offset the increase in insecurity resulting from old age" (p. 136). One may perhaps agree with his findings about illness, although his estimate of the size of the decrease seems very optimistic and he says little about the rising costs of medical care. But his selection of the age group 14-65 as the age span in which premature death may occur involves a serious underestimate of the importance of this risk as a cause of economic insecurity for it neglects the increasingly important group of aged widows, who thanks to greater longevity and their longer life expectancy relative to men, are coming to form a major component of the economically insecure population. There is even more doubt about the decrease in the frequency of economic insecurity caused by economic unemployment. Turnbull's own analysis of unemployment trends points out that much depends on the years selected as dates for comparison ("if one sets 1960 against 1935 the improvement is apparent: if one compares 1960 with 1906, 1960 does not look so good" pp. 72-73). And while one can agree that society will never again tolerate a level of unemployment such as was experienced in the 1930's, account must be taken of the fact that "the pervasiveness of long-term unemployment appears to have been on the increase since 1945" (p. 71). An evaluation of the significance of unemployment as a cause of insecurity would also call for consideration of the relative prevalence of irregular and partial employment. No evidence is presented for the statement that the increase in insecurity due to old age has been outbalanced by declines in the frequency of the other risks. In fact, if the medical break-throughs to which Turnbull looks forward (e.g. in the area of degenerative diseases) occur, old age will become a still more serious factor making for insecurity.

But even if one grants that economic insecurity because of interruption or loss of income is probably less than it was fifty years ago, owing both to de-

clining frequency of certain causes of non-earning and to the development of a structure of public alleviative programs providing some measure of income replacement, one would have expected that a study of the changing faces of economic insecurity would have given attention to other causes of insecurity which are becoming increasingly important. Prominent among these are what might be termed the sociological factors, notably family breakdown and, on what Turnbull calls the increased expenditure side, size of family. 65 per cent of the four and one-half million people, three-fourths of them children supported by the AFDC program, are there because the father is absent from the home due to divorce, separation, desertion or failure to have married the mother. And the numbers thus supported are far smaller than they should be, owing to the restrictive practices of many states which deny eligibility to families for reasons other than failure to pass a needs test. Similarly, Orshansky's analyses in the *Social Security Bulletin* have shown that of the over 30 million people living below the Social Security Administration poverty line over 5 million are children living in families where the breadwinner had been fully employed during the preceding year, while about half of the 15 million children in the poverty group were members of large families. Had Orshansky's data been available to Turnbull it seems likely that he would have been even more hesitant in hazarding the guess that "the psychological causes of poverty are increasing and the economic (premature death, old age and so on) lessening" (p. 134).

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Consumer Choice in the American Economy. By CAROLYN SHAW BELL. New York: Random House, 1967. Pp. xvii, 429. \$7.95.

The title, *Consumer Choice in the American Economy*, states what this book is about: consumer choice, consumption patterns. It also analyzes the market in which the consumer makes choices. Professor Bell's book ends with the same quotation from Adam Smith with which Elizabeth Hoyt's *Consumption in our Society* began in 1938. The composition of our Gross National Product hardly bears out the idea that "Consumption is the sole end and purpose of all production."

Consumer Choice in the American Economy first takes up budget studies, then theories of demand (consumer preferences, utility, and indifference), next considers the consumer market by means of description and models (price competition and non-price competition), lastly it discusses consumer protection.

The book is intended to be a text or supplementary book for a course in consumer economics which might be taught in economics, sociology, or marketing departments or in a school of home economics. The approach of the book is theoretical, analytical, and quantitative. It achieves very well the limited scope which the author has selected.

Among current books on consumption David Hamilton's very good *The Consumer in Our Society* had novelty and freshness when it was published in 1962; Arch W. Troelstrup's popular *Consumer Problems and Personal Fi-*

nance has gone into a third edition. Dr. Aurelia Toyer's paperback, *Get Your Money's Worth*, has readability and practicality. Toyer's book is useful for a union, or a consumer counsel, or an urban antipoverty group. Mrs. Bell's text has amplified and updated the numerous statistics in the consumption field. It is probably the most useful current text for liberal arts college students.

The treatment of diminishing marginal utility and of indifference curves is conventional. It compares favorably, however, with the demand theory analysis in such a book as Samuelson's *Economics*, and is substitutable.

The consumer protection section is concerned with welfare, information, and "Truth in Lending," but it does not show much consciousness of the changing world such as Kenneth Boulding exhibits in *Principles of Economic Policy* (sections on consumer sovereignty, welfare involvement, "income maintenance policy," and need for ethical theory arising out of conflict in value systems).

Mrs. Bell expresses a number of interesting value judgments in her book; for example, she is critical of mass production because it limits variety of choice although she recognizes the low unit cost of much of it. She thinks that the main justification of governmental consumer counsels is to improve information available to consumers. With regard to establishing standards of quality she prefers more enforcement of existing laws rather than imposing quality standards and product specifications. She regards consumers' own experience as the main source of information. Thus, advertising has the function of informing the consumer about variety, new products, and of helping to break his old habit of consumption.

The consumers of the poorer countries are not the concern of this book. Much of the world population spends over 40 per cent of its income on food. In some parts of Brazil over 65 per cent is spent for food. To teach a course in consumption to undergraduates and leave the students without comparison of their own extraordinary ability to command consumer goods and services with that of a large part of the world's population would seem to me to belong to the past.

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Related Disciplines

The Organization of Inquiry. By GORDON TULLOCK. Durham: Duke University Press, 1966. Pp. vii, 232. \$5.50.

In the words of the author, "The purpose of this book is to answer, or attempt to answer, certain questions about science." Science is intrinsically cumulative and cooperative; one man's work builds on that of others'. Judged by the results, science is organized quite efficiently. Yet "there is no central coordinating organization and . . . there is no more reason to believe in some sort of divine guidance for science than for economic activities." What, then, is the nature of the coordinating mechanism? For the system of science to work, scientists not only must work on problems of interest to other scientists,

but also must report their results honestly. "How does it happen that we can depend upon scientists to refrain from faking research results?" To answer these and related questions the book attempts to "investigate the nature of the scientific community . . . to explain why the individual scientist, who feels quite free and unconstrained, is nonetheless led to investigate problems of interest to others, [and to] exercise the most extreme precautions to insure accuracy."

Tullock's treatment of this broad topic is imaginative and perceptive. He points out that the basis for a coordination and control network rests, first, on the pragmatic interests of applied scientists and those who employ them, and, second, on the real interest in finding truth of at least some members of the basic science community. These two groups potentially can establish standards of relevance and reliability for the work of others. The potential is realized and the network given life through the funds provided by people interested in and willing to pay for practical results, the belief of many practical men and scientists that certain areas of basic research are important, and the desire of scientists, both applied and pure, to get ahead.

Tullock sees the pragmatic test of technical and economic relevance of results, turned into profit opportunities by the patent system, as guiding and controlling research by applied scientists. For the basic science community, Tullock sees the academic advancement system, and the role of publication in respected journals in that system, as the central mechanism. Editors want their journals to be important. If a journal is to be important it must publish articles that are read, favorably received, and not proven false. Both pure and applied scientists read the journals whose articles are in the fields of their interest, and are likely to be significant and reliable. Thus scientists, both pure and applied, interested in a field either because they have been paid to be or because they simply are interested, provide incentives for others to do research whose results they can use.

There is a lot to these ideas and others that Tullock elaborates in his book. Any reader is sure to pick up a number of insights and ideas.

There also is a lot to the book that is irritating. It may be a slight injustice to the author to say that the tone of the book should have been as follows: "I have been doing some thinking and reading about incentives and controls in the organization of scientific research, and here are some of my ideas." I doubt, however, that the implied level of serious research by the author falls grossly short of the mark. Throughout the book sentences and paragraphs take on a tone more suitable in a much more scholarly or more rigorous study. The author further makes the mistake, in the eyes of this reviewer, of pontificating on policy in a superfluous and superficial concluding chapter.

Despite these annoying problems—which really involve style—the book is well worth reading. And it has the virtue of being short.

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- SNAVELY, T. R. The Department of Economics at the University of Virginia—1825-1956. Charlottesville: Univ. Press of Virginia, 1967. Pp. viii, 224. \$4.75.
- STAMPP, K. M. The era of reconstruction, 1865-1877. New York: Vintage Books, 1967. Pp. ix, 228. Paper, \$1.95.
- SUTCH, W. B. The quest for security in New Zealand—1840 to 1966. New York: Oxford Univ. Press, 1967. Pp. xvi, 512. \$10.40; paper, \$4.35.
- TARPEY, L. X. AND WHITAKER, W. M., III. Editorial policies and review procedures: a survey of leading journals. Lexington: Bur. Bus. Research, College of Bus. and Econ., Univ. of Kentucky, 1967. Pp. 51. Single copies free.
- TAYLOR, A. J. P. Bismarck—the man and the statesman. New York: Vintage Books, 1967. Pp. 286. Paper, \$1.95. First pub. in 1955 by H. Hamilton and Knopf.
- TOMA, P. A. The politics of food for peace—executive-legislative interaction. Tucson: Univ. of Arizona Press, 1967. Pp. xiv, 195. \$3.95.
- VINGE, C. L. AND VINGE, A. G. U.S. government publications for research and teaching in geography and related social and natural sciences. Totowa, N.J.: Littlefield, Adams, 1967. Pp. xiv, 360. Paper, \$3.45. Revision of monograph pub. by National Council for Geographic Education in 1962.
- WARNER, W. L., ed. WITH UNWALLA, D. B. AND TRIMM, J. H. The emergent American Society. Vol. 1—Large-scale organizations. New Haven: Yale Univ. Press, 1967. Pp. xv, 665. \$15.
- WEBB, K. E. AND GROSSMAN, D. Geography. Totowa, N.J.: Littlefield, Adams, 1967. Pp. 231. Paper, \$1.95. 2000 multiple-choice examination questions with answers.
- WILENSKY, H. L. Organizational intelligence—knowledge and policy in government and industry. New York: Basic Books, 1967. Pp. xiv, 226. \$5.95.
- WOOD, A. L. Criminal lawyer. New Haven: College & Univ. Press, 1967. Pp. 335. \$6.50; paper, \$3.45.
- WUEST, J. J. AND VERNON, M. C., eds. New source book in major European governments. Cleveland: World, 1966. Pp. xviii, 700. \$8.00.
- YOUSE, B. K. AND STALNAKER, A. W. Calculus for students of business and management. Scranton: International Textbook, 1967. Pp. viii, 271. \$7.50.
- Education directory 1966-1967. Part 2—public school systems. Dept. of Health, Educ. and Welfare, Office of Educ. pub. Washington: Supt. Docs., 1967. Pp. 183. \$1.50.
- Engineering degrees (1964-65) and enrollments (fall 1965). U.S. Dept. of Health, Education and Welfare, Office of Education pub. no. OE-54006-65. Washington: Supt. Docs., 1967. Pp. 30.
- Federal support of international social science and behavioral research. Hearings before the Subcommittee on Government Research of the Committee on Government Operations, 89th Cong., 2nd sess., June 27, 28; July 19, 20, 1966. Washington: Supt. Docs., 1967. Pp. iv, 273.
- The Labour Party Foundation Conference and annual conference reports 1900-1905. London: Hammersmith Bookshop, 1967. Pp. 256. \$10.
- Problemas de administración pública en los países en desarrollo en la América Latina. Ciencia de la Admin. ser. no. 239. San José, Costa Rica: ICAP, 1967. Pp. 18.
- Statistical abstract of the United States, 1967. 88th annual ed. Prepared under direction of E. D. Goldfield, U.S. Bur. of the Census. Washington: Supt. Docs., 1967. Pp. xii, 1050. \$4.

NOTES

AER MANUSCRIPTS

The following manuscripts, exclusive of comments and replies and in addition to those listed in previous issues, have been accepted for publication in subsequent issues of the *American Economic Review*:

Giorgio Basevi, The Restrictive Effect on the U.S. Tariff Structure and its Welfare Value
Jagdish Bhagwati, More on the Equivalence of Tariffs and Quotas

J. M. Bonin, Consumption, Durable Goods Spending, and Changing OASDHI Seasonality
Bruno Contini, The Value of Time in Bargaining Negotiations: Some Experimental Evidence

J. R. Davis, Chicago Economists, Deficit Budgets, and the Early 1930s

C. J. Goetz, Earmarked Taxes and Majority Rule Budgetary Processes

D. R. Kamerschen, The Influence of Ownership and Control on Profit Rates

D. B. Keessing, A Country's Population and its Industrial Development: Some Evidence from Trade Patterns

Nissan Liviatan and David Levhari, The Concept of the Golden Rule in the Case of More than One Consumption Good

W. Y. Oi and E. M. Clayton, A Peasant's View of a Soviet Collective Farm

J. N. Rosse, A Curiosity in the Theory of Optimal Price Regulation: A Note

Hirofumi Shibata, A Note on the Equivalence of Tariffs and Quotas

Bo Sodersten and Karl Vind, Tariffs and Trade in General Equilibrium

G. E. Thompson, On Varying the Constants in a Linear Programming Model of the Firm

A. W. Throop, The Union-Nonunion Wage Differential and Cost-Push Inflation

Maurice Wilkinson, Factor Shares and the Direction of Technological Change

CANADIAN ECONOMICS ASSOCIATION AND CANADIAN JOURNAL OF ECONOMICS

For many years the Canadian Political Science Association has served as a common learned society for all Canadian social scientists. As the numbers of social scientists have grown, the Association has recently begun to subdivide itself into its components. The first step was taken when sociologists and anthropologists formed the Canadian Sociology and Anthropology Association. In 1966 it was decided to continue this process, by forming separate associations for political scientists and economists. The Canadian Economics Association thus has emerged from the Canadian Political Science Association, and now constitutes the Canadian learned society for economists.

The Canadian Economics Association, officially bilingual, hopes to facilitate scholarly contact among economists through its annual meeting and through conferences. However, its main activity will be publication of a new journal, *The Canadian Journal of Economics*, a bilingual quarterly whose first number will appear in February 1968. The *Journal* will consider materials in all branches of economics, pure and applied, both from Canadian residents and from others. It should serve as a medium for the dissemination of research findings relating to Canada. But it is not planned as a journal of Canadian studies. Its editors expect that its contents will be of interest to economists throughout the world. Manuscripts in English or French should be addressed to the Managing Editor, *Canadian Journal of Economics*, 100 St. George St., Toronto 5, Ontario, Canada.

Individuals may secure membership in the Association by writing to the Assistant Secretary-Treasurer, Canadian Economics Association, 100 St. George St., Toronto 5, Ontario, Canada. The ordinary membership is \$10.00 per year; the *Journal* is received with membership. Institutions may subscribe for \$10.00 per year.

PUBLICATIONS

The *Philippine Journal of Business & Finance*, a monthly, began publication in June 1967. All correspondence and manuscripts should be addressed to the Managing Editor, Suite 201, Manufacturers Building, Plaza Sta. Cruz, Manila, P.O. Box 1324.

A new international quarterly, *Socio-Economic Planning Sciences*, will begin publication in 1968. The new journal will particularly encourage contributions dealing with applications of systems analysis to the planning of public welfare and community services. Further information may be obtained from the Editor-in-Chief, Sumner N. Levine, City University of New York, Graduate Center, 33 West 42 Street, N.Y. 10036.

A new periodical, *The Annals of the Western Regional Science Association*, will begin publication in early 1968. Additional information may be obtained from the Editor, Michael K. Mischaikow, Department of Economics, Western Washington State College, Bellingham, Washington 98225.

The new editor-in-chief of *The Antitrust Bulletin* is Mr. James M. Clabault of Dunnington, Burtolow & Miller, 161 East 42nd Street, New York 10017. Manuscripts should be sent to him.

The U.S. Department of Commerce has published *Industry Profiles*, which provides basic information for each of over 400 four-digit Standard Industrial Classification industries for the period 1958-1965. The publication may be ordered from the U.S. Government Printing Office.

Announcements

A Visiting Lecturer Program in Statistics has been organized for the fifth successive year. The program is sponsored jointly by the principal statistical organizations in the United States, the American Statistical Association, the Biometric Society and the Institute of Mathematical Statistics. The National Science Foundation provides financial support. Leading teachers and research workers in statistics—from universities, industry, and government—have agreed to participate as lecturers. Lecture topics include subjects in experimental and theoretical statistics, as well as in such related areas as probability theory, information theory and stochastic models in the physical, biological, and social sciences. The purpose of the program is to provide information to students and college faculty about the nature and scope of modern statistics, and to provide advice about careers, graduate study, and college curricula in statistics. Inquiries should be addressed to: Visiting Lecturer Program in Statistics, Department of Statistics, Mathematical Sciences Building, Purdue University, Lafayette, Indiana 47907.

A department of demography in the College of Letters and Science was established in July, 1967, at the University of California, Berkeley. The only social science department of its kind in the country, this new unit will greatly expand the university's already extensive resources for training in the field of population studies. The department of demography grows out of an interdepartmental group-degree program established in 1965, which offered the first Ph.D. in demography in the United States. This group-degree program combined faculty from the departments of economics and sociology and from the School of Public Health. The new department retains the interdisciplinary character of the former program through joint appointments. Only M.A. and Ph.D. degrees are now offered but arrangements are underway for the inclusion of an undergraduate major in demography as well.

Inquiries should be addressed to Professor Judith B. Davis, Chairman, Department of Demography, University of California, Berkeley, California 94720.

The Institute for Social Research, University of Natal, is planning to hold a conference on "Focus on Cities—Studies in Contemporary Urban Life," in Durban, South Africa, July 8-12, 1968. For additional information, write to Mr. H. L. Watts, Conference Secretary, University of Natal, Durban, South Africa.

Authors of papers for possible inclusion in the Medical Care Section Program of the November 1968 meeting of the American Public Health Association in Detroit, Michigan, may obtain standard abstract forms from Dr. Donald C. Riedel, Yale University School of Medicine, 60 College Street, New Haven, Connecticut 06510. Two types of papers will be considered: research reports and descriptions of programs or demonstrations. The deadline for submitting abstracts is April 15, 1968. Those whose papers are selected for the program will be notified in early June.

The following unpublished paper, in mimeographed form, is available for interested persons and may be obtained by writing to the author at the address indicated: "A Simple Formal Model of Economic Growth," Gordon Winston, Yale University Pakistan Project, c/o Ford Foundation, P.O. Box 7282, Karachi—3, Pakistan.

The Asia Foundation is continuing for the fourteenth year its program of collecting useful books for distribution to institutions in Asia. Needed are books in excellent condition, in all college and adult level disciplines. Titles in the physical sciences should carry publication date of 1955 or later, in the social sciences and humanities, 1950 or later. Literary classics and anthologies of any date are welcome. Small quantities may be mailed by "Special 4th Class Rate—Books" directly to BOOKS FOR ASIAN STUDENTS, 451 Sixth St., San Francisco, California 94103. Book donations to the Asia Foundation are tax deductible.

Deaths

Thomas J. Anderson, Jr., professor of economics, School of Commerce, New York University, June 30, 1967.

S. R. Arnold, February 9, 1967.

Roger W. Babson, Babson Institute of Business Administration, March 1967.

Edward H. Chamberlin, professor emeritus of economics, Harvard University, July 16, 1967.

George R. Davies, professor emeritus of economics, Bureau of Business and Economic Research, University of Iowa, September 1967.

Max E. Fieser, Robert R. Nathan Associates, Afghanistan, May 26, 1967.

John H. Goff, professor emeritus of business administration, Emory University, September 7, 1967.

Franz Gutmann, lecturer emeritus, University of North Carolina, July 6, 1967.

Robert Szereszewski, The Eliezer Kaplan School of Economic and Social Sciences, The Hebrew University, Jerusalem, June 1967.

Charles J. Walsh, professor of economics, Fordham University, September 1967.

Carl Wood, January 13, 1967.

Retirements

Philip M. Brown, department of economics, Bowdoin College, January 1967.

Frank G. Dickinson, professor of economics, Northern Illinois University, June 1967.

William N. Loucks, professor of economics, University of Pennsylvania, June 1967.

Paul M. O'Leary, professor of economics, Cornell University, June 1967.

Simon N. Whitney, professor of economics, Rutgers—The State University, June 1967.

Charles R. Whittlesey, professor of finance and economics, University of Pennsylvania, December 31, 1966.

Visiting Foreign Scholars

Roy Harrod, Christ Church, Oxford: visiting professor of economics, University of Pennsylvania, fall 1967.

David J. Horwell, University of Southampton: visiting lecturer in economics, University of Pennsylvania, 1967-68.

Larry Levine, University of New Brunswick: Canada Council Fellow, Carnegie Institute of Technology.

Robin Marris, King's College, Cambridge: visiting professor of economics, Harvard University, 1967-68.

Michael McPhelin, Ateneo de Manila, Philippines: visiting professor of economics, Cornell University.

F. J. M. Meyer zu Schlochtern, Organization for Economic Cooperation and Development: visiting lecturer in economics, University of Pennsylvania, 1967-68.

Alec Nove, University of Glasgow: visiting professor of economics, University of Pennsylvania, spring 1968.

Max Peyrard, University of Paris: University of Utah, fall 1967.

Alan A. L. Powell, Monash University, Australia: visiting associate professor of economics, Rutgers—The State University, spring 1968.

Kai Shibata, Aoyama Gakuin University, Tokyo: visiting professor of economics, Carnegie Institute of Technology.

Roy W. Sturgess, University of Edinburgh: visiting lecturer in economics, Rutgers—The State University, 1967-68.

L. Roy Webb, University of Melbourne: visiting professor of economics, Cornell University.

John Willet, University of Melbourne: visiting professor of economics, University of Kentucky, spring 1968.

Giovanni N. Zanetti, Victoria University of Wellington, New Zealand: visiting assistant professor of economics, Michigan State University, January 1, 1968.

Promotions

Abbas A. Alnasrawi: associate professor of economics, University of Vermont.

Albert Ando: professor of finance and economics, University of Pennsylvania.

Alan R. Andreasen: associate professor of marketing and business administration, State University of New York at Buffalo.

R. M. Arunachalam: associate professor of economics, Platteville State University.

William L. Baldwin: professor of economics, Dartmouth College.

Robert A. Battis: professor of economics, Trinity College.

Nicholas A. Beadles: professor of economics, University of Georgia.

Mario L. Belotti: professor of economics, University of Santa Clara.

Ralph E. Berry: assistant professor of economics, Harvard University.

Eugene A. Brady: professor of economics, Iowa State University.

Conrad P. Caligaris: associate professor of economics, Northeastern University.

Joseph R. Cammarosano: professor of economics, Fordham University.

M. O. Clement: professor of economics, Dartmouth College.

Ward S. Curran: associate professor of economics, Trinity College.

Peter L. Danner: associate professor of economics, Marquette University.

John R. Darling: assistant professor of marketing, School of Commerce & Business Administration, University of Alabama.

- Bruce F. Davie: associate professor of economics, Georgetown University.
Tom E. Davis: professor of economics, Cornell University.
Lubomir A. D. Dellin: professor of economics, University of Vermont.
A. Deutsch: associate professor, department of economics and political science, McGill University.
Phoebus J. Dhrymes: professor of economics, University of Pennsylvania.
Peter B. Doeringer: assistant professor of economics, Harvard University.
George W. Douglas: assistant professor of economics, University of North Carolina.
Willard B. Doxey: professor of economics, Bribham Young University.
Mark Z. Fabrycy: assistant professor of economics, School of Commerce, New York University.
A. Fenichel: associate professor, department of economics and political science, McGill University.
Alfred J. Field: assistant professor of economics, University of North Carolina.
Charles B. Franklin, Jr.: associate professor of administration, Florida Atlantic University.
David C. Freytag: associate professor of economics, University of Cincinnati.
Gene A. Futrell: associate professor of economics, Iowa State University.
Robert E. Gallman: professor of economics, University of North Carolina.
James A. Gherity: professor of economics, Northern Illinois University.
Carl Gotsch: assistant professor of economics, Harvard University.
Neil E. Harl: professor of economics, Iowa State University.
William G. Harris: assistant professor of economics, Georgetown University.
Irwin L. Herrnstadt: professor of economics, Northeastern University.
Alan W. Heston: associate professor of South Asian economics, South Asia regional studies department, University of Pennsylvania.
Donald J. Hunter: assistant professor of economics, Iowa State University.
William E. Hurley: professor of continuing education, College of Commerce and Administration, Ohio State University.
Henry D. Jacoby: assistant professor of economics, Harvard University.
Hans E. Jensen: professor of economics, College of Business Administration, University of Tennessee.
Alfred E. Kahn: Robert Julius Thorne Professor of Economics, Cornell University.
Arlyn J. Larson: associate professor of economics, Arizona State University.
Howard Leftwich: associate professor of economics, University of Cincinnati.
Axel Leijonhufvud: associate professor of economics, University of California, Los Angeles.
Jane Leuthold: assistant professor of economics, University of Illinois.
David B. Lipsky: assistant professor of industrial relations, State University of New York at Buffalo.
Leo V. Mayer: assistant professor of economics, Iowa State University.
William J. McKinstry: professor of economics, Miami University.
Peter F. M. McLoughlin: professor of economics, University of Santa Clara.
Koichi Mera: assistant professor of economics, Harvard University.
William C. Merrill: associate professor of economics, Iowa State University.
Charles W. Meyer: professor of economics, Iowa State University.
John M. Mitchell: associate professor of business administration, Geneva College.
James B. Montgomery: associate professor of administration, Florida Atlantic University.
James L. Murphy: associate professor of economics, University of North Carolina.
Roger G. Noll: assistant professor of economics, California Institute of Technology.
Jerry H. Padgett: associate professor of economics, University of Georgia.

- Don V. Plantz : professor of economics, Arizona State University.
- John A. Powers : associate professor of economics, University of Cincinnati.
- James A. Prescott : associate professor of economics, Iowa State University.
- Richard N. Rosett : professor of economics, University of Rochester.
- Anthony Scaperlanda : associate professor of economics, Northern Illinois University.
- David L. Schwartz : assistant professor of economics, Albright College.
- Daniel M. Schydrowsky : assistant professor of economics, Harvard University.
- Elbert W. Segelhorst : associate professor of economics, California State College at Long Beach.
- Gene R. Simonson : professor of economics, California State College at Long Beach.
- Fred Slavick : professor, New York State School of Industrial and Labor Relations, Cornell University.
- J. G. Smith : associate professor, department of economics and political science, McGill University.
- James A. Stephenson : associate professor of economics, Iowa State University.
- Mahlon R. Strasheim : assistant professor of economics, Harvard University.
- Robert D. Taylor : assistant professor of accounting, School of Business Administration, University of Massachusetts.
- C. F. Joseph Tom : professor of economics, Lebanon Valley College.
- Ralph Underhill : assistant professor of behavioral sciences, Graduate School of Business, University of Chicago.
- Alice J. Vandermeulen : senior lecturer in economics, University of California, Los Angeles.
- Hylke Van de Wetering : associate professor of economics, Iowa State University.
- Frank E. Wagner : associate professor of economics, University of Missouri at Kansas City.
- Lawrence Weiser : assistant professor of economics, University of Illinois.
- Thadeus J. Whalen : associate professor of economics, University of Santa Clara.
- Theodore L. Whitesel : professor of economics, University of Texas at Arlington.
- G. Donald Wood : assistant professor of economics, Georgetown University.
- Robert G. Wyckham : assistant professor, department of economics and commerce, Simon Fraser University.

Administrative Appointments

- William R. Allen : chairman, department of economics, University of California, Los Angeles.
- Robert T. Averitt : acting chairman, department of economics, Smith College.
- David A. Baerncopf : head, department of accounting and business statistics, School of Business Administration, University of Oregon.
- Wilbur E. Benson : acting dean, College of Business and Public Administration, Florida Atlantic University.
- Herbert M. Bernstein : chairman, economics department, Washington and Jefferson College.
- W. Robert Brazelton : acting chairman, department of economics, University of Missouri at Kansas City.
- Brian G. Brockway : chairman, department of finance, marketing and law, College of Business and Economics, Lehigh University.
- David B. Brooks, Resources for the Future : chief, Division of Economic Analysis, U.S. Bureau of Mines.
- Dwight S. Brothers : associate director, Development Advisory Service, and lecturer, Graduate School of Business Administration, Harvard University.
- Burnham O. Campbell, University of California, Los Angeles : associate professor and chairman, department of economics, University of Hawaii, 1967-68.

Stephen B. Castle: professor and acting chairman, department of marketing, College of Business Administration, University of Akron.

Benjamin Chinitz: chairman, department of economics, Brown University.

A. J. Cordell: assistant manager of economic analysis, General Foods Corporation.

Kenneth J. Curran: acting president, Colorado College.

Albert L. Danielsen: associate professor and chairman 1967-68, department of economics, University of Georgia.

Daniel E. Diamond: assistant dean, School of Commerce, New York University.

Gordon K. Douglass: chairman, department of economics, Pomona College.

Martin T. Farris: chairman, department of economics, Arizona State University.

Bela Gold: chairman, department of economics, Case Western Reserve University.

Burton C. Hallowell, Wesleyan University: president, Tufts University.

David E. Kaun: chairman, board of studies in economics, University of California, Santa Cruz.

R. W. Kautz: head, department of economics and political science, University of Saskatchewan.

John M. Kuhlman: associate dean, Graduate School, University of Missouri.

Clayburn La Force: associate professor and vice chairman, department of economics, University of California, Los Angeles.

John E. LaTourette: chairman, department of economics, State University of New York at Binghamton.

Karl O. Mann: professor of industrial relations and chairman of graduate studies, School of Business Administration, Rider College.

Joseph W. McGuire: dean, College of Commerce and Business Administration, University of Illinois, July 1, 1968.

Carmelo Mesa-Lago: assistant director, Center for Latin American studies, and assistant professor of economics, University of Pittsburgh.

John R. Meyer: president, National Bureau of Economic Research.

William L. Miller: chairman, department of economics, University of Georgia.

John C. Murdock: dean, Graduate School, and director of research administration, University of Missouri.

Jack W. Nickson, Jr.: chairman, department of economics, School of Business Administration, Old Dominion College.

Douglass C. North: chairman, department of economics, University of Washington.

Edmund S. Phelps: chairman, department of economics, University of Pennsylvania, 1967-68.

Nikos G. Photias: associate dean, School of Business Administration, American University.

Myles E. Robinson: director, marketing program, School of Business Administration, American University.

Richard Scheuch: G. Fox and Company Professor of Economics and chairman, department of economics, Trinity College.

Warren C. Scoville: associate dean, graduate division, University of California, Los Angeles.

Martin Segal: chairman, department of economics, Dartmouth College.

Malcolm F. Severance: chairman, department of economics, University of Vermont.

Donald S. Shoup: assistant to the president, National Bureau of Economic Research.

Leonard S. Silk: editorial page editor and chairman, Board of Editors, *Business Week Magazine*.

Wendell R. Smith: dean, School of Business Administration, University of Massachusetts.

Max D. Snider: assistant dean, College of Business and Economics, Lehigh University.

Harold M. Somers, University of California, Los Angeles: co-editor, *Western Economic Journal*.

William W. Stevenson, Tennessee Valley Authority: senior economist, Acres Research and Planning Ltd., Toronto.

Robert Summers: professor of economics and chairman, graduate group of economics, University of Pennsylvania.

Benjamin J. Taylor: assistant director, Bureau of Business and Economic Research, Arizona State University.

Pedro C. M. Teichert, University of Mississippi: professor of economics and chairman of the faculties of economics, management and political science, University of West Florida.

Alice Vandermeulen, University of California, Los Angeles: co-editor, *Western Economic Journal*.

Peter N. Vukasin: dean, Harpur College, State University of New York at Binghamton.

Richard L. Wallace: chairman, department of economics, University of Missouri.

William H. Wallace, Duke University: economist and assistant vice-president, Federal Reserve Bank of Richmond, Virginia.

Willis D. Weatherford, Jr., Carleton College: president, Berea College.

James E. Wert, State University of New York at Buffalo: professor and chairman, department of finance, University of Arizona.

Robert H. Wessel: vice provost for graduate studies, University of Cincinnati.

Harry J. Wheaton: assistant dean, School of Business Administration, American University.

Harvey J. Wheeler: head, department of economics, School of Industrial Management and Textile Science, Clemson University.

Jack S. Wolf: professor of marketing and chairman of department, School of Business Administration, University of Massachusetts.

Vernon K. Zimmerman: acting dean, College of Commerce and Business Administration, University of Illinois.

Laszlo Zsolodos: chairman, department of economics, University of Delaware.

Appointments

Henry Aaron, Brookings Institution: associate professor of economics, University of Maryland.

Gerald S. Albaum: associate professor of marketing, School of Business Administration, University of Massachusetts.

Carlos W. Alborn: lecturer in economics, Rutgers—The State University.

Glen Alexandrin: lecturer in economics, University of Pennsylvania.

Harold C. Allen, Florida Atlantic University: University of South Florida.

Allan H. Anderson: visiting assistant professor of finance, University of British Columbia.

Hogeland B. Barcalow: assistant professor, College of Business and Public Administration, Florida Atlantic University.

Alton C. Barlett, State University of New York at Buffalo: associate professor, University of South Florida.

Merrill J. Bateman: associate professor of economics, Brigham Young University.

Arthur A. Bayer: assistant professor of economics, University of Vermont.

C. Russell Beaton, California State College at Fullerton: assistant professor, department of economics and commerce, Simon Fraser University.

Marion S. Beaumont: assistant professor of economics, California State College at Long Beach.

George J. Beier, University of Washington: economist, economics department, World Bank Group.

- D. M. Beights: visiting professor of administration, Florida Atlantic University.
- James A. Belasco: associate professor of business administration, State University of New York at Buffalo.
- Meyer W. Belovicz: assistant professor of finance, School of Business Administration, University of Massachusetts.
- Haskel Benishay, State University of New York at Buffalo: professor, Northwestern University.
- Charles A. Berry: assistant professor of economics, University of Cincinnati.
- Jagdish N. Bhagwati: professor of economics, Massachusetts Institute of Technology.
- Donald B. Billings: lecturer in economics, California State College at Long Beach.
- Richard Blackhurst: lecturer in economics, Rutgers—The State University.
- Charles Blackorby: assistant professor of economics, University of California, Santa Barbara.
- Thomas W. Bolland, State University of New York at Buffalo: assistant professor of business administration, Whittemore School, University of New Hampshire.
- M. E. Bond: assistant professor of economics, Arizona State University.
- John Boorman: assistant professor of economics, University of Maryland.
- F. E. Brown: visiting professor of administration, Florida Atlantic University.
- Robert E. Brown, University of Houston: assistant professor of economics, Sam Houston State College.
- Paul F. Bryant: instructor in economics, Union College.
- Lloyd J. Buckwell, Jr.: associate professor of accounting, Bowling Green State University.
- Harvey C. Bunke: professor of business economics and public policy, School of Business, Indiana University.
- Douglas R. Bunker: associate professor of business administration, State University of New York at Buffalo.
- Nicholas G. Carter, Massachusetts Institute of Technology: economist, economics department, World Bank Group.
- Herbert S. Cassel, State University of New York at Buffalo: acting associate professor, Florida State University, Cape Canaveral.
- John J. Casson, Jr.: instructor in economics, Pace College.
- M. V. Chari, State University of New York: lecturer in economics, University of Pennsylvania.
- Robert Cohen, Brooklyn College: appointment in economics, Eastern College.
- Charles L. Cole: associate professor of economics, California State College at Long Beach.
- Hughe D. Coleman: assistant professor, College of Business and Public Administration, Florida Atlantic University.
- A. Wayne Corcoran: associate professor of accounting, School of Business Administration, University of Massachusetts.
- Edward G. Corrigan: instructor in economics, Fordham University.
- Robert L. Crouch: lecturer in economics, University of California, Santa Barbara.
- J. Gerard Curzon: visiting professor of international economics, Graduate School of Business, University of Chicago.
- Addison T. Cutler: professor of economics, University of Cincinnati.
- James W. Dean: instructor in economics, Northeastern University.
- Fawzy G. Demian, University of Washington: instructor, department of economics and commerce, Simon Fraser University.
- Philip M. DeMoss: assistant professor of economics, University of Missouri at Kansas City.
- Enrique E. Dieulefait: visiting lecturer in economics, University of North Carolina.
- James W. Doane: instructor in economics, Northeastern University.

Robert W. Doede, University of Chicago: assistant professor of economics, University of Pennsylvania.

Robert Ebert: instructor in economics, Baldwin-Wallace College.

R. H. Eckhouse: adjunct professor of administration, Florida Atlantic University.

Milton T. Edelman: department of economics, Southern Illinois University.

Noel M. Edelson, RAND Corporation: lecturer in economics, University of Pennsylvania.

Douglas M. Egan, State University of New York at Buffalo: associate professor, Oregon State University.

Paul T. Ellsworth, professor emeritus of economics, University of Wisconsin: department of economics, University of California, Riverside.

Bernard D. Estafen: assistant professor of business administration, School of Business, Indiana University.

Frank Falero, Jr.: assistant professor of economics, Virginia Polytechnic Institute.

Dean G. Farrer, Fullerton Junior College: assistant professor of marketing, California State College, Long Beach.

Walter Feibes, State University of New York at Buffalo: assistant professor, University of Kentucky.

Martin S. Feldstein: assistant professor of economics, Harvard University.

Marianne Ferber: lecturer in economics, University of Illinois.

Douglas Fisher, Queens College, City University of New York: associate professor of economics, Graduate School of Public Affairs, State University of New York, Albany.

H. Nelson Flanders: visiting lecturer in finance, School of Business Administration, University of Massachusetts.

James D. Forbes: assistant professor of marketing, University of British Columbia.

Glen R. Foster: assistant professor of economics, Brigham Young University.

George B. France, University of York: instructor in economics, University of Connecticut.

Myron Frankman: assistant professor, department of economics and political science, McGill University.

William J. Frazer, Federal Reserve Bank of Chicago: visiting associate professor of economics, University of Kentucky.

Walter Galenson: professor, School of Industrial and Labor Relations, Cornell University.

Joseph C. Gallo: assistant professor of economics, University of Cincinnati.

Charles B. Garrison: assistant professor of economics, College of Business Administration, University of Tennessee.

William Gibson: acting assistant professor of economics, University of California, Los Angeles.

Philip M. Ginsberg, IBM Corporation: acting assistant professor of economics, University of Washington.

Thomas L. Glenn, Tulane University: assistant professor of economics, Franklin and Marshall College.

Elmer R. Gooding: assistant professor of economics, Arizona State University.

Robert J. Gordon: assistant professor of economics, Harvard University.

George Gostenhofer: lecturer in economics, Northeastern University.

Paul Graesser: assistant professor of economics, Northern Illinois University.

Harry Graham: assistant professor, Bureau of Labor and Management, University of Iowa.

Leslie E. Grayson, California Texas Oil Corporation: Development Advisory Service, Harvard University; advisor to the Minister of Economic Affairs, Ghana.

Stuart I. Greenbaum, Office of Comptroller of the Currency: associate professor of economics, University of Kentucky.

L. Christopher Griffiths: instructor in economics, Colorado College.

Sanford Gunn: instructor in accounting, School of Business Administration, University of Massachusetts.

John Hambor: assistant professor of economics, University of California, Santa Barbara.

Eila Hanni, Boston College: assistant professor of economics, Colorado College.

G. L. Harris: visiting professor of administration, Florida Atlantic University.

James B. Heisler: instructor in economics, Nebraska Wesleyan University.

B. J. Hersker: assistant professor of administration, Florida Atlantic University.

Harriet M. Hinck: instructor in economics, Rutgers—The State University.

Roger H. Hinderliter: lecturer in economics, Rutgers—The State University.

W. Dickerson Hogue: research associate, International Business Research Institute, and lecturer in international business administration, School of Business, Indiana University.

A. G. Holtman, Wayne State University: associate professor of economics, Florida State University.

Joseph J. Horton, Jr.: financial economist-econometrician, research division, Federal Deposit Insurance Corporation.

Allen S. Hunter: lecturer, department of economics and commerce, Simon Fraser University.

Sidney Ingerman: assistant professor, department of economics and political science, McGill University.

Tomotaka Ishimine: assistant professor of economics, California State College at Long Beach.

Donald F. Jackson, University of Texas: assistant professor of finance, Southern Methodist University.

William Jaffe, professor emeritus of economics, Northwestern University: visiting professor of economics, Harvard University.

Gerald R. Jantscher: research associate, Brookings Institution.

Jon Joyce: assistant professor of economics, Wesleyan University.

Madelyn Kafoglis: associate professor of economics, College of Business Administration, University of Tennessee.

Sigfried Karsten: assistant professor of economics, University of Wyoming.

Shou-Eng Kee, University of Georgia: associate professor, Indiana University.

Rita Keintz: lecturer in economics, Northeastern University.

Mitchell Kellman: instructor in economics, University of Pennsylvania.

Robert F. Kelly, Indiana University: associate professor of marketing, University of British Columbia.

J. D. Khazzoom: assistant professor, department of economics and political science, McGill University.

Kye C. Kim: assistant professor of economics, State University of New York, College at Brockport.

Jacky Knopp, Jr., State University of New York at Buffalo: associate professor, Canisius College.

John E. Koehler, Yale University: economics department, RAND Corporation.

Kenneth K. Kurihara, Rutgers University: distinguished professor of economic theory, Harpur College, State University of New York at Binghamton.

Jerry R. Ladman: assistant professor of economics, Arizona State University.

Arthur Laffer: research associate, Brookings Institution.

Douglas Lamont: associate professor of international business, University of Alabama.

David Larson: lecturer in economics, University of Maryland.

John Ledyard: research associate in economics, Carnegie Institute of Technology.

Joong-Koon Lee: assistant professor of economics, University of Cincinnati.

Harvey Leibenstein, University of California, Berkeley: Andelot Professor of Economics and Population, Harvard University.

Sima Lieberman: assistant professor of economics, University of Utah.

R. S. Livingstone: adjunct professor of administration, Florida Atlantic University.

Yih-Wu Liu: lecturer in economics, Southern Illinois University.

David J. Loschky: associate professor of economics, University of Missouri.

Douglas M. Lounsbury: lecturer in economics, California State College at Long Beach.

M. A. Loutfi: assistant professor, department of economics and political science, McGill University.

Richard J. Lurito: instructor in economics, Georgetown University.

Michael P. Lynch, University of Chicago: lecturer in economics, University of Pennsylvania.

Edwin Mansfield: visiting professor of economics, California Institute of Technology.

James C. T. Mao: professor of finance, University of British Columbia.

Joseph A. Martellaro: professor of economics, Northern Illinois University.

J. Barry Mason: assistant professor of marketing, University of Alabama.

John F. Masten, Southern Illinois University: East Carolina University.

Stanley Masters: assistant professor of economics, Rutgers—The State University.

Richard V. Mattessich: professor of accounting, University of British Columbia.

Laurence J. Mauer: assistant professor of economics, Northern Illinois University.

Warren Mazek, College of Wooster: assistant professor of economics, Florida State University.

Michael D. McCarthy, Southern Methodist University: assistant professor of economics, University of Pennsylvania.

Joseph W. McGuire, University of Kansas: Ford visiting research professor, Graduate School of Industrial Administration, Carnegie-Mellon University.

Joseph P. McKenna, Virginia Polytechnic Institute: professor of economics, University of Missouri at St. Louis.

Lawrence E. McKibbin: faculty lecturer in business administration, and senior researcher, International Business Research Institute, School of Business, Indiana University.

Eugene McLoone: joint appointment, lecturer, department of economics and College of Education, University of Maryland.

James W. Meehan, Jr.: assistant professor of economics, Northeastern University.

Peter V. Mini: assistant professor of economics, Northeastern University.

Michael Moore: lecturer in economics, University of Maryland.

Willard D. Morgan: assistant professor of economics, University of California, Santa Barbara.

Clarence C. Morrison: associate professor of economics, University of Georgia.

Thomas A. Morrison: associate professor of accounting, School of Business Administration, University of Massachusetts.

Collette H. Moser: lecturer in economics, Rutgers—The State University.

Max Moszer, Drexel Institute of Technology: associate professor of economics, Kent State University.

Max Mueller: lecturer in economics, University of Maryland.

Charlotte F. Muller: professor of urban studies, Graduate Center, City University of New York.

Neal B. Murphy, Federal Reserve Bank of Boston: financial economist, division of research and statistics, Federal Deposit Insurance Corporation.

Lester A. Neidell: lecturer in marketing, State University of New York at Buffalo.

Norman T. Ness, Anderson Clayton & Company : professor of economics, Pomona College, spring 1968.

Thomas E. Ness : faculty lecturer in marketing, School of Business, Indiana University.

W. E. Newkirk, Louisiana State University : associate professor of economics, Drake University.

Robert E. G. Nicol : assistant professor of accounting, University of British Columbia.

A. E. Nielsen : visiting professor of economics, Franklin College.

Charles Nisbet : assistant professor of economics, University of California, Los Angeles.

Morris Norman : assistant professor of economics, University of California, Santa Barbara.

A. L. O'Toole : visiting professor of administration, Florida Atlantic University.

Ibrahim M. Oweiss : instructor in economics, Georgetown University.

Walter P. Page : assistant professor of economics, University of Georgia.

Rollo Edward Park : economics department, RAND Corporation.

Sidney Parnes, State University of New York at Buffalo : professor, State University of New York, College at Buffalo.

Anthony H. Pascal, Economic Development Agency, Department of Commerce : economics department, RAND Corporation.

Werner H. Peterke : assistant professor of economics, Lebanon Valley College.

Llad Phillips : assistant professor of economics, University of California, Santa Barbara.

Nelson E. Pion : instructor in accounting, School of Business Administration, University of Massachusetts.

Judy Pitcher : instructor in economics, University of Kentucky.

Richard L. Porter : professor of economics, Oklahoma State University.

Calvin C. Potter : visiting professor of accounting and finance, University of British Columbia.

P. L. Preston : instructor in administration, Florida Atlantic University.

Clark R. Puckett : assistant professor of economics, State University of New York, College at Brockport.

Joaquin P. Pujol : instructor in economics, University of Pennsylvania.

David Qualls : lecturer in economics, University of Maryland.

Michael Rabbitt, University of California, Berkeley : assistant professor of economics, Franklin and Marshall College.

Ram P. Rajbahak : visiting lecturer in economics, Southern Illinois University.

Richard R. Ratcliff : professor of urban land economics, University of British Columbia.

William S. Rawson : assistant professor of economics, College of Business Administration, University of South Carolina.

Robert H. Reilly, Michigan State University : instructor in economics, University of Connecticut.

Lockwood Rianhard, Jr., State University of New York at Buffalo : associate professor, York University, Toronto.

Ronald G. Ridker : economic advisor, Agency for International Development Mission to India, New Delhi.

Fred Ritchie : lecturer in economics, DePauw University.

James E. Roberts : lecturer in economics, Southern Illinois University.

James W. Robinson : associate professor of economics, College of Business Administration, University of South Carolina.

James Rock : assistant professor, University of Utah.

Richard W. Roll : research associate in economics, Carnegie Institute of Technology.

Saul Rothman : instructor in economics, University of Pennsylvania.

John W. Rowe, Jr. : associate professor of economics, University of Oklahoma.

A. P. Ruderman, Pan American Health Organization: professor of health administration, University of Toronto.

Richard L. Ruth, Hobart and William Smith Colleges: assistant professor of economics, Marquette University.

W. T. Ryan: assistant professor of administration, Florida Atlantic University.

David K. Sandberg: instructor in economics, Brigham Young University.

Thomas J. Sargent: research associate in economics, Carnegie Institute of Technology.

Edward J. Schmidt: assistant professor of economics, Randolph-Macon College.

J. Chip Seagler: assistant professor of economics, University of Georgia.

J. C. G. Seidl: visiting professor of administration, Florida Atlantic University.

Joseph J. Seneca: lecturer in economics, Rutgers—The State University.

Merrill K. Sharp: instructor in economics, Brigham Young University.

Stanford Shedd: lecturer in economics, Southern Illinois University.

Heidemarie Sherman: instructor in economics, Whittemore School, University of New Hampshire.

A. Ross Shepherd: associate professor of economics, University of Missouri at Kansas City.

Phillip H. Siegel: department of business administration, Delaware Valley College of Science and Agriculture.

Charles J. Siegman, Swarthmore College: economist, division of international finance, Board of Governors of the Federal Reserve System.

Eugene Silberberg, Harpur College, State University of New York: assistant professor of economics, University of Washington.

Robert Silverzweig: instructor in economics, State University of New York at Buffalo.

James C. Simmons, Israel Institute of Productivity, Tel Aviv: associate professor of economics, Florida State University.

Richard H. Simpson: assistant professor of accounting, School of Business Administration, University of Massachusetts.

Arnold Simson: lecturer in economics, Syracuse University.

Charles T. Smith, State University of New York at Buffalo: assistant professor, Florida State University.

L. George Smith: professor of administration, Wichita State University.

James A. Socknat: visiting assistant professor of economics, Iowa State University.

Alan Sorkin: research associate, Brookings Institution.

Marvin N. Stark: lecturer, department of economics and commerce, Simon Fraser University.

Herbert Stein: senior staff, Brookings Institution.

Andrew Stern: assistant professor of economics, California State College at Long Beach.

Harold W. Stevenson, University of Minnesota: professor of finance, Arizona State University.

Justin D. Stolen: instructor in economics, DePauw University.

Thomas Stone: instructor in economics, University of Pennsylvania.

Myra Strober: lecturer in economics, University of Maryland.

Tsauh-An Su: assistant professor of economics, College of Business Administration, University of South Carolina.

John Swirls: associate professor of operations research, University of British Columbia.

Victor H. Tidwell, Kansas State University: associate professor of accounting, University of Iowa.

Michael K. Taussig: assistant professor of economics, Rutgers—The State University.

Richard A. Taylor: assistant professor of economics, Northern Illinois University.

John Terninko : instructor in business administration, Whittemore School, University of New Hampshire.

Thomas Thompson : acting assistant professor of economics, University of Washington.

William D. Thompson, Southern Illinois University: Central Missouri State College.

John Tilton : research associate, Brookings Institution.

Joseph L. Tryon, National Planning Association: associate professor of economics, Georgetown University.

Gordon Tullock, University of Virginia: professor of economics and political science, Rice University.

Thurai Venkataswami : instructor in economics, Northeastern University.

Meenakshisunder Venkatesan : assistant professor of marketing, School of Business Administration, University of Massachusetts.

E. Kent Vernon : instructor in economics, Brigham Young University.

A. Vicas : assistant professor, department of economics and political science, McGill University.

Annibal V. Villela, Instituto Brasileiro de Economia Fundacao Getulio Vargas: advisor, economics department, World Bank Group.

Fercy deF. Warner III, Michigan State University: instructor in economics, University of Connecticut.

Gerald Weber : research associate, Brookings Institution.

Donald C. Wellington : assistant professor of economics, University of Cincinnati.

L. C. Westbrook : assistant professor of administration, Florida Atlantic University.

Richard M. Westebbe, Ministry of Commerce, Greece: advisor, economics department, World Bank Group.

Gerald A. Weston : associate professor of economics, Arizona State University.

William E. Wetzel, Jr. : assistant professor of business administration, Whittemore School, University of New Hampshire.

Harland W. Whitmore : assistant professor of economics, University of Cincinnati.

Richard Wienecke : instructor in business administration, Geneva College.

Cyrus Wilson : assistant professor of marketing, School of Business, Indiana University.

Whatarangi Winiata : assistant professor of finance, University of British Columbia.

William H. Witherell : senior economic analyst, Standard Oil Company (New Jersey).

B. S. Wojtun : associate professor of economics, Indiana University of Pennsylvania.

Peter J. Woolley : lecturer, department of economics and commerce, Simon Fraser University.

Parker M. Worthing : assistant professor of marketing, School of Business Administration, University of Massachusetts.

Leaves for Special Appointments

Robert L. Allen, University of Oregon: Fulbright lecturer, University of Salamanca, Spain, 1966-67; Economic and Planning Advisor, Corporación de los Andes, and professor of economics, Universidad de los Andes, Mérida, Venezuela, 1967-68.

James H. Blackman, University of North Carolina: program director for economics, National Science Foundation.

George H. Borts, Brown University: research associate, Brookings Institution.

Eugene A. Brady, Iowa State University: Johns Hopkins University, Bologna Center, Italy.

John F. Burton, University of Chicago: staff labor economist, Council of Economic Advisers.

Robert P. Collier, Utah State University: visiting professor, Mildenhall Air Base, England.

John F. Crecine, University of Michigan: economics department, RAND Corporation.

David G. Davies, Duke University: Fulbright research scholar, Australian National University, Canberra.

Frank deLeeuw, Board of Governors of the Federal Reserve System: visiting professor of economics, State University of New York at Buffalo.

George Delehanty, Northwestern University: Rockefeller Foundation, research and teaching in Nairobi, Kenya.

Emile Despres, Stanford University: Rockefeller Visiting Research Professor in International Affairs, Brookings Institution.

James S. Duesenberry, Harvard University: Council of Economic Advisers, 1967-68.

Robert S. Fairbanks, Northern Illinois University: visiting associate professor, Institute of Government and Public Affairs, University of Illinois.

Geza Feketekuty, Princeton University: visiting professor of economics, Cornell University.

Ralph Gray, DePauw University: Governor's Economic Council, Arkansas.

David Green, Graduate School of Business, University of Chicago: visiting staff member, AID-Cornell University project, Pakistan.

Timothy Hallinan, Agency for International Development: economics department, RAND corporation.

Hugh G. Hansen, University of Wyoming: Engineering Research Institute, in Ceylon.

Bruce Herrick, University of California, Los Angeles: visiting professor, University of California-University of Chile Exchange Program, Santiago.

Hendrik S. Houthakker, Harvard University: Council of Economic Advisers.

Ronald Jones, University of Rochester: visiting professor, Cornell University.

Donald W. Katzner, University of Pennsylvania: Penn-Osaka Exchange, Osaka University, Japan.

Robert G. Layer, Southern Illinois University: Rockefeller Foundation, Bangkok, Thailand.

H. Gregg Lewis, University of Chicago: Centro de Investigaciones Económicas, Universidad Católica de Chile, Santiago.

Millard G. Long, Graduate School of Business, University of Chicago: economics project for Harvard University, Pakistan.

James C. Loughlin, Trinity College: Brookings Institution.

Arthur A. Mandel, San Francisco State College: visiting professor of economics, University of Hawaii.

John A. Menz, California State Office of Planning: Institute for Training and Research on Agrarian Reform, FAO, Santiago, Chile.

Walter Miklius, U.S. Department of Agriculture: visiting associate professor of economics, University of Hawaii.

M. I. Nadiri, Northwestern University: National Bureau of Economic Research.

R. E. Olley, University of Saskatchewan: department of economics, Queen's University, Toronto.

John A. Powers, University of Cincinnati: Comptroller of Currency.

J. Graham Smith, McGill University: McGill-overseas assignment, Institute for Development Studies, University of College of Nairobi, Kenya.

Vernon L. Smith, Purdue University: visiting professor of economics, Brown University.

Norman W. Taylor, Franklin and Marshall College: visiting professor of economics, Tunghai University, Taiwan.

Erik Thorbecke, Iowa State University: head, sector and market analysis section, Agency for International Development.

Jinkichi Tsukui, Seikei University: visiting associate professor of economics, University of Hawaii.

Paul L. Van Moeseke, Iowa State University: visiting professor of economics, University of Louvain.

Lewis E. Wagner, Bureau of Business and Economic Research, University of Iowa: Ford Foundation, Middle East.

Arthur Welsh, Bureau of Business and Economic Research, University of Iowa: Joint Council on Economic Education.

Richard B. Wirthlin, Brigham Young University: visiting associate professor of economics, Arizona State University.

Resignations

David G. Brown, University of North Carolina, August 31, 1967.

Gerald C. Fischer, School of Business, Indiana University.

Ralf C. Kuehnelt, State University of New York at Buffalo.

Kenneth McLennan, Rutgers—The State University, June 1967.

B. H. Schatzmann, School of Commerce and Business Administration, University of Alabama.

J. William Uhrig, Iowa State University.

Miscellaneous

Dudley F. Pegrum, emeritus professor of economics, University of California, Los Angeles, recalled to service.

AMERICAN ECONOMIC ASSOCIATION EMPLOYMENT SERVICES

NATIONAL REGISTRY FOR ECONOMISTS

The National Registry for Economists was established in January, 1966, to provide a centralized nationwide clearinghouse for economists on a year-round basis. It is located in the Chicago Professional Placement Office of the Illinois State Employment Service and is staffed by experienced placement personnel, operating under the guidance and direction of Regional and National Bureau of Employment Security Professional Placement officials, and in cooperation with the American Economic Association. It is a free service. There are no registration, referral, or placement fees. Application and order forms used in the Registry are available upon request from the: National Registry for Economists, Professional Placement Center, 208 South La Salle Street, Chicago, Illinois, 60604.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois 60201.

Vacancies

Economists, mathematicians, statisticians, operations analysts: Needed by the Research Analysis Corporation to participate in defense-related studies of resource allocation techniques and applications. Areas of interest include systems analysis of defense forces, weapon systems, and manpower and material resources, and its constituent elements of cost-effectiveness analysis and military cost analysis. A strong capability is maintained in computer-assisted model building. Staff members are assisted in acquiring a working knowledge of computer techniques if they do not already have it. Candidates should have at least a master's degree. For additional information, send résumé to Mr. John G. Burke, Supervisor of Professional Staffing, Research Analysis Corporation, McLean, Virginia 22101.

Teaching opportunities abroad: University positions, instructor to professor level, in the developing nations of Asia, Latin America, and primarily Africa. Two-year contracts. Salary dependent on level and location; transportation provided. Write to: Overseas Educational Service, 522 Fifth Avenue, New York, New York 10036. An equal opportunity employer.

Economic statistician: Under direction of chief, Research and Analysis Division, conducts research and prepares analyses of economic conditions and trends and statistical indicators relevant to the planning and development programs of the state. Requires master's degree in economics or economic statistics and four years of progressively responsible experience in economic research involving collecting, manipulating, and analyzing economic data. Salary range, \$9,840-\$12,552 per year. File applications with: State of Hawaii, Department of Planning and Economic Development, 426 Queen Street, Honolulu, Hawaii 96813.

Economist I: Under direction of chief, Research and Analysis Division, conducts research and prepares analyses and reports on macro- and microeconomic conditions relevant to the planning and development programs of the state. Requires four years of responsible professional experience involving work in economics and a master's degree in economics or business administration. Specialization in regional economics, econometrics, or statistics helpful. Salary range, \$9,840-\$12,552 per year. File applications with: State of Hawaii, Department of Planning and Economic Development, 426 Queen Street, Honolulu, Hawaii 96813.

Economics: Assistant dean, College of Business, Ph.D. or D.B.A. in business or economics required. Three to 5 years of full-time teaching experience in college or university. Administrative experience given special consideration. Limited teaching opportunity. Initial annual salary \$17,000-\$18,000. Effective date July 1, 1967. Contact: Dean Earl A. Roth, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Visiting professorship in China (Taiwan): Requirements are a graduate degree with ability to teach one or more of the following courses: business administration; electronic data processing; marketing, statistics, quantitative methods, economics and related fields. A four-month up to two-year contract can be arranged to suit individual preference. Round trip air expenses paid. Retired professors and businessmen are also welcomed. Write to: Professor Kenneth K. Fan, Utah State University, Logan, Utah 84321.

Economist: Firm of consultants in economics and statistics in Berkeley, California, is seeking an economist to help meet the demands of an expanding practice. This is a fulltime, career position with unusual potential in the largest practice of its kind in the United States. The work involves the application of econometric techniques to a broad variety of appraisal problems, including present value of life-time earning capacity of persons, future expenses of persons, future losses of businesses, and future income of property. The work requires the preparation of appraisals and reports and their presentation in court as an independent expert witness. The minimum requirement is a Ph.D. in economics, preferably labor economics, and a good working knowledge of statistics and the mathematics of finance. Starting salary depends on qualifications and experience. Send full résumé to: Philip Eden, 1303 Walnut Street, Berkeley, California 94709.

Economists: The United Nations needs economists for challenging research and operational posts at Headquarters, New York, in Geneva, Vienna, and in many of the developing countries throughout the world. These posts are in various fields of specialization (development, econometrics, fiscal policy, foreign trade, international finance, industrial management, transportation, agriculture, mining, water resources, urban planning), at all levels of responsibility and of varying duration—permanent or short term. Candidates with an M.A. or Ph.D. and experience in these fields are invited to contact for additional information: Office of International Organization Recruitment, Department of State, Washington, D.C. 20520.

Economists: Opportunity for participation in original and meaningful research in regional economic studies, industry studies, human resource analysis and state and local finance as part of a continuing state-wide planning function. Requirements include training in economics at the M.A., near Ph.D., or Ph.D. level. Positions can be filled at salary ranges of up to \$12,000, depending upon qualifications. Please send résumé to: James J. O'Donnell, Director, Maryland State Planning Department, 301 West Preston Street, Baltimore, Maryland 21201.

Economists: General Electric's Center for Advanced Studies has openings for econo-

mists who are interested in solving problems of government and industry. Preferred training and background will include experience in operations research and a Ph.D. in economics. Offices are in Santa Barbara, California, and Washington, D.C. Write to: Dr. Stephen Enke, P.O. Box QQ, Santa Barbara, California 93102.

Economist: U.S. Army Corps of Engineers in Huntington, West Virginia, requires an economist in the Planning Branch. Positions are from grade level GS-5 (\$5,331-\$6,915) to GS-9 (\$7,696-\$10,045) and are part of the federal civil service program. Basic requirements are degree in economics or business administration, appropriate experience or training, and the ability to write well. The duties to be performed are: conduct and participate in the coordination of economic studies of river basins, apply professional techniques and principles of economics in formulating water resource investments, determine the potential effects of water resource investments upon the regional and national economy, develop data sources, develop and maintain contacts with specialists in the field. Limited travel and opportunity for additional education are available. Applicants for this position should apply to: Chief, Planning Branch, Huntington District, Corps of Engineers, P.O. Box 2127, Huntington, West Virginia 25721.

Economists: With continued growth and a new M.A. program in economics, the Department of Economics, California State College at Fullerton will have openings in September, 1968, for several additional staff members at the junior level, Ph.D. or near Ph.D. Although the particular areas of specialization in economics are open, we are interested in persons with strong fundamental grounding in economic theory. In general, the Department seeks persons with a high degree of technical economic competence, dedicated to teaching, and devoted to continuing professional activity and development. The College, 8 years old and rapidly expanding, is located on the fringe of the Los Angeles metropolitan area. For further details, write (enclosing detailed résumé): Dr. Levern F. Graves, Department of Economics, California State College, 800 North State College Boulevard, Fullerton, California 92631.

Labor economists and statisticians: Attorneys have discovered that expert economists and statisticians can provide valuable assistance in determining the extent of monetary losses suffered because of wrongful injury or death. Qualifications required to engage in this highly rewarding activity include faculty status of associate professor and above, an interest in human capital analysis, and located near a major city. Our economic consulting organization has several prime markets open. Write to: D. Carl Deiter, President, Economic and Statistical Consultants, Inc., Box 17184, Tampa, Florida 33612.

Economist, chairman of department: California State College at Fullerton is seeking a qualified economist to fill the position of chairman of its Department of Economics. The position requires a Ph.D. degree from a major university and evidence of scholarly writing, leadership, and administrative ability. The approximate salary range is \$11,000 to \$17,000, depending upon experience and qualifications. The Department of Economics is in the School of Business Administration and Economics. Major interest is directed toward the bachelor and master of arts programs in economics. In addition, the department offers undergraduate service courses for various majors in the college and provides graduate courses for the master of arts in social science and the master of business administration programs. The undergraduate program of the School of Business Administration and Economics is fully accredited by the American Association of Collegiate Schools of Business. California State College is a rapidly growing, eight-year old institution, located on the fringe of Los Angeles, 25 minutes from Newport Beach, an hour from the mountains, and within easy driving distance of the University of California at Irvine. The position of department chairman under growth conditions such as the college is experiencing, provides ample opportunity for innovation, dynamic leadership, and professional development. Please address inquiries to: Paul T. Kinney, Dean, School of Business Administration and Economics, California State College at Fullerton, Fullerton, California 92631.

Economics: February or September, 1968. Ph.D. preferred; doctoral candidates with master's degree will be considered. Teaching assignment flexible, including range from elementary to advanced undergraduate courses and/or graduate courses. Rank and salary commensurate with qualifications. T.I.A.A., life insurance, major medical, other fringe benefits. Please send résumé to: Dr. Charles L. Kraemer, Chairman,

Department of Economics and Business Administration, Wagner College, Staten Island, New York 10301.

Economists and econometricians- Abt Associates, Inc., a small research firm in Cambridge, Massachusetts, desires to expand its staff of economists and econometricians. The company is young, vital and growing rapidly. The firm offers unlimited opportunities for independent and creative research in a wide range of areas, management of contracts and research studies, and contact with professionals on the staffs of numerous universities. Abt Associates is presently working with many branches of the U.S. government, private companies, foreign governments and international institutions. Present work includes economic development and planning, transportation, economics, economics of education, urban development, the industrialization of rural areas, and the design of information systems for regional development. Staff members are either social scientists or engineers and mathematicians, and many are both. Abt Associates emphasizes an interdisciplinary approach to economic and social questions and seeks creative operational answers to today's urgent problems. Highly qualified, young and dynamic Ph.D. holders or doctoral candidates are required. Salary open and depends on qualifications. Applicants should apply to: Dr. Clark C. Abt, President, Abt Associates, Inc., 55 Wheeler Street, Cambridge, Massachusetts 02138.

Dean: Dean of the College, spring, 1968. Community college in its tenth year needs replacement for retiring dean. Teaching experience at college level required. Administrative experience desired. Doctor's degree strongly preferred. The College has become the focal point of a nationally unique and significant health education complex. Communicate with: Dr. Moses S. Koch, President, Essex Community College, Baltimore, Maryland 21221. Area 3C1, Mu 6-1200.

Accounting: Two assistant or associate professorships in accounting. Rapidly expanding department located in state university in Greater Detroit Metropolitan Area. Ph.D. or D.B.A. with specialization in accounting theory. C.P.A. given special consideration. Salary \$11,000 and up. Summer teaching and consulting available. Contact: Dr. Emerson C. Erb, Jr., Head, Department of Accounting and Finance, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Finance: Assistant or associate professorship in finance and banking. Rapidly expanding department located in state university in Greater Detroit Metropolitan Area. Ph.D. or D.B.A. with specialization in capital structure, capital investment and financial theory of the firm. Salary \$11,000 and up. July, 1968. Summer teaching and consulting available. Contact: Dr. Emerson C. Erb, Jr., Head, Department of Accounting and Finance, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Business education: Instructor with ability to teach courses in office education. Must have master's degree. Man with plans for higher education preferred, but all applications are invited. Rank and salary open. July, 1968. Contact: Dr. Charles H. Duncan, Head, Department of Business Education, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Business economics: Associate professor to teach courses in managerial economics, business conditions, and related areas at the graduate level. Ph.D. or D.B.A. in economics and/or business administration. College teaching experience preferred. Salary \$11,000-\$13,000, depending on qualifications and experience. Additional stipend for teaching summer school. July, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Data processing: Assistant professor to teach undergraduate and graduate courses in data processing. Opportunity to teach at least one course in another area such as business statistics or introduction to business. Ph.D., D.E.A., or M.B.A. plus. Business experience in data processing required. Salary \$10,600-\$12,000, depending on qualifications and experience. Additional stipend for teaching summer school. July, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Insurance: Assistant or associate professor to teach undergraduate and graduate courses in insurance. Ph.D. or D.B.A. in insurance and related fields. College teaching and business experience in insurance desirable. Salary \$11,000-\$13,000, depending on qualifications and experience. Additional stipend for teaching summer school. July, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Introduction to business: Assistant professor to teach introductory course in business at freshman level. Ph.D., D.B.A., or M.B.A. plus in business administration, with college experience teaching introduction to business. Opportunity to teach at least one course in area of specialization at undergraduate or graduate level. Salary \$10,000-\$12,000, depending on qualifications and experience. Additional stipend for summer teaching. July, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Research and statistics: Associate professor to teach graduate courses in business research, qualitative analysis, and statistics, and undergraduate course in statistics. Ph.D. or D.B.A. in business administration with work in research and quantitative areas. Teaching experience at graduate level desirable. Salary \$11,000-\$13,000, depending on qualifications and experience. Additional stipend for teaching summer school. July, 1968. Contact: Dr. W. Oscar Collins, Head, Department of General Business, Eastern Michigan University, College of Business, Ypsilanti, Michigan 48197.

Management: Two assistant or associate professorships, one in industrial relations and one in management systems or production management, in rapidly expanding department. Greater Detroit Metropolitan Area. Salary \$11,000-plus for academic year. Summer teaching available. July, 1968. Contact: Dr. Claude I. Shell, Head, Department of Management, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Marketing: Assistant or associate professor. Teaching position in an expanding marketing department located in state university in the Greater Metropolitan Detroit Area. Ph.D. or D.B.A. in business or economics required. Training in behavioral sciences, international marketing and/or quantitative methods helpful. Undergraduate and graduate programs. Salary \$11,000 and up for academic year. July, 1968. Contact: Dr. Robert J. Williams, Head, Department of Marketing, College of Business, Eastern Michigan University, Ypsilanti, Michigan 48197.

Labor economists and/or statisticians: The measurement of the economic damages suffered by those who are wrongly injured or killed is a new area for the application of economic and statistical knowledge and research techniques. This field has been pioneered successfully and Associated Appraiser of Earning Capacity is now providing this service to attorneys on a nationwide basis. Those in it appraise the losses and are required to testify as an expert in court trials and must withstand hostile cross-examination. It is one of the highest-paid forms of work in the profession and it is possible to engage in it as a part-time activity. Requirements: (1) minimum status of associate professor; (2) experience in research in collective bargaining agreements and fringe benefit programs; (3) location in or near a major metropolitan area. Openings now available in some 20 cities. Waiting list open in other areas. Send résumé and list of publications to: Philip Eden, President, Associated Appraisers of Earning Capacity, 1303 Walnut Street, Berkeley, California, 94709.

Labor economists and/or statisticians: A.S.E.A. is a nonprofit professional society of economist-statisticians who appraise (1) life-time economic losses of persons, suffered as a result of wrongful death or injury or violation of legal rights; (2) economic losses of businesses resulting from a wrongful act; and (3) the future income of property and the appraisal of income property by the capitalization of income method. Examinations are open to all persons with the following minimum qualifications: Ph.D. in economics, associate professorship in economics or statistics, experience in labor economics. The examination covers the application of economic and statistical techniques and the mathematics of finance to these areas

of appraisal. Passage of these examinations helps establish qualifications and competence to do this work in general consulting practice as well as in courts of law. For details, please write to: Dr. Norman H. Leonard, Chairman, Board of Examiners, American Society of Econometric Appraisers, 328 North Liberty Street, Delaware, Ohio, 43015.

Economics: Two new positions for 1968-69 in the areas of micro-macro theory and labor-industrial economics. One appointment will be available in June, 1968, for participation in revision of televised introductory course. Appointments will be made at any level. Ph.D. or near Ph.D. required. Minimum salary level AAUP-A. Address applications to: Dr. David A. Martin, Chairman, Economics Department, State University College, Geneseo, New York, 14454.

Senior economists: CONSAD Research Corporation has immediate positions available in Pittsburgh and New York City. The work is long range and involves participation in interdisciplinary systems research and planning, applying modern analytic methods to public and private sector problems. For a personal discussion about these opportunities, please send a complete résumé to: Dr. Wilbur A. Steger, CONSAD Research Corporation, Pittsburgh, Pennsylvania, 15213.

Economics, business administration: Teaching, with excellent research opportunities. International and development economics; public finance; production management; accounting; marketing. Salary and rank dependent on qualifications. Ph.D.'s strongly preferred. Apply with résumé to: Dean Leo M. Loll, University of Alaska, College, Alaska, 99701.

Marketing: Position available in September, 1968, for Ph.D., D.B.A., or candidates for the degree. Seek persons with a high degree of technical competence, dedicated to teaching and devoted to continuing professional activity and development. Position with flexible course assignments at the undergraduate and graduate level. Growing professionally accredited state college. For further details, write, enclosing vita, to: Dr. Frank L. Roberts, Chairman, Department of Marketing, California State College at Fullerton, Fullerton, California, 92631.

Economist (micro and macro), statisticians: A nonprofit economic research organization in Washington, D.C., is seeking several professional economists and statisticians to participate in a study of regional growth and regional planning. Economic studies will focus on labor force requirements and demographic characteristics of regional populations, on problems of industrial structure and location, on issues of public finance and fiscal policy, and on projecting economic activity. Salary is commensurate with education and experience. Write to: Dr. Sidney Sonenblum, National Planning Association, 1606 New Hampshire Avenue, N.W., Washington, D.C., 20009.

Economic statistics, operations analysis, management science and/or management systems: New Department of Management Sciences needs three additional Ph.D.'s February or September, 1968. Rank and salary commensurate with qualifications. TIAA, life, disability, and major medical insurance and other fringe benefits. Please send résumé to: Dr. Sam Barone, Chairman, Department of Management Sciences, St. Louis University, School of Commerce and Finance, 3674 Lindell Boulevard, St. Louis, Missouri, 63108.

Economists: Newberry College will have two permanent positions available in September, 1968. Prefer candidates with the Ph.D. in economics. Rank and salary competitive. Send vita and references to: Professor James R. Rinehart, Department of Economics and Business Administration, Newberry College, Newberry, South Carolina, 29108.

Management: Professor who is Ph.D. in economics or D.B.A. (or candidate for either degree) sought to teach in three-course sequence in the field of management in new master of business administration program: fall, 1968, advanced management theory; spring, 1969, quantitative aspects of management; fall, 1969, the legal environment of business. Other courses will be assigned at the graduate level or in extensive undergraduate majors in accounting, business administration, or economics, in accordance with the candidate's aptitudes and preferences. Nine-

months' salary up to \$15,000 for candidate with doctoral degree and teaching experience. Please send résumé to: W. P. Carton, Acting Chairman, Department of Accounting, Business Administration, and Economics, Loyola College, Baltimore, Maryland, 21210.

Finance: Professor who is Ph.D. in economics or D.B.A. (or candidate for either degree) sought to teach in three-course sequence in the field of finance in new master of business administration program: fall, 1968, financial management; spring, 1969, financial institutions; fall, 1969, investment analysis. Other courses will be assigned at the graduate level or in extensive undergraduate majors in accounting, business administration, or economics, in accordance with the candidate's aptitudes and preferences. Nine-months' salary up to \$15,000 for candidate with doctoral degree and teaching experience. Please send résumé to: W. P. Carton, Acting Chairman, Department of Accounting, Business Administration, and Economics, Loyola College, Baltimore, Maryland, 21210.

Marketing: Professor who is Ph.D. in economics or D.B.A. (or candidate for either degree) sought to teach a three-course sequence in the field of marketing in new master of business administration program: fall, 1968, marketing management; spring, 1969, marketing research; fall, 1969, sales and advertising policies. Other courses will be assigned at the graduate level or in extensive undergraduate majors in accounting, business administration, or economics, in accordance with the candidate's aptitudes and preferences. Nine-months' salary up to \$15,000 for candidate with doctoral degree and teaching experience. Please send résumé to: W. P. Carton, Acting Chairman, Department of Accounting, Business Administration, and Economics, Loyola College, Baltimore, Maryland, 21210.

Accounting methods: Professor who is Ph.D. in economics or D.B.A. (or candidate for either degree) sought to teach in three-course sequence in the field of accounting methods in new master of business administration program: fall, 1968, managerial accounting; spring, 1969, advanced accounting theory; fall, 1969, accounting systems. Other courses will be assigned at the graduate level or in extensive undergraduate majors in accounting, business administration, or economics, in accordance with the candidate's aptitudes and preferences. Nine-months' salary up to \$15,000 for candidate with doctoral degree and teaching experience. Please send résumé to: W. P. Carton, Acting Chairman, Department of Accounting, Business Administration, and Economics, Loyola College, Baltimore, Maryland, 21210.

Accounting and business administration: September, 1968. Professor with M.B.A. and C.P.A. sought to teach principles of accounting, intermediate accounting, corporation finance (in first semester) and money and banking (in second semester). Size of undergraduate department makes it possible, however, to shift teaching assignments to fit individual capabilities. Class sections are small, generally 20 to 30 students. Our salaries are competitive with anything which is available throughout the country. Please send résumé to: W. P. Carton, Acting Chairman, Department of Accounting, Business Administration, and Economics, Loyola College, Baltimore, Maryland, 21210.

Economists: Ph.D. Three positions. One or more of the following specialties: econometrics and quantitative methods, economic history, comparative economic systems, public finance, history of economic thought, and labor economics. State university. Rank of assistant or associate professor; salary range from \$11,000 to \$15,000; generous benefits; nine-hour teaching load. Send inquiries and a complete résumé to: Robert M. Biggs, Department of Economics, College of Arts and Sciences, University of Toledo, Toledo, Ohio, 43606.

Economics: Expanding Economics Department has vacancies for the academic year beginning September, 1968, in the following areas: principles of economics, economic history, labor economics, international trade, statistics, mathematical economics. Salary and rank according to qualifications and experience. Applicants must possess strong interest in research and economic scholarship and be competent in teaching. Applications should be sent to: Dr. G. O. Rothney, Dean of Arts, Lakehead University, Port Arthur, Ontario, Canada.

Economics: The University of Alaska offers funded research opportunities for economics graduate students from other universities. We solicit especially to degree candidates shopping for dissertation topics in regional economic development, resource economics (minerals, fisheries, forestry, etc.), demography, studies of subsistence economies, state and local finance, and the economics of investment in human beings. The University can provide office or laboratory space and exceptional technical support (secretarial, computer, etc.). Work with small but energetic interdisciplinary faculty. Minimum salary for senior research assistant with master's degree or equivalent is \$900 a month. Write to: Victor Fischer, Director, Institute of Social, Economic and Government Research, University of Alaska, College, Alaska, 99701.

Finance, management, marketing: One position available at a four-year midwestern liberal arts college. Ph.D. or D.B.A. or all courses completed for doctorate required. Opportunity to become head of department. Rank and salary open. Emeriti and others with Ph.D.'s approaching retirement may apply. Write to: Father Alcuin Hemmen, President, St. Benedict's College, Atchison, Kansas, 66002.

Finance, management, quantitative methods: Nonsectarian, coeducational, accredited institution located on new suburban campus near Princeton, New Jersey; teaching positions and department chairmanships open in expanding undergraduate and graduate programs; Ph.D. or D.B.A. required; teaching load 9 hours; salary and fringe benefits competitive; appointment effective September, 1968. Apply to: Dr. D. Dertouzos, Acting Dean, School of Business Administration, Rider College, Trenton, New Jersey, 08602.

Economics: Liberal arts, coeducational college has opening for September, 1968. Introductory and advanced undergraduate courses. Ph.D. preferred; master's required. Rank and salary dependent on training and experience. Write to: Dr. Richard M. Park, Dean of the College, Franklin College, Franklin, Indiana, 46131.

Economist: To chair department in an emerging university, 70 miles from Toronto, Ontario. The position offers the challenges and rewards of developing and administering an economics program in a new provincially-supported university with the opportunities of a two-course teaching load. Ph.D. or equivalent teaching and research experience and some administrative experience required. Rank and salary commensurate with qualifications. Full professor, minimum salary \$15,000, preferred. Applications close January 31, 1968. Appointment to take effect July 1, 1968. Please forward curriculum vita and the names of three referees to: Acting Dean of Arts and Science, Brock University, St. Catharines, Ontario, Canada.

Economics and finance: An expanded program in real estate and urban development at large eastern university has vacancies in economics of land use and real estate finance and valuation. Opportunity to participate in graduate programs of interdisciplinary nature. Rank and salary open P339

Economist: September, 1968. Ph.D. preferred or doctoral candidates. Rank and salary dependent on advanced degree, business and/or teaching experience. Principles and advanced courses to be selected from money and banking, public finance, growth and development, labor economics, business and government and international economics. Twelve-hour teaching assignment. Three classes per semester; three or four courses per year. Liberal fringe benefits, including TIAA. Millikin University is coeducational with an enrollment of 1,400. It is affiliated with the United Presbyterian Church (U.S.A.). The school offers undergraduate majors in economics, accounting, marketing, and industrial engineering. Opportunities for teaching at additional compensation are present in both Evening School and Summer Session. Please send résumé and names and addresses of three references to: Dean James R. G. Olson, School of Business and Industrial Management, Millikin University, Decatur, Illinois, 62522.

Economists: Ph.D. or near (instructor to department chairmanships) and salary according to education and experience. Appointments beginning September, 1968, and a few midyear appointments, to teach all areas of economics in liberal arts colleges and universities located mainly in the Midwest and East. P340

Economics: Head of department of economics located in the School of Business Administration at state university in Texas. Ph.D. in economics. Teaching excellence and research experience desired. Six-hour teaching load. Rank open. Please submit vita. P341

Accounting and marketing: Applications are requested for a one-year appointment, effective September 1, 1968, with the possibility of appointment for a second year (1969-70), to replace a Department member on a leave of absence. The teaching load of twelve hours each semester will consist of one section of principles of marketing and three sections of elementary accounting. Applicants with the master's degree and no experience will be considered for appointment at the rank of instructor. Further graduate work and experience may qualify an applicant for appointment at the rank of assistant professor. Write to: Dr. William F. Railing, Chairman, Department of Economics and Business Administration, Gettysburg College, Gettysburg, Pennsylvania, 17325.

Economist: Ph.D. completed by September, 1968. One position as assistant or associate professor available starting fall, 1968, to teach principles, business finance, money and banking, and/or upper division course. Salary up to \$10,000. Write to: Dean Emerson O. Henke, Hankamer School of Business, Baylor University, Waco, Texas, 76706.

Economists: There are vacancies for two young economists, starting September, 1968. One position would involve teaching undergraduate and graduate courses in quantitative analysis, econometrics and statistics. Second position ideally would be held by an economist management/marketing specialist, with strong background in behavioral sciences. Ph.D. degree preferred. Salary up to \$12,500 for nine months; opportunities for summer school teaching and for participating in research projects. Present enrollment at the University is about 4,000 students and is expected to double within the next five years. Write to: Dr. Andre Simmons, Chairman, Department of Economics, Nevada Southern University, Las Vegas, Nevada, 89109.

Economists, statisticians: The state of Hawaii is organizing a central analysis group to carry out a continuing management planning program. Opportunities will exist for economists, statisticians, program planning and budgeting, and related personnel both on continuing and short-term basis. For further details write, enclosing relevant background information, to: Director, Department of Planning and Economic Development, State of Hawaii, Honolulu, Hawaii, 96813.

Economics and department chairman: New department of two faculty with two additional for September, 1968; major offered in liberal arts program with graduate offerings in master's program in social studies. Ph.D. degree required; teaching interests in statistics and quantitative methods preferred. Send résumé to: Dean, Social Sciences Division, Millersville State College, Millersville, Pennsylvania, 17751.

Economics: Southwestern state university invites applications for senior position (professor or associate professor) in areas of macro, natural resources, Latin-American economics. Applicants must have Ph.D. and a strong publication record as well as substantial experience in a Ph.D. program. Candidate for macro position should be able to cover occasionally in public finance. P342

Management: New programs leading to bachelor's and master's degrees in engineering management require professors. Ph.D. with experience preferred, but doctoral candidates and those with master's degrees will be considered. Areas of interest include finance, economics, marketing, production, quantitative methods and human relations. Excellent opportunities for growth exist. Salary range is \$9,500 to \$13,500 for nine months. Write to: B. R. Sarchet, Professor in Charge, Management, University of Missouri at Rolla, Rolla, Missouri, 65401.

Economists: Office of International Operations, Internal Revenue Service, has unique and intellectually challenging career opportunities for economists in Washington, D.C. Nature of work is primarily concerned with applying economic analysis to intercorporate activities between related corporations to ascertain whether they re-

flect competitive arm's length standards. Basic function is to examine and analyze transactions involving goods and services flowing between related corporations to determine whether the intercorporate transfer prices meet the arm's length standard. Conclusions are then drawn as to whether the corporation has clearly reflected its true taxable income. These economic analyses cover the entire spectrum of business activity and will have an important bearing on the development of tax law guidelines and precedents involving intercorporate pricing in the expanding area of international commerce. Positions are available at entrance salaries ranging from \$10,000 to \$15,000. Position and starting salary will depend upon educational background and professional experience. A representative will be available for interviewing applicants at the American Economic Association meeting in the Sheraton-Park Hotel in Washington, D.C., December 26-30. For further details, write to: Director of International Operations, Attention, Chief, Economic Advisory Branch, Internal Revenue Service, Washington, D.C., 20225.

Economists: With continued growth of the Department of Economics, California State Polytechnic College will have at least two positions open in September, 1968, at the assistant professor level. Immediate needs are for instructors interested in teaching the basic principles courses and in the business and government and public policy areas at the undergraduate level. Ph.D. preferred, but not mandatory. Especially interested in young graduate students completing degree requirements or economists with or without degree but with experience in quantitative economic analysis. For further information, write to: George Galbreath, Head, Department of Economics, California State Polytechnic College, Pomona, California, 91766.

Economics: Robert College, a private, coeducational, American-operated college offering Turkish students B.A. and M.A. programs in English has associate and assistant professorships available starting September, 1968, in economics, economic history, statistics and econometrics, business organization and administration, accounting and finance. Challenging career with interesting research possibilities. Salary \$8,000-\$10,000 per annum according to experience and qualifications. Housing allowance, health and accident insurance plans, tax benefits, travel paid. To apply send résumé before December 31, 1967, to: Recruitment Secretary, Robert College, Office of the Trustees, 548 Fifth Avenue, New York, New York, 10036.

Economics, finance: Assistant professor to professor ranks, salary range of \$9,000 to \$15,000, depending on qualifications. Considerable flexibility of assignments is possible in an expanding department offering economics and finance courses. Write to: Paul E. Fenlon, Department of Economics, University of Texas at El Paso, El Paso, Texas, 79999.

Economists: September, 1968 (also January, 1968); Ph.D. preferred. Teaching positions open in the areas of micro economic theory, macro economic theory, public finance, comparative economic systems, business cycles, statistics, and principles. Rank and salary appropriate to education and teaching experience. Write to: Dr. Marvin R. Cain, Chairman, Department of Social Science, University of Missouri at Rolla, Rolla, Missouri, 65401.

Economists: Indiana University of Pennsylvania, the state's newest university and third largest in undergraduate enrollment, has permanent position openings for the fall of 1968 for two economists, one with strength in general theory and international economics and one in general theory and public finance, also preferably business fluctuations. Each must also teach at least one section of principles regularly. Twelve-hour teaching load maximum, controlled class size. Rank of instructor, assistant or associate professor available dependent upon qualifications. Ph.D. or near-Ph.D. in economics required. Teaching excellence emphasized, research encouraged, professional growth prospects most favorable. Salary range for nine months: instructor, \$6,580-\$8,820; assistant professor, \$7,610-\$10,200; associate professor, \$9,260-\$12,400. Summer supplement normally available on prorated salary basis. Excellent fringe benefits. Write to: Dr. Francis G. McGovern, Chairman, Department of Economics, Indiana University of Pennsylvania, Indiana, Pennsylvania, 15701.

Director of graduate studies in business administration: College of Business at Northern Illinois University will employ a person with experience in directing

doctoral studies to assume responsibilities for developing the graduate program in business. M.B.A. and M.S. degrees now offered by graduate business faculty of 43 members. Additional degrees planned. Write to: Dean Robert L. Thistlethwaite, College of Business, Northern Illinois University, De Kalb, Illinois, 60115.

Mathematician: The College of Business at Northern Illinois University will employ a person with advanced academic preparation in mathematics. Qualifications: Ph.D., minimum of one graduate degree in mathematics; experience or academic degree in business or economics. Write to: Dean Robert L. Thistlethwaite, College of Business, Northern Illinois University, De Kalb, Illinois, 60115.

Economists, statisticians: The Bureau of Labor Statistics expects to have openings at all levels, GS-5 (\$5,331 a year) through GS-15 (\$17,550). Work consists of analysis and research, not only in labor statistics such as employment, unemployment, wages, fringe benefits, industrial disputes, etc., but also retail and wholesale prices, productivity, technological change, labor costs, economic growth and input-output relationships. All positions are within the federal Civil Service System. Need especially great for new Ph.D.'s and those with research or consultation experience past the doctorate. Send application to: Mr. William T. McGuigan, Personnel Officer, U.S. Department of Labor, Bureau of Labor Statistics, Room 2415, GAO Building, 441 G Street, N.W., Washington, D.C., 20212.

Transportation economists: Permanent positions in Columbus, Ohio. Salary range \$12,000-\$18,000. Senior level responsibilities include project leadership in areas of demand analysis, management problems, urban transportation planning, analysis of physical distribution requirements, and economics of transportation alternatives. Ph.D. degree preferred with research experience. Junior level of applicants should have technical bachelor's, plus master's degrees, in economics or business. Experience desirable but not necessary. Write to: Donald D. Renwick, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio, 43201.

Industrial economists: Permanent positions in Columbus, Ohio. Salary range \$12,000-\$20,000. Responsibilities include performance of economic studies related to the many problems of concern to industrial top management. Technological forecasting, market analysis, strategy planning, and operating planning and examples. Business abilities, background in science and engineering, and quantitative techniques would be used to interpret findings clearly to management in such current areas as: agriculture, chemicals, construction, electronics, foods, fuels, graphics, health, energy, packaging, paper, manufacturing, metals, minerals, transportation, and water resources. Master's or Ph.D. degree in economics, business or industrial administration, plus technical bachelor's degree in appropriate areas are desirable, with extensive training in quantitative analysis. Project leadership responsibilities to be expected as capability is demonstrated. Write to: Donald D. Renwick, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio, 43201.

Urban economist: Permanent position in Columbus, Ohio. Salary range \$15,000-\$18,000. Responsibilities include performance of research studies emphasizing the interrelationships of economic, political, and physical problems in the urban environment. Current areas include economic development, public health and education, transportation, air and water pollution, noise abatement, and housing. Project leadership responsibilities to be expected as capability is demonstrated. Ph.D. degree is preferred with cross-discipline background including economics, city planning, sociology, and political science. Ideally, five years of experience including project leadership is preferred. More extensive experience could substitute for Ph.D. degree. Write to: Donald D. Renwick, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio, 43201.

Service industry economists: Permanent positions in Columbus, Ohio. Salary range \$12,000-\$18,000. Responsibilities focus on economic research in such service industries as wholesale and retail trade, finance and banking, insurance, real estate, or agencies and associations serving them. Master's or Ph.D. degrees are preferred with some practical experience in one or more of the industries indicated. Training in quantitative analysis is desirable. Project leadership responsibilities to be expected as capability is demonstrated. Write to: Donald D. Renwick, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio, 43201.

International development economists: Permanent positions in Columbus, Ohio, with possible travel in all countries. Salary range \$13,000-\$20,000. Responsibilities focus on international economic research. General knowledge of regional economics, project planning, industrial processes or infrastructure development is desirable. Project leadership responsibilities to be expected as capability is demonstrated. Master's or Ph.D. degrees are preferred. A cross-discipline background is desirable, including economics, sociology, political science, and technical fields, with emphasis on quantitative analysis. A foreign language capability would be helpful and overseas experience is preferred. Write to: Donald D. Renwick, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio, 43201.

Quantitative economist: Permanent position in Washington, D.C., salary range \$20,000-\$24,000. Responsibilities include project leadership in quantitative cost-benefit studies of government programs in a program-budgeting context. This will include development and application of mathematical models to forecast needs, evaluate alternative programs, and allocate resources. Areas of current research include natural resources, transportation, health, military, and poverty. A Ph.D. degree in economics, emphasizing training in quantitative analysis, is desirable. Experience or training in other social sciences is an asset. Write to: Donald D. Renwick, Battelle Memorial Institute, 505 King Avenue, Columbus, Ohio, 43201.

Economics: Fall term, 1968. Ph.D. preferred; doctoral candidates with master's degree will be considered. Teaching fields to include managerial economics, finance, theory and management, senior and graduate levels; micro and macro theory at undergraduate and graduate levels and supervision of independent study at all levels. Rank and salary commensurate with background and experience. TIAA, life insurance, major medical, other fringe benefits. Please send résumé to: Dr. Donald W. Hill, Dean of the College, Rollins College, Winter Park, Florida, 32789.

Economics and business: Sizable new university, favorably located in the Vancouver metropolitan area, invites applications at all rank levels. Department has substantial M.A. and Ph.D. programs in economics in operation; graduate program in business administration imminent. Applicants must possess doctorate (near-doctorate for instructor rank). Additional specialists sought in accounting, economic development, economic history, economic theory, educational economics, finance, international business, international economics, production, organization, quantitative methods and regional economics. For some (but not all) of the senior positions administrative capabilities will be an asset. Write to: Dr. Parzival Copes, Head, Department of Economics and Commerce, Simon Fraser University, Burnaby 2, British Columbia, Canada.

Economists: Saint Mary's University will have three faculty openings available commencing with the academic year 1968-69. All three positions provide for teaching assignments at the undergraduate level. An honors program is expected to be added soon. Ph.D.'s are preferred; others may apply. One of the appointees will be invited to act as chairman of the Economics Department, having a reduced teaching load. Specialization in any area qualifies for application. Preference will be given to applicants showing a strong background in economic theory. Salary and rank depend on qualifications and experience. Contact: Dean H. G. Beazley, Commerce Faculty, Saint Mary's University, Halifax, Nova Scotia, Canada.

Economists: September, 1968. Two positions in economics. Eight-hour teaching load. The courses would be chosen from such fields as economic principles and theory, money and banking, labor economics, income and employment theory, public finance, economic cycles, mathematical analysis, economic systems. The person filling the first position would get first choice of courses. Appointment at the instructor level for a person without the doctorate, and at the assistant professor level for one with the Ph.D. Salary depends upon experience, qualifications and rank. Although an integral part of the University of Minnesota, the University of Minnesota, Morris, is a small, relatively new liberal arts college. It is located 150 miles west of the Minneapolis Campus in a rural area and has a student enrollment of 1,000. Write to: O. T. Driggs, Cochairman, Division of the Social Sciences, University of Minnesota, Morris, Morris, Minnesota, 56267.

Economist: For fall, 1968. Assistant professor. Ph.D. necessary; some teaching experience highly desirable. Normal teaching load nine hours. To teach undergraduate courses primarily in macroeconomics, public finance, and money and banking at a

senior college branch of the University of Michigan located in a suburb of Detroit thirty miles from Ann Arbor. Salary dependent on qualifications and experience. Please send résumé to: Dr. A. J. Obelsky, Department of Economics, University of Michigan Dearborn Campus, Dearborn, Michigan, 48128.

Economists: The Department of Economics, University of Calgary, will have openings for additional staff members at the assistant, associate and professional ranks for the academic year 1968-69. Requirements are a Ph.D., dedication to teaching and a strong interest in research. Salaries range from \$9,000 (minimum for assistant) to \$16,600 (minimum for full professor). Areas of specialization are open but qualifications in urban development would be an advantage. Send detailed résumé to: Dr. F. R. Anton, Head, Department of Economics, University of Calgary, Calgary, Alberta, Canada.

Economists: Rapidly growing state university (18,000 students) with 18-man Economics Department has three new permanent positions to staff with energetic individuals interested in both research and teaching. Positions can be filled at any rank and in a number of specialty fields; e.g., labor, industrial organization, and macro. All positions require Ph.D. in economics. Senior positions require a record of published research as well as interest in working with M.A. and Ph.D. oriented graduate students. Nine-month salary range \$10,000 to over \$18,000. Summer employment, when available, pays an additional two month's salary. Send résumé and references to: Jack W. Skeels, Department of Economics, Northern Illinois University, De Kalb, Illinois, 60115.

Economists: September, 1968. Appointments effective July 1, 1968. Ph.D. required. Two vacancies in the areas of micro- and macroeconomic theory, public finance, money and banking and international trade. One vacancy personnel administration and administrative theory. Atkinson College offers degree programs in the evenings. Rank and salary appropriate to qualifications and experience. Write to: Dr. G. E. Eaton, Chairman of Economics, Atkinson College, York University, 4700 Keele Street, Downsview, Toronto, Ontario, Canada.

Economists: The expanding contract research program of the Dikewood Corporation has created openings at various professional levels for researchers with a graduate degree. Knowledge of computer programming is desirable. Opportunities exist in mathematical economics, medical economics, cost-benefit analysis, regional and international development, transportation research, cost analysis, operations research, program-planning-budget analysis to work individually or as project leader. Most study projects involve multidisciplinary efforts and thus provide the chance to broaden one's perspective and experience in other academic fields. Studies are performed for military aero space, commercial, and governmental, nondefense customers. Dikewood Corporation is an organization of consulting scientists formed ten years ago in order to provide services in the fields of systems analysis, applied research, computer sciences, stock market investment advisory service, and mutual fund management. Please contact: Robert M. Carlisle, Dikewood Corporation, University Research Park, 1009 Bradbury Drive, S.E., Albuquerque, New Mexico, 87106.

Economists, junior and senior levels: Major New York City financial institution needs economists at junior and senior level. Emphasis will be in areas of (1) macroeconomic analysis and forecasting, (2) financial and money market analysis, and (3) industry and regional projections. Econometric models will be used in all areas. Original research encouraged. Training in statistics and econometrics necessary. Junior level: minimum M.A. or equivalent; senior level: Ph.D. or equivalent. Salary commensurate with training and experience plus excellent fringe benefits. Write to: Mr. Kenneth E. MacRae Personnel Officer, Bank of New York, 20 Broad Street, New York, New York.

Economist: September, 1968. Ph.D. preferred. Doctoral candidates and those with master's degree will be considered. Rank and salary depend on qualifications; liberal fringe benefits. Three classes per semester; three or four courses per year. Twelve-hour teaching assignment. Principles of economics, money and banking, public finance, economic development, and basic course in econometrics or mathematical economics. Marietta College is coeducational with an enrollment of 1,800.

The Department of Economics and Business Administration has the largest departmental enrollment and is expanding. The College is situated in a scenic town of 20,000 on the Ohio River. College representatives will be at the A.E.A.'s December meetings in Washington, D.C., to meet candidates. Please send résumé and names and addresses of three references to: Chairman, Department of Economics and Business Administration, Marietta College, Marietta, Ohio 45750.

Economists: Micro or macro. Two openings for September, 1968. Ph.D. or dissertation stage. Salary for associate \$9,875-\$12,389; assistant \$8,124-\$10,560. Appointment level depends on experience and qualifications. Assistant must have had four years of teaching experience and associate must have had six years of teaching experience. Teach principles and advanced course at college of about 4,000 enrollment where emphasis is on good teaching. College expanding toward liberal arts and business administration programs. State college on eastern seaboard convenient to Philadelphia and New York City. P343

Economists Available for Positions (Italics indicate fields of specialization)

Money and banking, international finance, economic analysis, economic policy: Man, 44, married; Ph.D. Five years of university teaching experience; 15 years of economic research in business; 2 years as government economist; modest publication record, including two books. Seeks stimulating position in teaching or research. Available at end of 1967. E1649

Economic theory, economic history, labor economics, money and banking: Man, 32, Indian; M.A., working towards Ph.D. at Calcutta University. Trained in labor welfare; 7 years of graduate and undergraduate teaching experience; several publications. Desires teaching or research position. E1658

Managerial economics, corporation finance, economic and financial analysis: Man, 33; B.S., Columbia, M.B.A., Ph.D. candidate. President of textile manufacturing company, former New York banker; several languages; experience in research for publication; teaching experience as army weapons instructor and platoon leader; tutor in economics. Desires teaching position in New York City area, part time or full time, undergraduate business and economics. Available on reasonable notice. E1659

Industrial relations and personnel management, economic planning, economic history: Man, 32, married; M.A. Assistant director of compensation service; 7 years of federal civil service; 3 years of teaching experience. Many articles in foreign field; fluency in Italian and other Romance languages; wide contacts, including business executives, labor representatives, and federal officials. Desires writing or college teaching position. E1666

Land and area development: Man; A.B., M.S., all college and professional honors. Nationally recognized; 35 years of high-level professional experience. Independent consulting economist in Washington, D.C., with own office for 25 years. Over 300 studies made in most economic fields, both in U.S. and abroad. Knowledge of Spanish, Italian; some knowledge of French and German. Desires additional assignments on contract, per diem, or retainer basis. Résumé furnished on request. E1669

Economic principles, international trade and development, business administration: Man, 44, married; European A.B., M.A. Unusual experience overseas; dynamic and resourceful; fluent in several European and oriental languages. Seeks teaching or research position. E1672

Management, business law, business organization: Man, 28, married; B.S. (industrial administration), M.B.A. (marketing), J.D. University teaching and industrial experience. Desires teaching position with increased opportunity for growth. Opportunities in connection with doctoral study of particular interest. Available in September, 1968. E1674

Microeconomics, economic development, international economics, money and banking, economic planning and public administration: Man, 27; B.A., M.A., Dr.rer.pol.

in 1967 (West Germany). Doctoral research carried out in the pure theory of international trade with special reference to development problems. Desires research, academic, or teaching position in United States or Canada. Available on short notice.

E1677

Labor economics, management, principles of economics, money and banking, statistics: Man, 53, married; Ph.D. Eighteen years of college and university teaching experience, including 3 years as department chairman; 9 years of personnel work in industry and government. Midwest only; prefers Missouri or Illinois. Available in September, 1968.

E1681

Macroeconomics, mathematical economics, comparative economic systems, Soviet economics, microeconomics: Man, 28; Ph.D. University teaching and research experience. Seeks academic position with time allowance for research or research position. Willing to go abroad. Available in June or September, 1968.

E1687

Economic problems of underdeveloped areas, international public and business finance, investment and accounting: Man, 44, Indian (Goan), married, with Western education and background; B.S.C. (Econ.) from European university, Ph.D. dissertation in underdeveloped country economics in progress, preparing for final professional accountancy examination. Twenty years of extensive experience in senior financial position in national transport undertaking in East Africa, 8 of which as officer in charge; employed since May, 1965, as auditor in U.S. government agency; business experience includes administrative as finance and accounts department head of moderate sized undertaking. Paper on population growth in underdeveloped country published. Extensive travel in Europe, Asia, and Africa. Excellent references. Seeks research, operational or administrative position with research organization, financial institution or business undertaking in U.S., Canada, or Europe. Prefers position offering creative challenge. Available in September, 1968.

E1688

Industrial organization, public finance, economic thought: Man, 45, married; Ph.D. Twenty years of college teaching experience at excellent liberal arts college; departmental chairman; extensive academic experience in course development, including honors programs; several postdoctoral grants and research experience; modest publications record. Prefers permanent position in liberal arts college in Middlewest or Rocky Mountain area.

E1691

Economics: Man, Indo-Pakistani, 33; M.A. Hons. (economic science), B.Com., B.Sc., A.C.C.S. Several years of teaching experience at various levels; chairman, department of commerce at a school. Desires teaching and/or research position at school or college level, preferably in a state with warm climate. Available any time at six-month notice

E1693

Public finance, money and banking, macroeconomics, principles: Man, 24, married, Canadian; Honours B.A. in Economics and Political Science, M.Sc. (Econ.) from London School of Economics, Ph.D. thesis under way. Two summers with government and 2 years of teaching principles course. Seeks teaching position with research possibilities. Prefers Canada. Available in 1968.

E1695

Macroeconomics, microeconomics, international trade, economics of underdeveloped countries, money and banking: Man 28, married, Canadian; M.A., Ph.D. candidacy examinations completed at major Canadian university, degree expected in November, 1968. Scholarships; 3 years as government economist; 1 year of university teaching as graduate teaching assistant. Seeks university teaching position in Canada or northern U.S. with possibilities for research. Available July-September, 1968.

E1696

Mathematical economics, econometrics, quantitative methods, micro- and macroeconomic theory, statistics, international economics, economic development and growth: Man, 34, married; M.S. (statistics), Ph.D. (economics). Graduate and undergraduate teaching and research experience. Publications. Willing to relocate. Prefers teaching position with graduate program. Also interested in research position in a leading research institution.

E1697

History of economic thought, European economy, comparative economic systems: Man, 30, single, German; Doctor in Economics. Desires university teaching position,

especially in European economic history; also able to lecture in above fields; book, articles, reviews in quality journals. Currently Oxford Research Scholar. Six years of research and teaching experience. Geographic location open. Available in September, 1968. E1699

Economic development, monetary and fiscal policy, international trade and finance, macro and micro theory: Man, 40, married; Ph.D. in economics from Yale, 1957. Desires to join university faculty with time for writing and research as well as teaching. Ten years of experience in economic policy adviser and research roles plus 2 years of full-time and considerable additional part-time teaching experience; last 5 years with Agency for International Development; currently in charge of economics branch of one of largest A.I.D. overseas missions. Available in September, 1968. E1700

International economics, economic development, economic history, economics of Asia, economic theory: Man, 36, married; B.A., M.S., Ph.D. Five years of college teaching experience; currently associate professor of economics; publications. Seeks teaching position with opportunity for research. Available in September, 1968. E1701

International economics, principles of economics, history of economic thought, public finance, money and banking, economics of transport and public utilities, labor economics, Indian, American, and Soviet economic development, commercial geography: Man, 26, Indian, holding a permanent residence visa to the U.S. valid until November 21, 1967; M.A. (economics), Distinction, LL.B. First Class, both from Nagpur University, plus 2 semesters of graduate work at Institute of International Studies, Geneva. One year of teaching experience; 3 years in Reserve Bank of India. Seeks suitable post, teaching, research, business, or other in U.S. or Canada. Available immediately. P. L. Rao, C-4, 132 Rue de Lausanne, Geneva, Switzerland.

Business statistics, probability and statistical inference, regression and correlation analysis, business and economic mathematics, managerial and business economics, financial management, corporate finance, money and banking, international economics, economic principles: Man, 49, married; Ph.D. in economics from one of the top ten eastern schools. Twelve years of diversified teaching experience at the graduate and upper levels, last 9 years as an associate professor; other industry and government experience at the senior level in economics and statistics; member of various professional and honorary organizations; fluent in Spanish; presently associate professor in a large Midwest university. Desires graduate or undergraduate teaching position in megalopolis with preference to the New York area. Will consider top employment with research organization or the United Nations in the New York area, or with a governmental agency in New York City, Philadelphia, Washington, Baltimore, or Boston. Available in September, 1968. E1702

Macro and micro theory, monetary economics, public finance, statistics: Man, 31, married; B.A., B.S., M.A., currently working on Ph.D. dissertation. Five years of college teaching experience. Research and writing experience with large commercial bank and federal government. Will consider teaching and/or research position in Southeast, including Washington, D.C. Available in January, 1968. E1703

History of thought, micro, macro, economic development: Man, 28, married; B.S. (mathematics) 1961, M.S. (economics), 1963, Ph.D. (economics), 1966, from a big ten university. Good mathematical background; thesis contains mathematical model describing the Corn-Law Controversy. Good potential for future publications. Three years of college economics teaching experience; also experience in military systems analysis. Desires university teaching and/or research position in U.S. Available in April, 1968, or June, 1968. E1704

General business, marketing, economics: Man, 31, Indian; M.B.A., Wayne State University, B.A. (Economics), Karnatak University, India. Five years of experience of which 2½ years in university research administration in U.S. and 2½ years as training officer in India. Desires research and/or teaching or administrative position with college, university, or research organization. Also interested in working for private business or international agency in the U.S. or Canada. E1705

Macro- and microeconomics, history of economic thought, economic development,

international economics, comparative economic systems: Man, 37; Ph.D. Six years of teaching experience; 3 years of economic research; publications; grants. Presently with one of the leading eastern universities. Available in September, 1968. E1706

Economic aspects of R and D, Soviet economy, economic development: Man, 34, single; B.A. plus 24 units of graduate work in economics at the University of California, Berkeley, M.A., University of Toronto, Ph.D., University of Cambridge. Seeks teaching or research position, preferably in his specialized field—economic aspects of R and D. Otherwise, in other two secondary fields. E1707

Mathematical economics, microeconomics, quantitative methods, econometrics, operations research, welfare economics: Man, 29, single, B.A. (mathematics), M.S. (economics), Ph.D. (economics), 1964, from big ten university. Three and a half years of experience in research and teaching; publications in top journals. Desires teaching position as associate professor. East or West Coast location preferred. Available in summer, 1968, and possibly earlier. E1708

Mathematical economics, statistics, econometrics, economic theory, interdisciplinary social science: Man, 34, married; Ph.D. Ford Foundation, N.S.F. and industry fellowships. Eight years of teaching experience. Desires administration and/or teaching in small or medium-size institution with teaching emphasis in United States or Canada. Available after June, 1968. E1710

Economics, public finance, taxation, law, government and business: Mature man, married; B.A., M.A. (economics), LL.B. Twelve years of full-time teaching of economics and business courses. Strong in law, economics, income and estate taxation, public finance, government and business. Bilingual, Spanish-English. Experience in government, law and business, Résumé sent upon request. Available in June or September, 1968. E1711

Labor, public finance, money and banking: Man, 45, married; Ph.D. Twelve years of teaching experience. Prefers college with accent on good teaching rather than publications. Prefers western area of country and associate professor rank. E1712

International business, comparative management, marketing, management theory: Man, 34, married; B.A. in economics, M.B.A. in marketing, Ph.D. dissertation to be completed around April, 1968. Presently teaching and doing research in a foreign country under a three-year contract with major U.S. university. Years of business experience in the U.S. and foreign countries. Several languages. Seeks teaching and/or research position. Available in fall, 1968. E1713

Economic theory, history of economic thought, mathematical economics: Man, 47, married; undergraduate mathematics degree, Ph.D. in economics, 1956. Ford Foundation fellowship in econometrics. Publications include book (in preparation), encyclopedia contribution, book reviews in leading journals. Fifteen years of college and university teaching experience, graduate and undergraduate. Seeks permanent position. Available in September, 1968. E1714

Microeconomics, history of economic thought, economic history: Man, 35; M.B.A., Harvard, Ph.D., Columbia (economics). Four years of experience in financial departments of major U.S. corporations; 4 years of experience as an instructor and assistant professor of economics. Some publications. Research interests in human organization of "the firm." Desires research, consulting, and/or teaching position located in Great Britain. Available in February or July, 1968. E1715

Public finance and fiscal policy, macroeconomic theory, economic growth, transportation economics: Man, 41, married; Ph.D. Fifteen years of teaching experience at both graduate and undergraduate levels; 10 years of experience in business consulting. Experienced in administration. Seeks dynamic academic challenge with administrative responsibilities in economic and/or business administration. Available in August, 1968. E1716

Administration: Man, 52, married; M.A., LL.M., Ph.D., economics and management. Degree from major university. Over 25 years of experience in business, administration, college teaching, and management consulting. Experience in university ad-

ministrative techniques and procedures, budgetary control and supervision, personnel supervision and management, public relations in all media, student activities supervision and administration; experienced in counseling, testing, and registrar procedures. Currently on AID contract with Ivy League university to develop the school of business and public administration at African university. Seeks dean of administration or school of business. Available in September, 1968. E1717

Principles of economics, business cycles, economic growth, Latin-American economic problems, industrial organization, marketing, human relations in industry, community development: Man, 33, married; B.A., M.A., Ph.D. Substantial publications. Two years of teaching experience in an American university; worked with international organizations. Currently employed as a consultant in a land colonization project in a Latin-American country. Desires overseas teaching, research, or consulting position. Available in January, 1968. E1718

Microeconomics, macroeconomics, money and banking, international trade, public finance, labor economics, economics of education: Man, 59, married; doctorate in economics. Twenty-five years of university teaching experience; presently full professor. Publications include five books. Wide experience in research and teaching of economic development. Seeks post in teaching college or university; also interested in research. Available in September, 1968. E1719

Regional economics, theory, international trade, money and banking, public finance, history of thought: Man, late 20's, married; M.A., all Ph.D. requirements completed except dissertation, which is under way. Teaching and research experience; author of several regional studies. Desires opportunity to combine teaching and regional research, preferably in New England. E1720

Management, business administration: Man, 23, married; M.B.A. Publications. Desires position teaching in above fields. Available for spring term, 1968. Dennis J. Moberg, 8443 Gross Point Road, Skokie, Illinois, 60076.

Economic theory (micro and macro), econometrics, economic thought, growth and development, international economics, monetary theory, money and banking, economic systems: Man, 48. Fifteen years of teaching experience at graduate and undergraduate levels; modest publication. Desires relocation in a progressive institution. E1721

Money and banking, monetary theory, macroeconomics, public finance, microeconomics, history of economic thought: Man, 28, married; Ph.D. Research and teaching experience; modest publication. Desires teaching and/or research position. Available in June or September, 1968. E1722

International economics, international trade, economic development: Man, 27; B.S., M.A. Presently teaching in a New England college; 2 years of teaching experience. Will consider teaching position in larger institution or challenging position with international organizations or governments in Latin America or the Middle East. Available in June, 1968. E1723

Econometrics, price theory, macro theory, public finance, statistics: Man, 24, married; Ph.D. expected in June or September, 1968. Some private research work. Fluency in French, Spanish. Willing to travel anywhere. E1724

Eastern European economics, comparative economic systems, international economics, business: Man, 38, married; A.B., completing M.A. (economics). Broad experience (15 years) in business administration. Seeks stimulating position. Available in September, 1968. E1725

Economic development, industrial economics, operations research, econometrics: Man, 39, Indian, now in U.S. with permanent resident status; Ph.D. (economics) from leading American university. Eleven years of research experience in government and business; modest publication record. Seeks position in research, business, or consulting in U.S. or Canada. Available in 1968. E1726

Macroeconomics, national accounting systems, money and banking, international

finance, government finance, comparative systems: Man, 46; Ph.D. Management experience; 8 years of teaching; modest publications; several fellowships and grants. Seeks challenging position. Metropolitan center or near such in East preferred.

E1727

History of economic thought, economic history, international trade and finance, economic theory, principles: Man, 36, married; Ph.D. in economics. Six years of teaching experience; currently chairman of 5-man economics department of small New England college. Desires teaching position at university with possibility of some graduate teaching. Will relocate. Available in June, 1968.

E1728

Principles, public finance, money and banking, taxation: Man, 49, single; Ph.D. Fifteen years of college and university teaching experience. Desires position beginning in January, 1968.

E1729

Economic, social, political, and international aspects of agrarian reforms: Man, 41, single, permanent resident of the U.S. since December, 1966; M.B.A., M.A. in political economy, 1951, completed course work for M.A. in mathematical economics, 1967. For 2 years conducted original research in and instructed U.S., NATO, and CENTO officers on advisory capacity on above subject at the Paramilitary Academy, U.S. Army (Europe); 12 years of experience in financial and economic analyses of national development projects (natural resources) in Europe, Middle East, and Asia, agricultural development, and farm cooperative development; 3 years of experience in socioeconomic and political aspects of insurgency; visited 24 countries in 4 continents; 4 languages. Seeks teaching and/or research position with university or research organization. Willing to teach course on above or related subjects at graduate level and in one or more of the fields of international economics, development economics, and international relations. Résumé on request. Available in February, 1968.

E1730

Monetary theory, macroeconomics, microeconomics, money and banking, international economics, principles: Man, 26, married; Ph.D. Fulbright Scholar. Excellent references. Minor publications. Two years of teaching experience in a U.S. college; now lecturer in a British university. Seeks position in a significant and stimulating university or college. Prefers northern U.S. but location of secondary importance. Available in September, 1968.

E1731

Industrial organization, macro and micro theory, public finance, economic development: Man, 29; A.B., M.A. (economics), partial completion of requirements for Ph.D. at major university. Good academic record; teaching assistant in graduate course. Research experience in business and government. Currently working as a program budget analyst in resources for legislature of major state. Interested in college teaching at small liberal arts college. Available in June, 1968.

E1732

General business, marketing, economics: Man, 38, married; B.A., M.A., Boston University. Eleven years of business experience in administration, purchasing, advertising, budgets, forecasting. Special interests in real estate and construction. Desires challenging position in economic research and possible future teaching opportunity. Prefers northeast U.S. location. Available in January, 1968.

E1733

Business and society, European economic history, economic development, history of economic thought: Man, 33; Ph.D., economics. Strong interdisciplinary interest. Seeks research position with private firm or government agency in large metropolitan area. Available in September, 1968.

E1734

Micro- and macroeconomic theory, history of economic thought, international economics, managerial economics, business conditions analysis, statistics, mathematical economics: Man, 42; Ph.D. Graduate and undergraduate teaching experience. Publications. Desires relocation.

E1735

Principles, public finance, money and banking, history of economic thought: Man, Ph.D. Sixteen years of college and university teaching experience. Research interest is in cost-productivity relationships in higher education. Publication soon. Desires relocation in institution stressing good teaching.

E1736

Economic principles, labor problems and legislation, development of economic thought: Man; Ph.D. large eastern university. Fourteen years of teaching experience; excellent references. Seeks teaching position in the New York City-New Jersey metropolitan area for September, 1968. E1737

Micro- and macroeconomics, labor, public finance, economic development, analysis of development projects, Latin-American economic problems, international trade, statistics, money and banking, agricultural economics: Man, 42, married; M.A., Ph.D. in economics. Teaching and research experience in large U.S. and leading Latin-American university. Modest publications. Desires teaching with research opportunities. Available in September, 1968. E1738

Comparative systems, economic history, history of economic thought, money and banking, macroeconomics: Man, 46, married; Ph.D. from major university; Phi Beta Kappa, Fulbright Scholar, Ford Foundation Fellow. Twelve years of university teaching. Currently visiting professor at large university. Desires professorship and/or administrative position in Far West or New England. Available in June or September, 1968. E1739

International economics, theories of economic growth, economics of developing nations, micro theory, principles of economics: Man, 38, married, naturalized Canadian; Ph.D. in economics. Eight years of teaching and research experience, government, consulting and administrative experience. Desires graduate or undergraduate teaching or research appointment. Available in June or September, 1968. E1740

Macro- and microeconomics, money and banking, public finance, econometrics, international trade: Man, 28; A.B. with honors in economics, Cornell, M.A., Ph.D. preliminary examinations passed, dissertation in progress, University of Pennsylvania. Scholarships and fellowships. In fourth year of full-time teaching and lecturing in economics. Previously research assistant at Board of Governors of the Federal Reserve System. Book reviews; newspaper articles. Desires teaching and/or research. Available in summer or fall, 1968. E1741

Economics, international trade, political science: Man, 40, foreigner; B.A. in economics and political science, M.A. in philosophy (English and French). Twelve years of government experience in foreign trade, economics, administration, and politics. Speaks several languages. Seeks suitable position in private business, research institution, or international organization. Available in 1968. E1742

Manpower problems, regional growth, micro- and macroeconomics, international trade, public finance, money: Man, 36, married; Ph.D. candidate at major eastern university, degree expected in December, 1968. Eight years of graduate and undergraduate teaching and research; industrial consulting; publications. Desires teaching position with opportunity for research. Available in July or September, 1968. E1743

Money and banking, business finance, international finance, economic theory, principles of statistics, accounting: Man, 36; B.Sc. from England, M.S., Columbia University, Ph.D. near completion at University of Pennsylvania. Seven years of teaching experience in this country, 2 years abroad, and 2 years in business. Available in fall, 1968. E1744

International economics, industrial organization, microeconomics, history of economic thought: Man, 44, married; B.A., M.A., Ph.D. from top eastern university. Presently teaching in liberal arts college on West Coast. Publications, including one book. Desires teaching position in other college or university. Available in September, 1968. E1745

East-West relations, international trade, economic studies: Man, 60, Canadian; Ph.D., Harvard University. Some teaching and many years of government experience in international trade, commodity arrangements, and related intergovernmental organization. Expert on East and West relations with several years of experience in eastern Europe. Would be interested in organizing or joining a center of East

European studies, including graduate seminars and faculty coordinator or in related business consulting activities. Chicago area preferred. Available in April, 1968.

E1746

Micro and macro theory, growth and development, comparative systems, business cycles, international economics, money and banking: Man, 35, married, Indian citizen and U.S. permanent resident; B.A. (commerce), M.A., Ph.D. Presently associate professor; 8 years of university teaching experience at the undergraduate and graduate levels; fellowships; modest publication record; administrative experience. Desires relocation to a teaching and/or research position; overseas appointments not ruled out. Available in September, 1968.

E1747

Economic theory, industrial relations, organization theory, labor economics, development economics: Man, 36, married; M.A., Ph.D., 1964 from major U.S. university. Fulbright Scholar. Publications. Eleven years of teaching and research experience. Currently working on econometric models in a Canadian government department. Desires teaching, research, or administrative position. Available in summer, 1968, and possibly earlier.

E1748

Money and banking, monetary theory, financial institutions and markets, quantitative analysis, economic theory: Man, 40, married; Ph.D. from major university. Thirteen years of experience in teaching, research, and administrative positions in universities and business. Excellent teacher; good record of research and publication; active nationally as consultant in field of specialization. Desires to leave position in university administration to engage full time in teaching, research, and consultation. Seeks institution in warm climate. Requirements are rank and salary commensurate with experience and capacity, TIAA-CREF, liberal rules governing income from consulting.

E1749

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